INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE

IFPRI

IFPRI Discussion Paper 02235

January 2024

Double-Booked: Effects of Overlap between School and Farming Calendars on Education and Child Labor

James Allen IV

Poverty, Gender, and Inclusion Unit

INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE

The International Food Policy Research Institute (IFPRI), a CGIAR Research Center established in 1975, provides research-based policy solutions to sustainably reduce poverty and end hunger and malnutrition. IFPRI's strategic research aims to foster a climate-resilient and sustainable food supply; promote healthy diets and nutrition for all; build inclusive and efficient markets, trade systems, and food industries; transform agricultural and rural economies; and strengthen institutions and governance. Gender is integrated in all the Institute's work. Partnerships, communications, capacity strengthening, and data and knowledge management are essential components to translate IFPRI's research from action to impact. The Institute's regional and country programs play a critical role in responding to demand for food policy research and in delivering holistic support for country-led development. IFPRI collaborates with partners around the world.

AUTHORS

James Allen IV (j.allen@cgiar.org) is an Associate Research Fellow in the Poverty, Gender, and Inclusion Unit of the International Food Policy Research Institute (IFPRI), Washington, DC.

Notices

ABSTRACT

Overlap between school and farming calendars—pervasive in agrarian settings—constrains children's time for both activities, potentially forcing trade-offs between schooling and child labor. Using shift-share estimation, I study an exogenous shift to overlap between school and crop calendars in Malawi, weighted and aggregated by communities' pre-policy crop shares, matched to panel data on school-aged children. From pre- to post-policy, a five-day (i.e., one school-week) increase in overlap during peak farming periods decreases children's school advancement by 0.14 grades—one lost grade for every seven children—while only resulting in 3.9 percent fewer children working on the household-farm. Policy simulations show how adapting the school calendar to minimize overlap with peak farming periods can be an effective strategy to increase school participation.

Keywords: Education, Child Labor, Household Farms, Time Allocation, Sub-Saharan Africa, Malawi

ACKNOWLEDGMENTS

I appreciate feedback from my dissertation committee Dean Yang (chair), Lauren Falcao Bergquist, David Lam and Tanya Rosenblat, and from others including Achyuta Adhvaryu, Carolina Alban Conto, Harold Alderman, Kate Ambler, Yuehao Bai, Natalie Bau, Hoyt Bleakley, Jeff Bloem, Brian Dillon, Esther Duflo, Jon Denton-Schneider, Dan Gilligan, Sara Heller, John Hoddinott, Maximilian Huppertz, Naureen Karachiwalla, Katrina Kosec, Jessica Leight, Isaac Mbiti, Russell Morton, Emir Murathanoglu, Almedina Music, Agnes Quisumbing, Emma Riley, Shalini Roy, Bryce Stein-berg, Rebecca Thornton, Carly Trachtman, Basit Zafar and participants at the University of Michigan's EDS, H2D2, and Ford School, the University of Cape Town's SALDRU, Midwestern Development Day, Université Clermont Auvergne's CERDI, Baylor, Clark, NEUDC, PacDev, MWIEDC, and IFPRI. I thank Danielle-Andree Atangana, Noelle Seward and Laston Manja for amazing research assistance. This research was supported by the National Institute on Aging of the National Institutes of Health (award number T32AG000221) and small grants administered by the University of Michigan's Population Studies Center (PSC), the CGIAR Initiative on Gender Equality, and the CGIAR Initiative on Fragility, Conflict and Migration.

1 Introduction

Understanding how households allocate their children's time between schooling and child labor has vital implications for economic development. Time in school is an input for educational attainment, which is linked to higher earnings in adulthood (Duflo 2001; Psacharopoulos and Patrinos 2018), better health outcomes for individuals and their children (World Bank 2017), and is widely recognized as a key driver of economic growth (Mankiw, Romer, and Weil 1992). Child labor increases household income and consumption in the short run but often competes with time in school (Edmonds 2007; UIS and UNICEF 2015). Thus these household allocations can heavily impact a child's and even a country's future economic well-being.

In sub-Saharan Africa (SSA)—which has the lowest regional rate of educational attainment (World Bank 2017) but the highest rate of child labor (UIS and UNICEF 2015)—the trade-off between schooling and child labor is perhaps most salient when the school calendar overlaps with peak farming periods. Children in agrarian areas commonly contribute to household agricultural production,[1] and SSA school calendars are often not adapted to seasonal farm labor demand.[2] In theory, greater overlap between the school and farming calendars should reduce time allocations to school and also to household-farm work by further constraining a child's stock of time available for both activities. Indeed, across SSA, countries with a greater percentage of overlapping days in their school and farming calendars also have reduced rates of primary school advancement.[3] However, as such evidence is cross-sectional and non-causal, it motivates a more rigorous assessment of the following research question: how does overlap in the school and farming calendars affect children's schooling and child labor outcomes?

In this paper, I use a novel shift-share design to study an exogenous *shift* to overlap between school and crop calendars in Malawi caused by a nationwide four-month shift to the school calendar between 2009 and 2011, which I match to children's schooling and labor outcomes in 2009 (pre-policy) and four years later in 2013 by their communities' pre-policy crop *shares* using Malawi's Integrated Household Panel Survey (IHPS). First, I estimate the crop-level "shift" (or change) in the number of days during which both school is scheduled and a crop is being sown or harvested (times of peak farm labor demand) due to the school-calendar-change policy. Following the shift-share framework put forth by Borusyak, Hull, and Jaravel (2022), the identification assumption is that these crop-level overlap shocks are as-good-as-randomly assigned, which is supported by a balance test of pre-policy characteristics that substantiate shock orthogonality, the non-agricultural rationale for implementing the policy (i.e., to better align the school calendar with government, university and neighboring countries' calendars), and the fact that it inadvertently *increased* overlap

1. Most of the workforce in SSA is employed in agriculture (World Bank 2021), and family work accounts for between 40–80% of all out-of-school child laborers (UIS and UNICEF 2015), though the UNICEF definition excludes children working less than 28 hours in the past week.

2. The claim is supported by policymakers currently working on SSA school calendar reform, including at UNESCO International Institute for Educational Planning and the World Bank (UNESCO 2024; Oviedo and Mulangu 2024).

3. In Section 6.4, I document a significant negative correlation between overlapping school and farming calendars and primary school survival rates (i.e., the rate of students reaching a later grade) across SSA. Specifically, Figure 4 shows that for every additional percentage point increase in the fraction of school and sowing/harvest days that overlap, a country's rate of students reaching the fifth grade (i.e, "survival rate", on the vertical axis) is 2.39 percentage points lower, on average.

between school and most crop calendars. Then, the exogenous crop-level "shifts" are weighted by community-level crop "shares" and are summed across the 115 crops in the sample to construct a community-level shift-share measure of overlap between school and farming calendars. This construction is analogous to Bartik (1991) and Autor, Dorn, and Hanson (2013), which estimate local-level shift-share measures as the weighted average of plausibly exogenous industry-level shocks with respect to shares of local industry employment. As the source of exogeneity comes from the "shift" and not the "shares", it is distinct from shift-share strategies that rely on exogeneity in exposure shares (Goldsmith-Pinkham, Sorkin, and Swift 2020) or a difference-in-difference strategy that might seek to use variation in cropping patterns to compare differences before and after the policy, neither of which was pursued ex ante as cropping decisions are likely endogenous to household characteristics and thus there is no strong argument for parallel pre-trends in schooling and child labor.

As an example, I describe how crop-level overlap is calculated for maize and tobacco—the first and third most commonly produced crops, respectively, among households in the pre-policy study sample. Consider that the policy changed Malawi's school calendar's start date from January to September, moving the end-of-year break from November-December to July-August. In Malawi, most maize is sown from mid-November through December and harvested in May and June, overlapping with school for 51 days pre-policy and 65 days post-policy, resulting in a "shift" of 14 days in its overlap with the school calendar. Alternatively, most tobacco is sown only in December and harvested in February and March, resulting in a overlap "shift" of only 9 days.[4] The identification assumption is that these "shifts" are as-good-as-randomly assigned across crops—that is, in another imagined permutation of the post-policy school calendar, the roles could be reversed with tobacco experiencing a greater overlap shift than maize. Conversely, identification does *not* come from comparing communities that grow relatively more maize to those that grow relatively more tobacco, as such cropping decisions are likely endogenous to children's school and labor decisions; rather, following Borusyak, Hull, and Jaravel (2022), pre-policy crop shares simply adjust for the fact that some communities are more exposed to maize-specific shifts and others to tobacco-specific shifts by weighting the crop-level shifts accordingly for each community. Moreover, identification does *not* come from comparing communities with different levels of farming activity, as I control for each community's concentration of on-farm labor.

The empirical analysis uses panel data that focuses on the sample of children of primary-school age (6-13) in the pre-policy year, who are fully exposed to the shock over the four-year study period. My primary outcomes are highest completed grade level and an indicator for working on the household farm during sowing and harvest periods. The IHPS data confirm that Malawi is a setting in which both schooling and farm work are common activities: in the year prior to the school calendar shift, 81% of children aged 6–13 years in my sample were enrolled in school, and 86% lived in a farming household. To estimate effects, I regress post-policy outcomes on shift-share overlap controlling for

4. Appendix B presents these descriptive statistics on the most common crops in the study sample. Crops calendars are defined by crop type, elevation (high, medium, low) and season (rainy, dry, or permanent). Of the 12 most commonly produced crops in the sample, the policy-induced "shifts" range from 0–21 days.

pre-policy outcome values (making an ANCOVA specification), the community share of on-farm labor, among other controls. To assuage recent concerns about over-rejection in shift-share estimation, I perform a randomization inference procedure following Borusyak and Hull (2021) that perturbs the plausibly exogenous shock to overlap between school and crop calendars, while holding fixed the endogenous community crop shares.[5]

First, I find that increases in overlap between the school and farming calendars lead to significant decreases to changes in both schooling and household-farm labor over the four-year period.[6] A five-day (i.e., one school week) increase in school calendar overlap during peak farming periods leads to a significant reduction in school advancement by 0.14 grades—a sizable decrease that is equivalent to one lost grade for every seven children. I test for effects on time allocation in school using the best available data and find overlap does not have significant impacts on initial school enrollment, current enrollment, or extended absence (missing two or more consecutive weeks of school); however, I estimate an almost significant negative effect on grade advancement for the sub-sample enrolled in school both pre- and post-policy (RI p-value of 0.132), suggesting that overlap likely affects school participation at the *intensive* margin—for example, affecting daily attendance or time spent studying after school. At the same time, while the share of children working on the household farm went from 25% to 47% over the four-year period, a five-day increase in school calendar overlap during peak farming periods decreases this trend by 3.9 percentage points. However, testing on children's total hours worked annually finds null effects, suggesting that overlap likely affects household-farm work at the *extensive* margin; moreover, the estimate's upper 95% confidence limit only translates into a reduction of about one day of farm work annually. Taken together, the results suggest that households appear to make much larger reductions to their children's time in school relative to time on the household farm, despite both allocations facing the same constraint.

Second, I test for heterogeneity by a child's age, sex and household wealth, on whether households make additional adjustments to farm production or other forms of child labor, and for long-run impacts. For heterogeneity by individual characteristics, I find that boys and younger children may be somewhat more affected by overlap between the school and farming calendars. For household wealth, point estimates suggest that the poorest tercile of households make larger reductions in schooling, the middle tercile make larger reductions to household farming, and the wealthiest tercile is not significantly affected by overlap, consistent with the idea that poorest households rely most child labor, middle households are more likely to prioritize schooling over child labor, while many wealthy households are simply not constrained by overlap. For household adjustments, I find the overlap leads to increased spending on hired labor and seeds—likely as a substitute for child labor that, if anything, should attenuate the main effects—but not enough

5. The Borusyak, Hull, and Jaravel (2022) setup "nests shift-share reduced-form regressions". Further, I pursued Borusyak and Hull (2021)'s randomization inference procedure because it remains valid despite a high concentration of crop shares in the data (for rainy-season maize) that could hinder asymptotic approximation, though I show in Appendix E.2 that my results are also robust to other inference procedures including the Borusyak, Hull, and Jaravel (2022)'s share-weighted shock-level regression.

6. Findings presented in this paragraph are calculated by multiplying coefficients in Table 2 by 0.61, the standard deviation increase in shift-share overlap from a five-day (i.e. one school week) increase in school calendar overlap during the rainy-season sowing and harvest in the average sample community.

to significantly affect total farm costs or profits, nor does it affect children's time allocation to other forms of child labor. Finally, I look for long-run impacts of overlap on schooling and household-farm in the 2016 and 2019 IHPS and, despite a smaller sample size, find suggestive evidence that overlap has persistent negative effects on grade completion, including among the cohort of new students ages 0–5 pre-policy whose schooling is fully exposed to the new overlapping calendar.

Third, to determine what the ideal school calendar might look like, I run a policy simulation that approximates counterfactual effects of other potential school calendars.[7] The simulation shows that the pre-policy school calendar had among the least overlap of any potential school calendar and that the post-policy school calendar increased shift-share overlap by a substantial 1.8 standard deviations. This is because the pre-policy school calendar aligned its end-of-year break during the labor-intensive sowing period, minimizing overlap during this period of high farm labor demand. Consistent with this, I also estimate that sowing-period overlap has a larger negative point estimate on grade advancement than harvest-period overlap, though the difference is not statistically significant. Thus, the simulations suggest that reverting to the nationwide pre-policy school calendar would provide significant schooling gains for the average primary-school-aged child. Alternatively, because crop bundles—and hence farming calendars—vary by community, I find that overlap falls further when communities adopt their own overlap-minimizing school calendar rather than the one calendar that minimizes average overlap across all communities suggesting potential gains from decentralizing or locally adapting the school calendar.

This paper makes several contributions to the literature, most notably on trade-offs between schooling investments and child labor. Prior work has found that positive shocks to wage returns from child labor reduce schooling (Atkin 2016; Santos 2014; Shah and Steinberg 2017), though these effects can sometimes be conflated with a countering "income effect" driven by correlated changes in adult wages (Bai and Wang 2020) and household income (Beegle, Dehejia, and Gatti 2006). Meanwhile, other studies have analyzed how positive shocks to expected or actual returns of schooling lead to increases in schooling, but either did not examine impacts on child labor (Nguyen 2008; Jensen 2010) or found effects on child labor that were less well identified (Ravallion and Wodon 2000), inconsistent across interventions (Kazianga, De Walque, and Alderman 2012), negative but temporary (Edmonds and Shrestha 2014), or heterogeneous based on parent preferences (Jensen and Miller 2017).[8] Relative to these past studies, this paper contributes a novel and direct test of the existence and magnitude of this trade-off by analyzing a negative shock that constrains the total time available for both school and farm work, rather than analyzing a shock to the nominal returns of one of these activities.[9] Doing so results in clear robust evidence of a trade-off between allocations to schooling

7. The simulation maintains the same structure as the original post-policy school year but starts school on Monday in a different week in the year, making 52 potential school calendars.

8. Further, a related literature looks at the differential returns of improved healthcare access (Adhvaryu and Nyshadham 2012) or early-life investments (Bau et al. 2023) on schooling and child labor.

9. In this way, overlap is theoretically unique from interventions that change either the nominal marginal benefit or marginal cost of schooling, including its opportunity cost, as rather than change the nominal returns to school or labor for a given time period, overlap instead determines the number of time periods in the calendar year during which these returns are competing in the household problem.

and child labor on the household farm in this low-income agrarian setting. Further, my findings that reductions to time in school are likely larger than those on the household farm are consistent with Ravallion and Wodon (2000), and that younger boys face stronger negative schooling effects is consistent with other evidence from India (Shah and Steinberg 2017; Bai and Wang 2020) but not SSA (Kazianga, De Walque, and Alderman 2012).

This paper engages in some speculative extension of its primary results to contribute estimates on the returns to education (Psacharopoulos and Patrinos 2018) and how they are typically underestimated (Jensen 2010; Nguyen 2008). Specifically, I relate my estimates to others from Malawi: Montenegro and Patrinos (2014)'s "Mincerian" estimates of 9.8% for the average rate of return for another year of schooling in 2010 and Dizon-Ross (2019)'s 2012 estimates of 3.2% of Malawian households' perceived returns to schooling. First, I use adult wage data to replicate Montenegro and Patrinos (2014)'s "Mincerian" estimates and make a back-of-the-envelope calculation to back out that a five-day increase in overlap during peak production can reduce a child's present discounted value of lifetime income by approximately $156 in 2009 USD.[10] Second, using assumptions from the conceptual framework, I surmise from the empirical results that household's equate 0.14 lost grades of schooling to, at most, one lost day of household-farm work annually over the three post-policy years. Then, using wage data under-15 farm labor and additional back-of-the-envelope calculations, I estimate that the average household's upper-bound estimate for the perceived value of a completed grade of school is about $50 in 2009 USD. The overall result is quite consistent with the literature: households' perceived value of a completed year of schooling is only about one-third of its "Mincerian"-estimated potential present-discounted contribution to the child's lifetime income.

This paper also makes a general contribution to the literature on the economics of time by modeling and demonstrating how additional time constraints can further limit time allocations to certain activities. Since Becker (1962)'s seminal work on household time allocation, household models typically constrain time at some fixed total time T without other restrictions. However, some activities may only be available for some time less than T—for example, the sunset may determine time available for sleep (Gibson and Shrader 2018) or, as in this paper, school calendars dictate the time available for school. Alternatively, two mutually exclusive activities may only be available at the same time—for example, when religious festivals occur during the maize planting and harvest (Montero and Yang 2022) or, as in this paper, when school is scheduled during the typical farming season. Within development economics, the idea of additional time constraints has two important extensions. First, poor households suffer from "time scarcity" in addition to a multitude of other previously studied constraints affecting the household budget—e.g., household income, savings, credit, insurance, etc.—and this paper offers evidence that imposing additional time constraints on poor households can indeed have negative consequences. Second, "time scarcity" in agrarian settings is *seasonal* based on how seasonal rainfall affects farm labor demand. So just as the farm calendar drives seasonal variation in liquidity

10. A 9.8% return on the sample-average annual income for working-age adults with any schooling of $444 in 2009 USD is equal to $44. I then approximate its present discounted value as an annuity assuming interest rate $r = 0.03$ and $n = 48$ time periods that represent working-age years.

constraints (Basu and Wong 2015; Burke, Bergquist, and Miguel 2019; Fink, Jack, and Masiye 2020) and labor market opportunities (de Janvry, Duquennois, and Sadoulet 2022), this paper shows how seasonal variation in time constraints can be managed to improve time allocations to other important activities like schooling.

Finally, this paper contributes to an active area of policy debate by suggesting that the school calendar itself is a feasible policy lever to increase time in schooling and educational attainment. Aas many SSA school calendars originated during the colonial period, this recommendation generally relates to the literature on the role of colonial institutions on human capital and economic growth (Acemoglu, Johnson, and Robinson 2001; Glaeser et al. 2004; Bolt and Bezemer 2009). More specifically, the results validate at least 40 years of descriptive evidence (e.g., Schiefelbein 1987; Admassie 2003) and recommendations from international educational development practitioners (e.g., Bustillo 1989; Kadzamira and Rose 2003) about the benefits of adapting the school calendar around agricultural labor demand and, moreover, affirm that such actions would likely do more to increase schooling than household farm work. Also, the results build on evidence that overlapping end-of-year examinations with the harvest has detrimental effects (Ito and Shonchoy 2020) by providing new evidence that overlap during exam periods has a significant positive effect on household-farm work and that negative schooling effects can persist across peak farming periods regardless of timing within the school year.[11] Extrapolating from my causal estimates, I project that reverting to the nationwide pre-policy school calendar could translate into an additional 2.05 million additional years of schooling in Malawi.[12] Then, the intervention would be in the 90th percentile of interventions analyzed in Evans and Yuan (2019) if school calendar reform in Malawi cost \$3.0 million or less to implement.[13] Thus, these findings can inform policymakers currently working on SSA school calendar reform, including UNESCO International Institute for Educational Planning and the World Bank (UNESCO 2024; Oviedo and Mulangu 2024), on how they can increase school participation by adapting the school calendar to accommodate seasonal farm labor demand.

This paper proceeds as follows. Section 2 presents a conceptual framework that conceptualizes overlap as an added time constraint in a household model. Section 3 describes the setting, Malawi's school calendar change, and the data. Section 4 details the estimation strategy, including construction of the shift-share measure, the randomization inference technique, and balance tests. Section 5 presents primary and secondary analyses. Section 6 describes insights from simulating alternative school calendars and comparing school calendar reform to other types of educational interventions. Additionally, Section 6.4 presents a non-causal cross-sectional analysis which finds that, across sub-Saharan Africa, countries with a greater percentage of overlapping days in their school and farming calendars also have

11. Additionally, school calendars can affect school participation by changing the length of the school year (Watkins 2000) and making it easier for households to finance school fees (Dillon 2021), and can also mediate how child labor is affected by agricultural productivity shocks (Merfeld 2023).

12. The calculation is: the effect of a one standard deviation increase in shift-share overlap from Table 2 column (1) of -0.232 grades · -1.8 standard deviation change in shift-share overlap from reversing the policy · 4.9 million primary school students in the 2021/22 school year (Government of Malawi 2022).

13. Development practitioners actively advocating for school calendar reforms believe these costs estimates are reasonable (Alban Conto 2024; Music 2024) and calculating the true implementation costs in practice is an ambition of future research.

lower primary school survival rates, highlighting the potential relevance of these findings in the region and beyond. Section 7 concludes.

2 Conceptual Framework

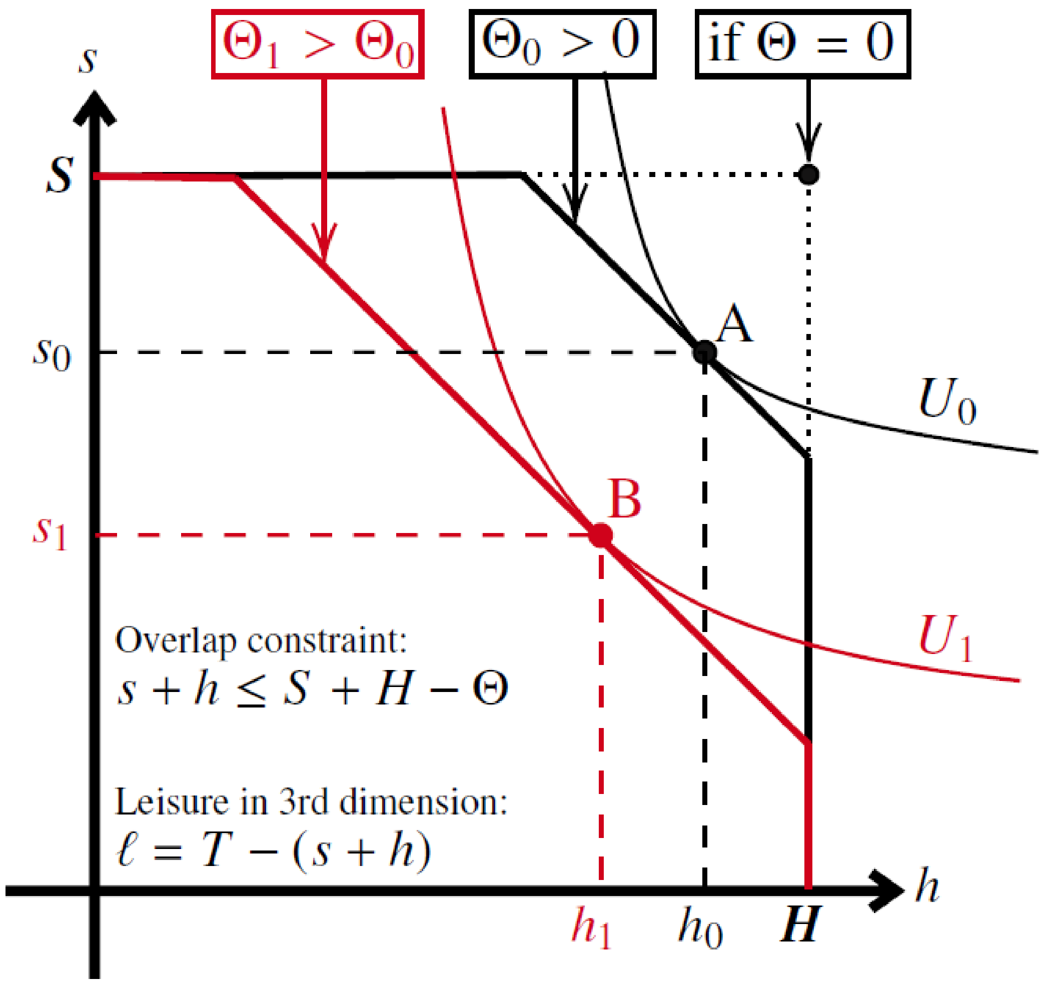
I model the school and farming calendars and the overlap between them as additional constraints on time that limit the amount of available time that a child can spend on either school or farm work. In this section, I present a general overview and graphical representation of the model, which serve as a framework to the study. In Appendix A, I detail a simple household model and under what conditions an increase in overlap (holding fixed the length of each calendar) could be expected to decrease allocations to both time in school and farming.

Suppose a child has a total time endowment T which is spent on schooling s, farm work h, and leisure ℓ such that $s+h+\ell=T$. The school calendar consists of $S \in (0,T)$ and the farming calendar consists of $H \in (0,T)$. There is also an overlap between the school calendar and the farming calendar represented by Θ: time at which school is scheduled during the farming season. When school and the farming season overlap, a child can either attend school or farm, but not both. Therefore, an "overlap constraint" limits the amount of available time that a child can spend on either school or farm work, as follows: $s+h \leq S+H-\Theta$, which I call the "overlap constraint". In words, a child's joint time allocation to schooling and farm work is constrained by time in the school calendars plus time in the farming calendar *minus* time in which the school and farming calendars overlap.

Figure 1 summarizes the main takeaways of the model. The figure depicts the time allocation trade-off as a budget constraint diagram, where the area from the origin to the frontier represents a child's time allocation opportunity set in household-farm work h and schooling s given the overlap constraint $s+h \leq S+H-\Theta$, while leisure $\ell = T-(s+h)$ represents an unseen third dimension. On the horizontal axis, the school-farming time frontier begins at H, the total number of days in the farming calendar, and continues linearly upwards. On the vertical axis, the school-farming time frontier begins at S, the total number of days in the school calendar, and continues extends horizontally rightwards. If there is no overlap in the school and farming calendars (i.e., $\Theta = 0$), then the time opportunity set extends to the point (H,S). However, when overlap in the calendars exists—e.g., at Θ_0—an "overlap line" with a slope of -1 cuts into the opportunity set, limiting possible allocations of schooling and farm work (and leisure). Further, when overlap increases from Θ_0 to Θ_1, the "overlap line" draws closer to the origin.

Household preferences for s and h (and ℓ) are represented by the convex indifference curves U_0 and U_1. If both s and h provide positive diminishing returns to marginal utility, households have strictly convex preferences for s and h characterized by a diminishing marginal rate of substitution between the two inputs. In Figure 1, households choose allocations on the school-farming time frontier where the overlap constraint is binding—i.e., $s+h=S+H-\Theta$ (Assumption 1 in Appendix A). Additionally, households choose allocations on the interior of the "overlap line" where

Figure 1: **A Child's Time Constraint in the Household Problem**



Notes: Figure depicts how the time constraint for schooling and farm work $s+h \leq S+H-\Theta$ enters into the household problem, where a household's allocation of a child's time in schooling s and household-farm work h is constrained by the maximum number of school days S and suitable farming days H minus how the number of days that the school and farming calendars overlap Θ. At $\Theta = 0$, the time allocation opportunity set extends to the point (H,S). At Θ_0 and Θ_1, an "overlap line" with a slope of -1 cuts into the frontier. Household preferences define indifference curves U_0 and U_1. As overlap increases from Θ_0 to Θ_1, the "overlap line" draws closer to the origin, potentially forcing reductions of both s and h that reduce household utility.

the marginal utility of time in school is equal to the marginal utility of time spent farming, and not "at the kinks" where either s is bound by S or h is bound by H (Assumption 2 in Appendix A).[14]

When overlap increases from Θ_0 to Θ_1, households must reallocate h and s to the newly constrained time frontier. In Figure 1, the household reallocates from point A to point B, leading to an unambiguous decrease in utility from U_0 to U_1. Preferences are also such that households will choose to reduce allocations to both s and h when overlap increases, though the magnitudes of the reductions will depend on the shape of the marginal utility curves.

3 Data

To test the model's predictions, I require data to represent children's time in school s and on the household farm h that are plausibly affected by an exogenous shock to overlap between the school and farming calendars. I look in Malawi, where a four-month shift to the school calendar between 2009 and 2011 coincided with the first wave of the World Bank's Integrated Household Panel Survey (IHPS). In addition to outcome data, I also require measures of the overlap between the school calendar and crop-specific calendars on sowing and harvest periods and community exposure to the school calendar policy change, which I construct using data on community crop shares.

In this section, I first describe this study's setting and the school calendar change. Next, I describe the individual- and household-level data taken from the IHPS. Finally, I describe the crop calendars that collectively comprise the farming calendar in the overlap measure.

3.1 Setting

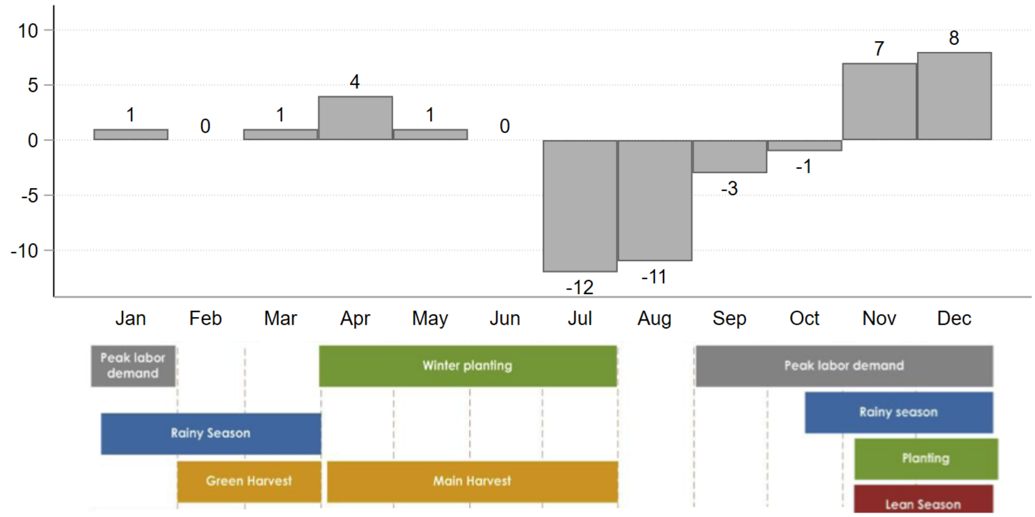
Malawi is a landlocked country in southeast Africa with an estimated population of 13 million people and real GDP per capita of US$300 as of 2008 (World Bank (2010) for all of Section 3.1 unless otherwise noted). At the time of the first IHPS in 2010, 63% lived on less than US$2 a day, and 82% of the population lived in rural areas where most engaged in subsistence, smallholder, rain-fed agriculture. Youth malnutrition was estimated at 49%, and adult literacy was 69%. In 2008, 37% of the population were children ages 5–16—the highest in Southern Africa. Relative to other countries in sub-Saharan Africa (SSA), Malawi had the fifth lowest GDP per capita.

Malawi's main farming period corresponds to its primary rainy season. As shown in bottom panel of Figure 2, Malawi's farming calendar for the primary growing season typically starts with the planting in mid-November and December and ends with the harvesting of crops from April through July (Famine Early Warning Systems Network 2013). The timing of planting is quite concentrated while differences in crop cycle length (i.e., how long it takes a crop to grow) spread out the timing of crop harvest. Consequently, household agricultural labor demand peaks during the planting in November and December, drops significantly from January through March while crops are maintained,

14. Appendix A also describes household responses when these assumptions are relaxed.

Figure 2: **Malawi's School Calendar Change Increased Overlap with Farming Calendar**

Policy-Induced Change in School Days by Month



Typical Farming Calendar

Notes: Table presents a high-level of Malawi's school calendar change and its interaction with the typical farming calendar. The top panel depicts the net change in the number of school days from the 2009 (pre-policy) to the 2011 (post-policy) school calendar, showing that the school calendar change shifted school days out of July and August and to November and December. The bottom panel depicts Malawi's typical farming calendar according to the Famine Early Warning System Network (Famine Early Warning Systems Network 2013), and shows that the shift increased the number of school days during the rainy-season planting (sowing) period—what FEWS NET designates as "peak labor demand". In this study, I use a shift-share estimation strategy that uses the plausibly exogenous variation that Malawi's school calendar change provides to overlap between school and specific crop calendars, which differentially affected communities based on their pre-policy crop allotments.

and then upticks a little during the harvest until the farming season is complete.[15] This farming calendar is largely influenced by maize cultivation with 97% of all farming households harvesting at least some maize just prior to the study period (Denning et al. 2009).[16] Labor by children on the household farm is common and legally permissible (Government of Malawi 2000).[17]

Malawi's formal education system consists of eight grades (or "Standards") of primary school, four grades (or "Forms") of secondary school, and four years at the university level. While private schools exist, 99% of students in primary and 77% in secondary attend public institutions. Between 2000 and 2010, school enrollment increased but fell slightly relative to population growth. School enrollment is heavily concentrated in primary school, particularly grades 1-4, due to high rates of initial enrollment into primary school and grade repetition. Specifically, in 2007, the gross enrollment ratio (GER)[18] was 137% for grades 1-4, 61.4% for grades 5-8, and 16% for secondary school.

15. This characterization is consistent with Figure 6 of de Janvry, Duquennois, and Sadoulet (2022), which plots the estimated mean hours per week demanded by household farms for farming households in Malawi using data from a retrospective agricultural questionnaire.

16. In Section B.2, I present aggregated crop shares across sample communities to show how maize is also very common in the study sample. In Section 4.4, I discuss how the high concentration of maize motivates use of the randomization inference procedure following Borusyak and Hull (2021). Also note that crop calendars for maize do vary in my data depending on the season of production (rainy or dry) and less so for the altitude zone (high, medium, or low).

17. Government of Malawi (2000) states that rules concerning employment of persons under the age of fourteen "shall not apply to work done in homes".

18. The GER is the proportion of overall enrollment, irrespective of age, relative to to the population of the age group intended for a specified educational level

High initial enrollment and late entry in primary school are often attributed to Malawi's Free Primary Education (FPE) program in 1994 that abolished tuition fees for primary school. As a result, households in 2007 paid about US$1.80 on average annually per student in primary school—likely on uniforms, books and materials—and primary school expenditures only represented 17% of households' total education budgets.[19] Thus, primary school fees do not majorly prohibit enrollment.[20]

Additionally, grade repetition increased between 1999 and 2006, reaching a rate of 20% in primary education. A 2004 survey found that 47% of grade 1 students were repeating it, with repeaters of other primary school grades ranging from 13% to 30%. High repetition rates worsen student-teacher ratios, schooling costs and dropout rates. As such, dropout is common, and only 35% of primary school students complete grade 8. School principals reported family responsibilities as the main reason for dropout (44% for boys and 41% for girls) in a 2007 survey, with marriage, pregnancy, and employment stated as other reasons.

3.2 School Calendar Shift

School calendars in sub-Saharan Africa (SSA) do not change often and are highly correlated with former colonial status. According to UNESCO, Institute for Statistics (2022) (UIS) data, in 45 SSA countries from 1997-2019 (over 900 country-years), there are only six instances of countries permanently shifting the start or end months of their school calendar by two months or more: Angola and Ghana both lengthened their calendar, and Malawi, Rwanda, South Sudan and Tanzania have shifted their calendar once. I examine Malawi's school calendar change due to the availability of a detailed record of the school calendar change and contemporaneous household panel data.

In Malawi, the nationwide public school calendar shifted from an early January start date to an early September start date over the course of two school years. The 2009 school calendar started in early January and ended in mid-to-late November. Then, the 2010 school calendar served as a transition year, starting school one month prior in early December 2009 and ending in early August 2010. Finally, the 2010/11 school calendar began the new schedule, starting school an extra three months prior in early September and ending in early-to-mid July.[21] In this study I calculate the change in overlap between the pre-policy 2009 and the post-policy 2011 school calendars.

As shown in the top panel of Figure 2, this school calendar change reduced school days in July and August and increased school days in November and December, largely due to the shift in the end-of-year school break. The exact dates of these school calendars, including breaks, are well documented by Frye (2011). Using this record, I construct

19. See World Bank (2010), Table 3.10 and divide by the 2007/08 exchange rate in Table 1.1.

20. While the FPE program is often credited with a 51% increase in enrollment (e.g., Kattan and Burnett 2004), the 1994 educational reform package also included Malawi's first school calendar change that effectively *reduced* overlap between school and periods of peak farm labor demand. Thus, my results suggest that some of the FPE enrollment boost may have been due to a reduction in overlap with the farming calendar as well as the reduction in its direct costs.

21. Interestingly, this change actually reversed Malawi's first school calendar change in 1994 (prior to the earliest year in the UIS dataset), moving the start date from early September to early January, as part of a large education reform package that also abolished fees for primary school. By contrast, the 2009-11 school calendar change was not accompanied with other well-documented educational reforms.

$SchoolCalendar_{d,t}$, an indicator for if school was scheduled for day $d \in [1,365]$, where $d = 1$ is January 1st and $d = 365$ is December 31st, in time period t, either 2009 (pre-policy) or 2011 (post-policy).

Reasons for the school calendar change as reported by local and district officials (Frye 2011) include: 1) alignment with the university calendar in Malawi; 2) alignment with the university and international school calendar of neighboring countries; 3) closer alignment with the government fiscal calendar, which begins in July; 4) reversing the policy of the previous administration for politically symbolic reasons; and 5) alignment with the initiation schedule for the Yao people, a Bantu ethnic and linguistic group. Reasons (1)-(4) build confidence in the argument that the school calendar change was made without consideration of the farming calendar. Reason (5) may be correlated with the Yao farming calendar; however, in Table 1 (presented in Section 4.5, I do not find any correlation between shift-share overlap and the pre-policy share of Yao households in the community—only 11% of households, on average, across communities—though I include it as a control in my main specification nonetheless to err on the side of caution.

3.3 Panel Survey Data

Outcome data, household agricultural data, and individual time-use data come from Malawi's Integrated Household Panel Survey (IHPS) 2010-2013 (Government of Malawi, National Statistical Office 2013). This section summarizes key variable attributes while additional details are provided in Appendix B.

Outcome data include highest grade level completed and variables describing household-farm work during peak periods. First, $Grade_i$ is the highest grade level completed for individual i in the referenced academic year. Additionally, I have measures of enrollment and extended absences that I use to examine channels affecting overlap's effect on grade level. Second, $Farmed_i$ is an indicator equal to one if individual i worked on household plots during peak periods in the past year, and zero otherwise. Peak periods are defined as rainy-season sowing and harvest periods, when 93.6% of the average household's cultivated acres are under production. Table 1 in Section 4.5 presents summary statistics of baseline values of these outcomes for the sample of interest. In Appendix B.1, I provide more detail on the assumptions made when cleaning the data and further show that the main results are robust to alternative cleaning procedures.

Agricultural data include pre-policy levels of cultivated acres for each crop type recalled for the rainy season 2008/09, dry season 2009, and permanent crops collected in the agriculture questionnaire. I use these data to estimate the pre-policy share of cultivated acres devoted to crop c in community ℓ as the variable $acreshare_{c,\ell}$.[22] To account for differences in crop calendars across different elevations and seasons of production, each crop c is defined as a unique combination of its altitude zone (high, medium, or low), season (rainy, dry, or permanent), and basic crop type (e.g., maize, soybean, etc.). In Appendix B.2, I provide additional details and include tables with aggregated crop

22. Communities are defined by a 16-household enumeration area in the IHPS. See Appendix B for more details.

shares across sample communities. Of 115 crops c grown in the sample, I find that 93.5% of acres are cultivated in the rainy season, 69.5% are cultivated in the medium-altitude zone, and the most common crop—consistent with Denning et al. (2009)—is medium-altitude rainy-season maize, which makes up 41.4% of all cultivated acres in the sample.[23]

Individual-level time use data include estimates of hours worked by members of surveyed households across different labor categories in the household questionnaire and reports of annual hours worked by household members (not hired labor) on household farm plots in the agriculture questionnaire. Using the data from the household questionnaire, I calculate *off-farm labor* as the sum of annual hours worked for primary wage jobs, secondary wage jobs, ganyu (day) labor and unpaid work. Using the data from the agriculture questionnaire, I estimate *on-farm labor* as the sum of annual hours worked by household members on household agricultural plots. Both measures are summed across all adults in the community to estimate annual hours worked on-farm $hours\_onfarm_\ell$ and off-farm $hours\_offfarm_\ell$).[24] I then estimate the on-farm share of community ℓ's total annual hours worked as $farmshare_\ell$. Across sample communities, $farmshare_\ell$ has a mean of 69.1%, meaning that 69.1% of reported adult labor occurs on household farms in the average sample community.[25]

As controls in the regression, I use individual sex and age, household size, and a household asset index as a stable measure of household wealth.[26] Additionally, from the IHPS community-level survey, I use estimates of a community's share of Yao households (given possible heterogeneity discussed in the previous section) and an indicator for if the community experienced a drought in the prior five years.

3.4 Crop Calendars

Data on community crop production are matched to crop calendars from the Food and Agriculture Organization (FAO) Crop Calendar Tool, which provides start and end months for the sowing and harvest periods for 45 major crops in Malawi, which match to 83% of pre-policy cultivated acres in the IHPS data. For remaining 17% of cultivated acres, I use the modal sowing month and harvest month reported by households in the first IHPS. Following the FAO Crop Calendar Tool, I define a crop c as a unique combination of its altitude zone (high, medium, or low), season of production (rainy, dry, or permanent), and basic crop type (e.g., maize, soybean, etc.) as crops can have different calendars for different altitudes and seasons. Using these data, I generate $CropCalendar_{d,c}$, an indicator for if crop c is either being sown or harvested on day $d \in [1, 365]$, where $d = 1$ is January 1st and $d = 365$ is December 31st.

23. In Section 4.4, I discuss how the high concentration of maize motivates use of the randomization inference procedure following Borusyak and Hull (2021), under which my shift-share estimation strategy remains valid.

24. These data were recalled in 2010 for the 2009/10 rainy season—a period that partially occurred during the 2010 transitory school year and thus may be endogenous to the policy change. For this reason, I only use hours worked of individuals aged 23 or older (who are almost entirely out of school) when constructing $hours\_offfarm_\ell$) and prefer to use pre-policy measure of cultivated acres to measure crop shares rather than plot-level labor estimates.

25. Additionally, across sample communities, $farmshare_\ell$ has a median of 86.1%, minimum of 0.0% (no farming), and a maximum of 98.1%.

26. Following Yang et al. (2021), I take the first principal component of a vector of indicator variables for ownership of 12 assets: car, motorcycle, bicycle, radio, television, sewing machine, refrigerator, iron, bed, table, clock, and solar panel. Missing are two assets not reported in the data: freezer and mobile phone.

Additional details and examples of crop calendars for common crops are provided in Appendix B.3.

4 Empirical Approach

4.1 Constructing Overlap

I construct overlap as a shift-share variable that captures a community's exposure to crop-level shocks of changes to overlap between the school and crop calendars. First, I estimate the crop-level shock as the "shift" (or change) in the number of days during which both school is scheduled and crop c is being sown or harvested (times of peak farm labor demand) due to a nationwide change in the school calendar. Then, I estimate the "share" of annual labor devoted to producing crop c in the community, which measures the community's exposure to crop c's overlap shock. Crop-level shocks weighted by the community crop shares are then summed across all crops to collectively capture the community's exposure to the set of crop-level overlap shocks.

I estimate the crop-level shock using the indicator variables characterizing school and crop calendars defined in the previous section. I define $overlap_{c,t}$ as the number of days in year t in which both school is scheduled and crop c is being sown or harvested. Formally,

$$overlap_{c,t} = \sum_{d=1}^{365} (CropCalendar_{d,c} * SchoolCalendar_{d,t}) \tag{1}$$

where time period t takes on the values of the pre-policy year 2009 or the post-policy year 2011. I then estimate the shock change in overlap $\Delta overlap_{c,2011-2009}$ as the difference in $overlap_{c,t}$ for crop c in 2011 relative to 2009. In Appendix B.3, I show pre- and post-policy overlap and the change in overlap shock for common crops in the sample. Across the 115 unique crops grown in the sample, $\Delta overlap_{c,2011-2009}$ has a mean of 2.3 days, standard deviation of 10.7, minimum of -27, and maximum of 21.

I estimate the community's exposure to crop c's overlap shock via the community crop share $share_{c,\ell}$, which captures the relative importance of crop c in community ℓ based on the labor and land resources devoted to it. Specifically, I estimate $share_{c,\ell}$ using community-aggregated IHPS data on on-farm vs off-farm labor and pre-policy levels of cultivated acres by crop by season (described in Section 3.3), as shown in Equation 2. Then, I estimate the community ℓ's share of crop c as the product of these two shares for each crop:

$$\begin{aligned} acreshare_{c,\ell} &= acres_{c,\ell} / \sum_{C} acres_{c,\ell} \\ farmshare_{\ell} &= hours\_onfarm_{\ell} / (hours\_onfarm_{\ell} + hours\_offfarm_{\ell}) \\ share_{c,\ell} &= acreshare_{c,\ell} * farmshare_{\ell} \end{aligned} \tag{2}$$

All shares are $\in [0,1]$. Note that the sum of $share_{c,\ell}$ across all crops is equal to $farmshare_\ell$ since the sum of $acreshare_{c,\ell}$ across all crops is equal to 1.

Putting these together, I estimate shift-share overlap for each community by weighting the crop-level shock by community crop share $share_{c,\ell} \in [0,1]$ and summing across crops:

$$ssoverlap_\ell = \sum_c (share_{c,\ell} * \Delta overlap_{c,2011-2009}) \tag{3}$$

Finally, I normalize $ssoverlap_\ell$ to unit variance across all IHPS communities surveyed in 2010. Across the 135 communities in the sample, this variable *before normalizing* has a mean of 8.0, standard deviation of 4.5, minimum of -3.9, and maximum of 17.8, and *after normalizing* has a mean of 1.8, standard deviation of 1.0, minimum of -0.9, and maximum of 4.0. To put $ssoverlap_\ell$ into perspective, I run a simulation to estimate that adding five days (i.e., one week of school) of overlap spread across sowing and harvest of rainy-season maize in the average sample community is nearly equivalent to a 0.61 standard deviation increase in normalized $ssoverlap_\ell$.[27] Therefore, to ease interpretation when discussing the results, I will multiply estimated coefficients by 0.61 and interpret the findings as a five-day (i.e., one school week) increase in overlap during peak farming periods.

Moreover, for secondary analyses, I construct versions of shift-share overlap that refer to specific periods of the farming and school calendar. First, I distinguish between sowing and harvest periods in $CropCalendar_{d,c}$ and use the process described above to construct measures of shift-share overlap during sowing $ssoverlap\_sow_\ell$ and harvest $ssoverlap\_harv_\ell$, which together sum to $ssoverlap_\ell$. Second, I identify the first and last four weeks of school in $SchoolCalendar_{d,t}$ to construct measures of shift-share overlap between peak farming periods and the first month of school $ssoverlap\_admis_\ell$ and last month of school $ssoverlap\_exams_\ell$, which represent the important admissions and exam periods. All new shift-share overlap regressors are normalized to unit variance of $ssoverlap_\ell$ to have comparable coefficients.

4.2 Sample

I define my sample as individuals surveyed about their schooling outcomes for the pre-policy school year 2009 when they were 6-13 years old. In the IHPS 2010 (henceforth, "baseline"), this includes households that were interviewed prior to the last scheduled day of the 2010 school year (i.e., August 7, 2010), so that schooling responses about the previous academic year refer to the pre-policy 2009 school year. The age range of 6-13 years old ensures that the sample is fully exposed to the shock over the four-year study period. In Malawi, children typically start school at age six and, if they complete one grade each school year, can complete primary school by age 13 and secondary school by age 17. Thus, from 2009 to when they are ages 10-17 in 2013, the sample are eligible school-aged children—the

27. See Appendix C.1 for details on the simulation used to make this comparison.

population of interest for this study.[28]

This yields a sample of 2,287 individuals at baseline, 2,142 of which are still present in the IHPS 2013, producing a sample retention rate of 94% that Table 1 shows is not correlated with shift-share overlap. Table 1 also provides descriptive statistics of the sample: 50% female, average of 9.3 years old, and belonging to a household with an average of 6.6 members. At baseline, each individual had completed an average of 1.4 grades, and 25% were reported to work on the household farm during the peak rainy season. Balance tests across these and other variables are described in Section 4.5.

Within the sample, both schooling and household-farm work are indeed common at baseline. For schooling, 81% of children aged 6–13 years in my sample were enrolled in school. Most un-enrolled children had not started school. Among enrolled children, 29% were enrolled in grade 1, 51% in grades 1-2, and 81% in grades 1-4. The large share of children out-of-school or in lower grades helps explain how the average child has only completed 1.4 grades—the baseline mean for $Grade_i$. For farming, 98% of the sample live in farming communities (i.e., those EAs with some reported on-farm labor), and 86% live in farming households. Moreover, 25% were reported to work on the household farm during the peak rainy season—the baseline mean for $Farmed_i$—which includes about 15% of children age 6 and 39% of children age 13.

4.3 Regression

Using the fact that the Borusyak, Hull, and Jaravel (2022) setup nests shift-share reduced-form regressions, I estimate the panel data using the following ANCOVA specification to identify the causal effect β of $ssoverlap_\ell$:

$$Y_{i,\ell} = \alpha + \beta ssoverlap_\ell + \delta Y_{pre,i,\ell} + \rho farmshare_\ell + \mathbf{w}'_{i,\ell}\gamma + \varepsilon_{i,\ell} \quad (4)$$

where $Y_{i,\ell}$ is the outcome for individual i in community ℓ from the IHPS 2013; $ssoverlap_\ell$ is the shift-share regressor calculated in Equation 3; $Y_{pre,i,\ell}$ is the baseline value of the outcome from the IHPS 2010[29]; $farmshare_\ell$ is the on-farm share of total annual hours worked (i.e., the sum of crop shares); $w_{i,\ell}$ is a vector of controls defined below; and $\varepsilon_{i,\ell}$ is an error term.

Two critical controls are $Y_{pre,i,\ell}$ and $farmshare_\ell$. The inclusion of the baseline outcome measure $Y_{pre,i,\ell}$ makes Equation 4 an ANCOVA specification that utilizes the full extent of the panel data. Consequently, one should interpret β as the *change in* the outcome between the IHPS 2010 and IHPS 2013. The control $farmshare_\ell$ represents the sum of community crop shares across crops, as recommended by Borusyak, Hull, and Jaravel (2022) in the case of incomplete

28. This age range is also supported by pre-policy data of enrollment by age showing over half of children being enrolled in school from ages 6 to 17 but not otherwise.

29. The baseline value for $Grade_i$ comes from the pre-policy school year 2009, and the baseline value for $Farmed_i$ comes from the 2009/2010 rainy season.

shares. It ensures that the regression compares individuals from communities with comparable shares of on-farm labor while estimating the effect of $ssoverlap_\ell$.

The vector of pre-policy controls $\mathbf{w}_{i,\ell}$ includes the location-level controls and crop-level controls. Location-level controls include individual sex and age; household size and asset index; an indicator for three sample communities containing no farming households, the community's share of Yao households, and an indicator for if the community experienced a drought in the prior five years. Crop-level controls include: seasonal dummies (rainy, dry, permanent) and a dummy for grain crops as share-weighted location-level variables (as in Borusyak, Hull, and Jaravel (2022)) and altitude zone. Crop season and altitude are included as controls as both are correlated with crop calendar length and thus crop-level overlap as well, while the grain dummy captures the community share of maize—Malawi's dominant cash and food crop. I also include regional fixed effects to account for any additional spatial correlation in crop calendar length or spurious spatial autocorrelation in the outcomes.[30] Finally, I include the baseline values of all the main outcomes $Grade_i$ and $Farmed_i$ in each regression of post-policy outcomes.[31]

Given the theoretical model's predictions that additional overlap in the school and farming calendars reduces both time in school and hours worked on the household farm, I hypothesize that $\beta < 0$ for the main outcomes $Grade_i$ and $Farmed_i$.

Most secondary analyses will use the primary specification in Equation 4 but with alternative subsamples or outcomes. In Appendix D, I detail the specification used to estimate the causal effect of $ssoverlap_\ell$ at specific periods in the farming and school calendar.

4.4 Inference

Following the shift-share framework put forth by Borusyak, Hull, and Jaravel (2022), causal identification requires the assumption that the crop-level overlap shocks are as-good-as-randomly assigned, which is supported by a balance test presented below in Section 4.5, the non-agricultural rationale for implementing the policy (i.e., to better align the school calendar with government, university and neighboring countries' calendars), and the fact that it inadvertently *increased* overlap between school and most crop calendars. This strategy is analogous to other papers that construct a shift-share variables by identifying as-good-as-random industry-level shocks and then taking their weighted average using shares of local industry employment to measure local-level shock exposure (Bartik 1991; Blanchard et al. 1992; Autor, Dorn, and Hanson 2013).[32] Together, these support interpreting β in Equation 4 as the *causal* effect of overlap

30. The sample is divided across Malawi's three regions as follows: 37.2% (N=796) in the Southern region, 39.4% (N=844) in the Central region, and 22.4% (N=502) in the Northern region.

31. The residual after regressing $ssoverlap_\ell$ on the controls $farmshare_\ell$ and $\mathbf{w}_{i,\ell}$ has a mean of 0.005, standard deviation of 0.273, minimum of -1.44, and a maximum of 1.18. This suggests that the controls have not introduced bias and, as expected, explain much of the variation in shift-share overlap while still leaving sufficient variation for the analysis that is plausibly exogenous.

32. Conversely, it is distinct from estimation strategies that might rely on exogeneity in crop shares, including other shift-share settings (Goldsmith-Pinkham, Sorkin, and Swift 2020) or difference-in-difference strategies, as crop shares are allowed to be endogenous to household characteristics in the current setup.

on the outcome.

However, conventional standard errors in shift-share regressions do not account for unobserved correlation between observations with similar exposure shares, and tend to over-reject when this correlation is positive (Adão, Kolesár, and Morales 2019; Borusyak, Hull, and Jaravel 2022). Therefore, I test the hypotheses by employing a randomization inference (RI) procedure that compares my actual effects to those estimated from counterfactuals of shift-share overlap. Following insights from Borusyak and Hull (2021), I specify a shock assignment process aligning with my assumption that the crop-level change-to-overlap shocks are plausibly exogenous to my outcomes, while holding fixed the community crop shares that are likely endogenous. By using shock counterfactuals to simulate an empirical distribution of test statistics, I can test the sharp null hypothesis that shift-share overlap has no effect for any observation by checking if actual test statistics are in the tails of the null distribution (Fisher 1935). This RI test remains valid despite the high concentration of rainy-season maize in the data that could hinder asymptotic approximation (Borusyak and Hull 2021).

The RI procedure is as follows. First, I randomly draw (with replacement) crop-level shocks $\Delta overlap_{c,2011-2009}$ from its actual distribution. Second, I weight the redrawn shocks by actual crop shares $share_{c,\ell}$, and sum across crops to generate a counterfactual shift-share overlap measure for each location (as in Equation 3). Third, I run the regression specified in Equation 4, replacing only the actual shift-share overlap measure with the counterfactual one, and collect the counterfactual β, called $\tilde{\beta}$. Then, I repeat these three steps 1000 times. Finally, I calculate Fisher exact p-values as the fraction of $\tilde{\beta}$ for which $|\tilde{\beta}| \geq |\hat{\beta}|$.

In Appendix E, I describe alternative—and in some cases more conservative—inference procedures that I use to test the robustness of my results. Alternative RI procedures include drawing shocks from a normal distribution defined by the first and second moment of the actual overlap distribution, redrawing shocks with replacement within season, and estimating counterfactual shocks based on simulated school calendar changes. I also run a share-weighted shock-level regression that produces exposure-robust standard errors (Borusyak, Hull, and Jaravel 2022). Further, I detail how the high concentration of rainy-season maize in my data possibly violates a key assumption for their procedure, which explains why I chose the RI procedure for inference ex ante.

4.5 Balance Tests

To evaluate the key identifying assumption that shocks are as-good-as-randomly assigned, I implement a "balance tests" on baseline values of key outcomes and location-level characteristics outlined in Borusyak, Hull, and Jaravel (2022). These variables proxy for the unobserved residual evaluated via the RI procedure. The results support the assumption that shift-share overlap is pseudo-randomly assigned to observations in Equation 4.

Specifically, I check for balance in retention between the eligible 2010 IHPS sample and the 2013 IHPS sample to rule out sample selection issues confounding the analysis. Then, I check for balance on the baseline values of $Grade_i$

and $Farmed_i$ and location-level controls. Balance regressions are estimated on the following:

$$Y_{base,i,\ell} = \alpha + \beta ssoverlap_\ell + \rho farmshare_\ell + \mathbf{w}'_{i,\ell}\gamma + \varepsilon_{i,\ell} \tag{5}$$

where $Y_{base,i,\ell}$ is the outcome but all other regressors from Equation 4 are included except when serving themselves as the dependent variable.

Table 1 presents summary statistics and balance test results. Retention between the 2010 and 2013 IHPS sample is 94% and is balanced with respect to shift-share overlap. Baseline values of the primary outcomes $Grade_i$ and $Farmed_i$ are also balanced, alleviating concern that imbalances could drive the main results. Finally, all baseline location-level controls are balanced at the 95% confidence level, including the community share of Yao households (alleviating concern that the Yao ethnic group's initiation was a possible reason for the school calendar change).[33] Overall, the balance tests substantiate the study's identification assumption that the crop-level overlap shocks are as-good-as-randomly assigned and that the regressor contains the exogenous variation required for causal interpretation of the results.

Table 1: **Summary Statistics and Balance Tests**

	Summary Statistics			Balance Test	
VARIABLES	N	Mean	SD	Coef.	RI pval
Retention:					
Included in 2013 Sample	2,287	0.94	0.24	-0.015	0.133
Baseline Controls:					
Baseline Values of Primary Outcomes:					
$Grade_i$: Highest Grade Completed	2,142	1.43	1.70	-0.098	0.312
$Farmed_i$: Indicator if Farmed in Peak Rainy Season	2,142	0.25	0.43	0.010	0.565
Other Location-Level Controls:					
Individual Sex: Female Indicator	2,142	0.50	0.50	0.017	0.615
Individual Age	2,142	9.29	2.29	0.093	0.359
Household Size	2,142	6.64	2.32	-0.229	0.312
Household Asset Index	2,142	0.92	1.08	-0.102	0.068
Community had no on-farm labor	2,142	0.02	0.13	0.011	0.496
Community had drought in prior 5 years	2,142	0.28	0.45	-0.181	0.095
Community share of Yao Hhs	2,142	0.11	0.24	0.088	0.282

Notes: Table presents summary statistics and a baseline balance test of pre-policy characteristics (the latter proposed by Borusyak, Hull, and Jaravel (2022) as a falsification test). Columns (1)-(3) report sample summary statistics for retention and baseline controls. Columns (4)-(5) report the coefficient and randomization inference p-values (RI pval) from "balance test" regressions of retention and baseline controls on $ssoverlap_\ell$ (defined as "Shift-Share Overlap$_\ell$" in Table 2). Regressions also include specified controls from Equation 5: on-farm share of total annual hours worked; individual sex and age; household size and asset index; community-level indicator for containing no farming households, the community's share of Yao households, and an indicator for if the community experienced a drought in the prior five years; crop-level controls seasonal dummies (rainy, dry, permanent) and a dummy for grain crops as share-weighted location-level variables and altitude zone. Excluded from the regression is the baseline value of the control when serving themselves as the dependent variable.

33. Further, all variables but two are balanced at the 90% confidence level. Communities with greater exposure to the school calendar change had *marginally* fewer assets but were less likely to experience a drought in the prior five years, that latter possibly making these communities relatively better off in the pre-policy period.

5 Empirical Results

5.1 Primary Results

The primary results support the model's predictions that the added time constraint imposed by additional overlap between the school and farming calendars decreases time allocations to both school and work on the household farm. Table 2 presents results testing the paper's primary hypotheses with conventional standard errors in parentheses and randomization inference (RI) p-values in square brackets. In columns (1) and (2), I estimate a statistically significant negative effect of shift-share overlap on both $Grade_i$ and $Farmed_i$. In Appendix E, I show that these results are also robust to other and often more conservative inference procedures.

Table 2: **Shift-Share Overlap Effects on Changes in Children's Grade Level and Household-Farm Labor**

VARIABLES	Highest Completed Grade in 2013 "Grade" (1)	Indicator if Farmed in 2013 "Farmed" (2)
Shift-Share Overlap$_\ell$	-0.232	-0.064
	(0.116)	(0.036)
	[0.038]	[0.044]
Pre-Policy Outcome$_i$	0.821	0.129
	(0.044)	(0.025)
Observations	2,142	2,142
R-squared	0.638	0.213
Pre-Policy Outcome Mean	1.43	0.25
Δ Outcome Mean (Post - Pre)	2.30	0.22

Notes: Table presents results of the main specification: "Shift-Share Overlap$_\ell$" is a shift-share measure of change in overlap between the school and farming calendars for community ℓ, constructed as the policy-induced "shift" in the number of days during which school and a crop's production both occurred, weighted by the pre-policy "share" of community labor devoted to producing each crop, summed across all crops, and normalized to unit variance (denoted $ssoverlap_\ell$ in Equation 4). "Pre-Policy Outcome$_i$" is the pre-policy value of the outcome (denoted $Y_{pre,i,\ell}$ in Equation 4), making this an ANCOVA specification that measures impacts on changes in outcomes. Outcomes are from 2013 from school-aged individuals ages 6-13 pre-policy: column (1) is an individual's last completed grade level "Grade", column (2) is an indicator equal to one if an individual worked on the household farm during the rainy-season sowing and harvest periods, and zero otherwise, "Farmed" and column (3) is the corresponding number of hours worked "Farm Hours". Regressions include specified pre-policy controls, which include the baseline value of the outcome; the on-farm share of total annual hours worked; individual sex and age; household size and asset index; community-level indicator for containing no farming households, the community's share of Yao households, and an indicator for if the community experienced a drought in the prior five years; crop-level controls seasonal dummies (rainy, dry, permanent) and a dummy for grain crops as share-weighted location-level variables and altitude zone; regional fixed effects to account for spatial correlation; and pre-policy values of "Grade" and "Farmed" (when not already included). Conventional robust standard errors in parentheses. Randomization inference p-values in square brackets.

These negative effects are substantial across the four-year period. Recall from Section 4.1 that a five-day (i.e., one school week) increase in overlap during the rainy-season sowing and harvest in the sample-average community increases shift-share overlap by 0.61 standard deviations. Then, multiplying 0.61 by the coefficients in Table 2, I estimate that a five-day increase in overlap during peak production decreases $Grade_i$ by 0.14—equivalent to one grade lost for every seven children. Additionally, a five-day increase in overlap during peak production decreases $Farmed_i$ by 3.9 percentage points. With the share of the sample engaged in household-farm work increasing by 22 percentage points over the four-year panel period (as the children age and physically mature), this represents an almost one-fifth reduction in children now working on their household farm. Based on the theoretical model (as depicted in Figure 1), these overlap-induced reductions in children's time allocations to both schooling and farm work should bring about an unambiguous decrease in household utility.

5.1.1 Time Allocation Analysis

Next, I test for overlap's effects on the best available measures of time allocation to schooling and farming. I find that school participation is mostly impacted along its intensive margin (i.e., not via enrollment), while farming is mostly impacted along its extensive margin (i.e., whether or not a child farms). These results are informative of how households make decisions regarding their children's time and lend further support to the theoretical model.

First, I test for overlap's effect on a cascading set of outcomes that represent three possible extensive margins for schooling. Column (1) is whether an individual started school by 2013 (i.e., initial enrollment). For those who had started school by 2013, column (2) is whether an individual was currently enrolled in school in 2013. And for those who were enrolled in 2013, column (3) is whether an individual is reported by the household respondent to have missed more than two consecutive weeks of school in the previous school year (i.e., extended absence). Effects of overlap on these outcomes are small in magnitude and statistically insignificant, suggesting that these extensive margins are not driving overlap's effect on grade advancement.

Table 3: **Shift-Share Overlap Effects on Time Allocations**

	Time in School in 2013				Time Farming in 2013	
	Started School	Enrolled if Started School	Absent 2 Wks if Enrolled	Grade Intensive	Hours Spent Farming	Farming Intensive
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Shift-Share Overlap$_\ell$	-0.006	0.006	-0.010	-0.151	-3.932	-2.500
	(0.019)	(0.023)	(0.023)	(0.113)	(3.750)	(8.520)
	[0.682]	[0.619]	[0.466]	[0.132]	[0.183]	[0.767]
Pre-Policy Outcome$_i$	0.277	0.095	0.001	0.860	0.227	0.125
	(0.023)	(0.020)	(0.030)	(0.043)	(0.092)	(0.062)
	[0.682]	[0.619]	[0.466]	[0.132]	[0.183]	[0.767]
Observations	2,142	2,043	1,874	1,515	2,142	1,016
R-squared	0.299	0.138	0.031	0.671	0.207	0.105
Pre-Policy Outcome Mean	0.83	0.85	0.04	1.61	9.82	22.05
Δ Outcome Mean (Post - Pre)	0.12	0.06	0.02	2.63	21.49	57.24

Notes: Table presents results of the main specification but with a cascading set of outcome variables to analyze possible channels for overlap's negative effect on schooling. Dependent variables and sub-samples defined as follows: Column (1) is an indicator equal to one if an individual started school by 2013, and zero otherwise. Conditional on starting school by 2013, column (2) is an indicator equal to one if an individual was enrolled in school in 2013, and zero otherwise. Conditional on enrollment in 2013, column (3) is an indicator equal to one if an individual is reported by the household respondent to miss more than two consecutive weeks of school in the last year, and zero otherwise. Finally, conditional on enrollment in school in 2009 and 2013 and not missing more than two consecutive weeks of school, column (4) is a measure of highest completed grade level ruling out possible effects on the extensive margins. Regressor $ssoverlap_\ell$ and included controls defined in Table 2. Conventional robust standard errors in parentheses. Randomization inference p-values in square brackets.

Second, I test for overlap's effect on the intensive margin of schooling by testing on a sub-sample that was least likely to be affected by these extensive margin channels. Table 3 column (4) regresses $Grade_i$ on shift-share overlap, as in Table 2, but only for the sub-sample of individuals enrolled in school in both 2009 and 2013 and not reported to miss more than two consecutive weeks of school in 2013. The smaller sample size reduces analytical power, but the result is a negative coefficient that is sizable in magnitude with an RI p-value of 0.132. While not conclusive, it suggests that overlap's effect predominately occurs at the intensive margin, especially when compared to the null results on the extensive margin. One may speculate about whether this intensive margin effect is due to reduced school attendance (for less than two consecutive weeks) or less time spent after school on studying or homework; however, data limitations make testing of these intensive margin mechanisms an ambition of future work.

Third, I test for overlap's effect on hours worked on the household farm during peak periods, both for the full sample in column (5) and among the 47% of the sample who engaged in any household farm work in 2013 in column (6). The outcome is a child's total of annual hours worked on household plots during the sowing and harvest periods in the rainy season, according to household reports. In each, I estimate negative coefficients that are small in magnitude and statistically insignificant. Moreover, even at the upper bound of the 95% confidence interval, column (5) implies that a five-day increase in overlap during peak production decreases annual hours worked at the household farm by only 6.88 hours, on average.[34] To put that into perspective, this is about one school day for the median grade level in primary school.[35] Overall, this suggests that overlap's effect on household-farm work lies along its extensive margin.

5.1.2 Speculative Extensions of the Primary Results

The primary results relate to, but cannot provide precise estimates of, overlap's effect on daily school attendance and its implications on grade progression and the perceived value of time in school. However, borrowing assumptions from the theoretical model and some back-of-the-envelope calculations, I believe I can extend the primary results to infer some likely possibilities. Recognizing the use of strong assumptions and lack of empirical evidence, I cautiously label this section as "speculative" but present it nonetheless to relate the study to other stands of literature and hopefully motivate future work on this topic.

First, I aim to infer impacts on daily school attendance. Under Assumption 1 of the theoretical model, a five-day increase to overlap must decrease a child's total allocation to school and farming by exactly five days.[36] And, in Table 3 column (5), I estimate at the upper limit of the 95% confidence interval that five-day increase in overlap during peak production could decreases annual household-farm work by, at most, 6.88 hours—about one day. Then, if Assumption

34. The calculation is: [3.932 (point estimate) + 1.96 * 3.770 (standard error)] * -0.61 (simulated change in SD for a five-day increase in overlap)

35. Generally, Malawian public primary school starts at 7:30 for all standards and ends after 4.5 hours for Standards 1-2, 6.5 hours for Standards 3-4, and 7 hours for Standards 5-8.

36. Recall Assumption 1 of the theoretical model: households always prefer schooling and farming, when available, to leisure given some number of calendar days in which only leisure (and neither school nor farming) is available.

1 holds, the same five-day increase in overlap must decrease time in school by, at least, four days annually.[37] If so, this suggests that household demand for child labor during peak farming periods is relatively more inelastic than demand for time in school, on average, though pinning down precise estimates for overlap's effect on time in school must be left to future research.

Further, if a five-day increase in overlap induces a four-day decrease schooling annually, which sums to 12 missed days of school over the three post-policy years, the implied effect of student absenteeism on grade advancement is large. Twelve absent days is equivalent to missing 6.2% of the 195-day post-policy school year and yet it leads to a 0.14 grade level reduction, explaining less than half of the decline. One possible explanation is that absences lead to direct learning losses on days missed and then also reduce the return to learning on future school days attended, consistent with a model of human capital accumulation where early losses multiply future deficits (Cunha and Heckman 2007).[38] Alternatively, student absences during farming periods may serve as a negative signal of student prospect and ability in an environment with limited and noisy measures of student performance.[39] Again, testing such explanations is left to future research on the determinants of grade progression in developing contexts.

Second, I approximate the perceived value of time in school for the average sample household, following calculations detailed in Appendix F.1. As a starting point, I assume that the average overlap-constrained household will reallocate time to each activity until the point where the expected utility lost from decreases in schooling is equal to the expected utility lost from decreases in farm work (given monotonic and concave preferences for schooling and farm work, as assumed in the model). If so, then the results in Table 2 column (1) and 3 column (5) suggest that, on the margin, households equate 0.14 grades to three days of household-farm work (i.e., an upper-bound estimate of one day annually for three post-policy years) in terms of their expected impact on household utility. To estimate the perceived value per completed grade, I multiply the three lost days of household-farm work by the average under-15 daily wage for hired farm labor, and divide by 0.14. This procedure produces an upper-bound estimate for the perceived value of a completed grade of school for the average household at $50 in 2009 USD.

By contrast, I project that a five-day increase in overlap during peak production can reduce a child's present discounted value of lifetime income by approximately $156 USD. These calculations are detailed in Appendix F.2, where I replicate Montenegro and Patrinos (2014)'s "Mincerian" estimates for the average rate of return for another year of schooling in Malawi at 9.8% in 2010. With the sample-average annual income across wage and ganyu work for working-age adults with 1–12 years of schooling being $444 in 2009 USD, this equates to a nominal annual return of $44 USD, which—if added to annual income for all working-age years—has an approximate present discounted

37. If Assumption 1 does not hold, then overlap's effect on time in school may be smaller, though not so much smaller that it cannot feasibly explain the observed reductions in highest completed grade level.

38. This explanation is consistent with evidence from developed countries that being absent for 10 days annually in elementary school reduces student performance by 4.5 percent and 8 percent of a standard deviation (Cattan et al. 2023; Goodman 2014, respectively), but inconsistent with evidence on low rates of learning in sub-Saharan Africa (World Bank 2017).

39. This is consistent with evidence from South Africa that grade progression has a large unobserved and perhaps stochastic component, leading to high rates of enrollment and grade repetition (Lam, Ardington, and Leibbrandt 2011).

value to an individual of $1,112 USD under standard discounting assumptions. I also estimate higher returns under other sensible assumptions, building confidence that these results are reasonable (if not conservative). Then, if a five-day increase in overlap during peak production reduces grade completion by 0.14 grades, it can reduce the present discounted value of lifetime income by approximately $1,112 USD 0.14 = $156 USD for the average child.

Comparing potential lifetime returns of $156 to the perceived value of $50 for one completed grade of school, the average sample household's revealed valuation of an additional year of school is only about 32.1% of its potential contribution to the child's lifetime income. This finding is surprising but also consistent with the literature on households underestimating the returns to education (Jensen 2010; Nguyen 2008) and specifically Dizon-Ross (2019)'s 2012 estimates of Malawian households' perceived returns to secondary school relative to primary-school earnings of 3.2%—again, about one-third of Montenegro and Patrinos (2014)'s "Mincerian" estimate of 9.8%. Other possible explanations include households strategically under-investing in schooling to benefit the household (Jensen and Miller 2017), present bias driven by short-term consumption needs, liquidity constraints, or underestimating the negative effect of lost time in school on grade advancement. Regardless of the reason, this back-of-the-envelope comparison highlights how households underestimate the value of time in school when making marginal decisions about how to best allocate their child's time under constraints.

5.2 Secondary Results

Secondary analyses further characterize the significant negative effects of overlap between the school and farming calendars on $Grade_i$ and $Farmed_i$. First, I test for heterogeneity by a child's age, sex and household wealth. Second, I test for impacts of overlap during different farming and schooling periods. Third, I assess impacts on household farming decisions and other forms of child labor.

5.2.1 Heterogeneity Analysis

In this section, I test for heterogeneity in the primary analysis by individual sex and age and by household asset wealth. Doing so reveals that boys, younger children, and households in the lower two-thirds of asset wealth may be somewhat more affected by overlap between the school and farming calendars.

Table 4 examines heterogeneity in the main results by individual's sex, age and household asset wealth. All interaction variables are already included as controls and are defined with summary statistics in Table 1. In columns (1)-(2), "Shift-Share Overlap$_\ell$" is interacted to an indicator equal to one if the individual is female and is zero otherwise. Here, the results reveal that overlap's constraining effects on schooling and household-farm work are experienced most for boys. For boys, a five-day increase in overlap during peak production decreases $Grade_i$ by 0.17 and reduces $Farmed_i$ by 4.4 percentage points. Meanwhile, the interaction effect for girls is positive and sizable in magnitude but statistically insignificant, suggesting that both genders are vulnerable. In columns (3)-(4), the regressor is interacted with the individual's pre-policy age. Here, the results reveal that overlap's constraining effects on schooling and household-farm work are experienced most for younger children, with a positive and almost statistically significant interaction terms that suggest that the effects of overlap somewhat less as children age. This may be because, after leaving school, children are no longer bound by the overlap constraint. Finally, in columns (5)-(6), the regressor is interacted with the household's pre-policy asset wealth index. Again, the interaction term is positive (though statistically insignificant), suggesting that overlap is perhaps experienced more by poorer households, though the next table sheds more light on this.

Table 4: **Shift-Share Overlap Heterogeneous Effects by Sex, Age and Asset Wealth**

INTERACTING VARIABLE:	**Female Indicator**		**Individual Age**		**Household Asset Index**	
	Grade	Farmed	Grade	Farmed	Grade	Farmed
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Shift-Share Overlap$_\ell$	-0.278	-0.073	-0.453	-0.153	-0.294	-0.060
	(0.118)	(0.037)	(0.177)	(0.054)	(0.116)	(0.037)
	[0.031]	[0.029]	[0.037]	[0.047]	[0.019]	[0.079]
Interaction with Control &	0.104	0.023	0.024	0.009	0.095	-0.006
Shift-Share Overlap$_\ell$	(0.064)	(0.019)	(0.015)	(0.004)	(0.030)	(0.009)
	[0.238]	[0.265]	[0.106]	[0.135]	[0.209]	[0.575]
Observations	2,142	2,142	2,142	2,142	2,142	2,142
R-squared	0.638	0.213	0.638	0.214	0.640	0.213
F-test	0.074	0.132	0.068	0.067	0.030	0.220

Notes: Table presents results of the main specification with "Shift-Share Overlap$_\ell$" interacted with a control variable already included in the regression. In columns (1)-(2), the regressor is interacted with an individual's gender where the interaction variable is equal to one if the individual is female and is zero otherwise. In columns (3)-(4), the regressor is interacted with the individual's pre-policy age. In columns (5)-(6), the regressor is interacted with the household's pre-policy asset wealth index. Regressors "Shift-Share Overlap$_\ell$" and included controls defined in Table 2. All interaction variables are defined with summary statistics in Table 1. Conventional robust standard errors in parentheses. Randomization inference p-values in square brackets.

Next, Table 5 examines heterogeneity in the main results by household asset wealth. Results of the main specification are presented for subsamples of individuals belonging to the bottom, middle or top tercile of their households' pre-policy asset index. First, I compare the relative trade-off between overlap-induced declines in school and farming between the lower two terciles. While the poorest households in the bottom tercile make larger reductions in schooling (significant at the 90% confidence level), households in the middle tercile make larger reductions to household farming, consistent with the idea that poorest households are most reliant on child labor. However, I make this interpretation cautiously given these point estimates are not significantly different for each other. Further, overlap's effects appear weaker for wealthier households, as coefficients corresponding to the top tercile of household asset wealth are much smaller in magnitude and generate weaker RI p-values than in other sub-samples. This would be the case if wealthier households are less vulnerable to time constraints on the joint allocation to school and household-farm work—for example, if wealthier households are less likely to have their children work on a household farm. Indeed, 27% of top-terciles households do not even have a farm, compared to only 9% of households in the lower two terciles.

Table 5: **Shift-Share Overlap Effects by Household Assets**

	Bottom Tercile		Middle Tercile		Top Tercile	
	Grade	Farmed	Grade	Farmed	Grade	Farmed
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Shift-Share Overlap$_\ell$	-0.283	0.020	-0.157	-0.146	-0.052	0.001
	(0.249)	(0.061)	(0.163)	(0.062)	(0.203)	(0.081)
	[0.075]	[0.560]	[0.158]	[0.012]	[0.735]	[0.991]
Pre-Policy Outcome$_i$	0.901	0.049	0.839	0.163	0.765	0.181
	(0.071)	(0.038)	(0.083)	(0.042)	(0.072)	(0.054)
Observations	768	768	708	708	666	666
R-squared	0.531	0.174	0.614	0.245	0.652	0.191
Pre-Policy Outcome Mean	0.96	0.33	1.24	0.24	2.17	0.18
Δ Outcome Mean (Post - Pre)	2.00	0.25	2.13	0.25	2.82	0.17

Notes: Table presents results of the main specification on sub-samples determined by an individual's household asset index (estimated as the first principal component of a vector of indicator variables for ownership of 12 assets in 2010). Results for the bottom tercile of the asset index are in columns (1)-(2), middle tercile in columns (3)-(4), and top tercile in columns (5)-(6). Regressors "Shift-Share Overlap$_\ell$" and "Pre-Policy Outcome$_i$" and included controls defined in Table 2. Conventional robust standard errors in parentheses. Randomization inference p-values in square brackets.

5.2.2 Period-Specific Effects

To test which calendar periods are most sensitive to overlap, Table 6 presents variations of the main specification with regressors representing overlap during different farming and school periods. The results suggest that labor demand is relatively inelastic during the labor-intensive sowing period, leading to greater reductions in schooling and smaller reductions in farming.

First, columns (1) and (2) estimate how the impact of overlap varies between sowing and harvest periods on grade advancement and household-farm work, respectively. Both columns estimate negative point estimates on sowing and harvest overlap, and the estimates are not statistically different from each other. However, only sowing overlap is marginally significant in column (1) (RI p-value of 0.100), while only harvest overlap is statistically significant in column (2). While not conclusive, the results broadly support the hypothesis that labor demand is relatively inelastic during the sowing period, with households less willing to reduce their children's household-farm work as compared to the harvest period at the expense of time in school.

One possible explanation is that the rainy-season sowing period is generally more concentrated in the calendar year (de Janvry, Duquennois, and Sadoulet 2022), meaning that sowing is a period of peak labor demand even relative to the harvest. Indeed, in Appendix C, I find that a five-day (i.e., one school week) increase during rainy-season sowing increases shift-share overlap by 0.68 standard deviations, while a five-day increase during rainy-season harvest increases it by only 0.54 standard deviations—the difference caused by the fact that sowing periods across all crops are more concentrated from mid-November through December, whereas harvest periods across all crops vary from

Table 6: **Shift-Share Overlap Effect at Different Time Periods**

VARIABLES	Grade (1)	Farmed (2)	Grade (3)	Farmed (4)	Grade (5)	Farmed (6)
Shift-Share Overlap$_\ell$			-0.218	-0.018		
			(0.128)	(0.041)		
			[0.082]	[0.587]		
Shift-Share Overlap: Sowing$_\ell$	-0.298	-0.025			-0.365	0.019
	(0.166)	(0.048)			(0.181)	(0.054)
	[0.100]	[0.592]			[0.091]	[0.761]
Shift-Share Overlap: Harvest$_\ell$	-0.177	-0.096			-0.045	-0.062
	(0.126)	(0.044)			(0.163)	(0.057)
	[0.210]	[0.039]			[0.771]	[0.214]
Shift-Share Overlap: Admission$_\ell$			0.027	-0.036	0.155	-0.069
			(0.126)	(0.041)	(0.158)	(0.052)
			[0.797]	[0.282]	[0.176]	[0.075]
Shift-Share Overlap: Exams$_\ell$			0.030	0.075	0.102	0.057
			(0.099)	(0.030)	(0.112)	(0.035)
			[0.750]	[0.023]	[0.351]	[0.090]
Pre-Policy Outcome$_i$	0.821	0.128	0.820	0.129	0.820	0.128
	(0.044)	(0.025)	(0.044)	(0.025)	(0.044)	(0.025)
Observations	2,142	2,142	2,142	2,142	2,142	2,142
R-squared	0.638	0.213	0.638	0.215	0.638	0.216
Pre-Policy Outcome Mean	1.430	0.252	1.430	0.252	1.430	0.252
Δ Outcome Mean (Post - Pre)	2.300	0.222	2.300	0.222	2.300	0.222
Test pval:Sow=Harv	0.491	0.192			0.170	0.289
Test pval:All+M1=0			0.293	0.314		
Test pval:All+M3=0			0.333	0.349		
Test pval:M1=M3			0.984	0.034	0.746	0.021

Notes: Table presents variations of the main specification. Columns (1) and (2) regress Equation D.1: "Shift-Share Overlap: Sowing$_\ell$" and "Shift-Share Overlap: Harvest$_\ell$" (denoted $ssoverlap\_sow_\ell$ and $ssoverlap\_harv_\ell$) are measures of shift-share overlap for the sowing and harvest periods, respectively, which add together to equal "Shift-Share Overlap$_\ell$" (as defined in Table 2). Columns (3) and (4) regress Equation D.2: "Shift-Share Overlap: Admission$_\ell$" and "Shift-Share Overlap: Exams$_\ell$" (denoted $ssoverlap\_admis_\ell$ and $ssoverlap\_exams_\ell$) are added to "Shift-Share Overlap$_\ell$" as measures of shift-share overlap between peak farming periods and the first and last month of school, respectively, representing admissions and exam periods, and can be interpreted as interaction terms (i.e., the additional effect of overlap during admissions and exam periods). Columns (5) and (6) regress Equation D.3 in which "Shift-Share Overlap: Admission$_\ell$" and "Shift-Share Overlap: Exams$_\ell$" can also be interpreted as interaction terms since "Shift-Share Overlap: Sowing$_\ell$" and "Shift-Share Overlap: Harvest$_\ell$" sum to "Shift-Share Overlap$_\ell$". All new regressors are normalized to unit variance of "Shift-Share Overlap$_\ell$" to have comparable coefficients. Outcomes, the regressor "Pre-Policy Outcome$_i$" and controls included in the regression defined in Table 2. Conventional robust standard errors in parentheses. Randomization inference p-values in square brackets.

February through July depending on the length of the crop's growth cycle. Comparing the results through this framing only exacerbates sowing overlap's negative effect on schooling: a five-day increase in sowing overlap decreases $Grade_i$ by 0.20—i.e., about one lost grade for every five children.

Second, columns (3) and (4) estimate how the impact of overlap during school's admissions and exam periods (i.e., the first and last four weeks of school, respectively) varies from the remainder of the school year. In column (3), the negative effect of overlap on grade completion remains significant without interacting with the admissions or exam periods and both estimates of admissions-period and exam-period overlap are not statistically significant. However, in column (4), exam-period overlap has a significant positive effect on the household-farm work, significantly more so that admissions-period overlap. Overall, the results build on evidence that overlapping end-of-year examinations with the harvest has detrimental effects (Ito and Shonchoy 2020) by showing negative schooling effects can persist across planting and harvest periods regardless of timing within the school year but also finding evidence exam-period overlap might increase children's work outside of school.

Finally, columns (5) and (6) present results for the full specification. Only minor changes to the point estimates and statistical significance build confidence in the robustness of the prior results. The most notable change is, in column (6), the negative effect of harvest overlap on household-farm work becomes statistically insignificant, while the negative effect or admission-period overlap becomes significant at the 90% confidence level), further supporting the finding that overlap's placement within the school year can have heterogeneous effects on household-farm work.

5.2.3 Adjustments to Farming and Other Child Labor

Next, I test if households made adjustments to the household farm in order to reduce demand for for child labor. I find some small changes to cultivated acres in some crop categories, but no significant change to total cultivated acres. I also find increased expenditures to hired labor and seeds but not enough to affect total farm costs or profits. I also test of households adjusted their children's time allocation to other forms of child labor activities, for which there is no evidence. Note that such adjustments should attenuate the effect of overlap on schooling and farm work and thus do not diminish the relevance of the main results.

Table 7 regresses household-level farm outcomes on a household-averaged version of Equation 4 for all 1,174 households containing sample individuals. Panel A examines adjustments to the household's cultivated acres overall, in the rainy vs. dry season, and for the three most common crops in the sample: maize, groundnut and tobacco. Panel B examines adjustments to farm inputs, revenues and profits. Dependent variables include major cost categories: labor hired from outside the household, expenses on seeds, and expenses on fertilizer, pesticide and herbicide; total farm costs including smaller expenses like land rent, coupon purchases, and transportation; total farm revenue including crop sales, land rent or coupon sales; and total farm profits. All outcomes are winsorized at their 95th percentile due

Table 7: **Shift-Share Overlap Effects on Adjustments to the Household Farm**

PANEL A: Adjustments to Cultivated Acres

VARIABLES	Total Farmed (1)	Rainy Season (2)	Rainy Maize (3)	Rainy Groundnut (4)	Rainy Tobacco (5)	Dry Season (6)
Shift-Share Overlap$_\ell$	0.054	-0.007	0.029	0.055	0.009	0.024
	(0.115)	(0.108)	(0.067)	(0.022)	(0.016)	(0.011)
	[0.579]	[0.937]	[0.674]	[0.092]	[0.705]	[0.012]
Pre-Policy Outcome$_i$	0.531	0.512	0.314	0.315	0.360	0.298
	(0.035)	(0.035)	(0.030)	(0.040)	(0.035)	(0.068)
Observations	1,174	1,174	1,174	1,174	1,174	1,174
R-squared	0.442	0.454	0.344	0.300	0.356	0.163
Pre-Policy Outcome Mean	1.68	1.57	0.96	0.14	0.11	0.02
Δ Outcome Mean (Post - Pre)	0.03	0.01	-0.17	0.01	-0.04	0.02

PANEL B: Adjustments to Farm Inputs

VARIABLES	Cost: Hired Labor (1)	Cost: Seeds (2)	Cost: Other Inputs (3)	Farm Costs (4)	Farm Revenues (5)	Farm Profits (6)
Shift-Share Overlap$_\ell$	772.258	286.425	-837.673	2065.973	4116.866	1397.472
	(449.398)	(204.381)	(1631.147)	(2412.945)	(3933.840)	(3372.570)
	[0.036]	[0.058]	[0.637]	[0.374]	[0.384]	[0.742]
Pre-Policy Outcome$_i$	0.388	0.552	0.975	0.993	1.081	0.654
	(0.086)	(0.084)	(0.093)	(0.086)	(0.110)	(0.092)
Observations	1,174	1,174	1,174	1,174	1,174	1,174
R-squared	0.186	0.139	0.333	0.332	0.329	0.193
Pre-Policy Outcome Mean	1175.0	542.3	4686.7	8051.2	9168.4	2030.6
Δ Outcome Mean (Post - Pre)	1076.4	617.7	5539.0	9107.6	13717	4773.1

Notes: Table presents regressions of household-level farm outcomes on a household-averaged version the main specification for all households containing sample individuals. In Panel A: Outcomes are the household's total cultivated acres for the listed category. In Panel B: Columns (1)-(3) measure different farm costs: labor hired from outside the household in column (1), expenses on seeds in column (2), and expenses on fertilizer, pesticide and herbicide in column (3). Column (4) is total farm costs including other expenses like land rent, coupon purchases, and transportation. Column (5) is total farm revenue including crop sales, land rent or coupon sales. Column (6) is total farm profits: revenue minus costs. All dependent variables are winsorized at their 95th percentile, and farm profits is also winsorized at its 5th percentile due to negative outliers. Regressors "Shift-Share Overlap$_\ell$" and "Pre-Policy Outcome$_i$" and included controls defined in Table 2. Conventional robust standard errors in parentheses. Randomization inference p-values in square brackets.

Table 8: **Adjustments to Other Child Labor**

VARIABLES	Farmed: Dry Season (1)	Tended Livestock (2)	Worked: Day Labor (3)	Worked: Unpaid (4)
Shift-Share Overlap$_\ell$	0.009	0.007	-0.002	-0.010
	(0.018)	(0.018)	(0.023)	(0.018)
	[0.549]	[0.619]	[0.880]	[0.424]
Pre-Policy Outcome$_i$	0.059	0.071	0.117	-0.058
	(0.069)	(0.041)	(0.054)	(0.010)
Observations	2,142	2,142	2,142	2,142
R-squared	0.086	0.037	0.077	0.026
Pre-Policy Outcome Mean	0.01	0.04	0.03	0.01
Δ Outcome Mean (Post - Pre)	0.04	0.03	0.07	0.02

Notes: Table presents regressions of indicators for other forms of child labor on the main specification. All dependent variables are equal to one if the individual worked any amount of the listed activity in the past 12 months, and are zero otherwise: column (1) is household-farm work during the most recent dry-season sowing and harvest; column (2) is tending to household livestock; column (3) is working as a day laborer (i.e., ganyu); and column (4) is working unpaid. Incidence of each activity falls below 5% at baseline and at or below 10% post-policy. Formal work is excluded as it employs less than 0.15% of the sample. Regressor $ssoverlap_\ell$ and included controls defined in Table 2. Conventional robust standard errors in parentheses. Randomization inference p-values in square brackets.

to positive outliers, and farm profits is also winsorized at its 5th percentile due to negative outliers.

The results in Table 7 provide evidence suggesting that households respond to the additional constraint on their children's time by making minor adjustments on the household farm that *substitute away* from child labor during the peak farming periods. In Panel A, a one standard deviation increase in shift-share overlap is not associated with changes to cultivated acres overall, in the rainy season, or to rainy-season maize or tobacco. However, it does cause households to cultivate more groundnuts (significant at the 90% confidence level), despite following a similar planting cycle as maize, and nearly double their acreage of dry season crops. In Table 7 Panel B, a one standard deviation increase in shift-share overlap leads households to purchase significantly more hired labor and seeds—both increases worth over 70% of average household's expenses on these categories at baseline. By comparison, there is no evidence that households reduce their demand for child labor by reducing their cultivated acres. Moreover, these adjustments appear to result in positive but statistically insignificant increase to total farm costs, farm revenues, and also farm profits. That overlap's reductions in child labor do not negatively affect farm profits is an important finding, suggesting that the average household either places so little value of time in school that they sometimes give up schooling for child labor despite its zero returns to farm profits, or that households overestimate their child's productivity versus that of hired labor prior to tighter time constraint enacted by the policy.

Table 8 tests for overlap's effect on whether or not a child was engaged in other forms of child labor in the prior 12 months: dry-season household-farm work, tending to household livestock, working as a day laborer (i.e., ganyu), and working unpaid for another household.[40] No significant effects are identified, suggesting that school-farming calendar

40. Formal work is excluded as it employs less than 0.15% of the sample post-policy.

overlap does not have any observable effects on other forms of child labor.[41]

5.2.4 Long-Run Analyses

To analyze the potential long-run effects of overlap on schooling outcomes, I use data from the 2016 and 2019 wave of Malawi's Integrated Household Panel Survey (IHPS). Unfortunately, due to a reduced target sample size and additional participant attrition, 2016 and 2019 data are only available for 44% and 39% of my original sample, respectively, and retention from the 2010 IHPS is weakly positively correlated with shift-share overlap suggesting slight over-sampling from locations that experienced greater overlap due to the school calendar change. To address both issues, I broaden my sample to include those ages 0-5 pre-policy who become school-aged in later years (ages 7-12 in 2016 and 10-15 in 2019). This sample includes 1,918 and 1,714 individuals in 2016 and 2016, respectively, and retention of the sample that includes this group is slightly less imbalanced at the 90% confidence level in 2016 and producing an RI p-value of 0.141 in 2019.[42] Still, while they should be interpreted with care due to the limitations outlined above, the results suggest that changes to overlap between the school and farming calendars are potentially persistent in the medium-run and may have long-run repercussions as well that warrant future research.

First, I regress the 2016 and 2019 values of highest completed grade level on shift-share overlap using Equation 4. In Table 9, I present the results for three age groups: the full long-run sample ages 0-13 in 2009 (pre-policy); the new cohort who were ages 0-5 in 2009 (pre-policy) and became ages 7-12 in 2016 and 10-15 in 2019; and the main study sample who were ages 6-13 in 2009 and became ages 13-20 in 2016 and 16-23 in 2019. The results suggest that overlap has persistent negative effects on grade completion through at least 2016, while the point estimate in 2019 remains negative but is statistically insignificant. Moreover, the age breakdown suggests that the effects of overlap on household-farm work are persistent for the new cohort of students who were ages 0–5 pre-policy and fully exposed to post-policy school calendar. For this new cohort, I estimate that a five-day increase in overlap during peak production decreases their highest completed grade level by 0.15 grades in 2016 and 0.14 grades in 2019. While these estimates have p-values of 0.13 and 0.20, respectively (possibly due to a much smaller sample size), the point estimates themselves are remarkably consistent with those found in Table 2 and suggest that these effects may likely persist among new students who were exposed after the policy change. Meanwhile, among the main sample ages 6–13 pre-policy, I estimate a significant negative effect in 2016 that translates to a five-day increase in overlap during peak production decreasing their highest completed grade level by 0.27 grades, which is almost twice the magnitude of the estimate in Table 2; however, the effect is statistically insignificant in 2019.

In sub-Appendix G.2, I show these results for the other primary outcome $Farmed_i$. For $Farmed_i$, the age breakdown suggests that the effects of overlap on household-farm work are persistent for the new cohort of students who

41. Note that incidence of all activities falls below 5% at baseline and at or below 10% post-policy, so the lack of 12-month recall data on common alternative activities may be a limitation of the analysis.
42. Appendix section G.1 provides more details on the 2016 and 2019 IHPS dataset.

Table 9: **Long-Run Impacts on Grade Level**

VARIABLE: Grade in	Full Long-Run Sample Pre-Policy Age 0-13 2016 (1)	 2019 (2)	New Cohort Pre-Policy Age 0-5 2016 (3)	 2019 (4)	Children in Study Sample Pre-Policy Age 6-13 2016 (5)	 2019 (6)
Shift-Share Overlap$_\ell$	-0.414	-0.141	-0.239	-0.234	-0.457	0.253
	(0.179)	(0.249)	(0.169)	(0.245)	(0.310)	(0.419)
	[0.005]	[0.521]	[0.130]	[0.204]	[0.013]	[0.539]
Pre-Policy Outcome$_i$	0.755	0.844	0.370	0.653	0.884	1.181
	(0.053)	(0.077)	(0.236)	(0.159)	(0.069)	(0.109)
Observations	1,918	1,714	929	834	989	880
R-squared	0.729	0.605	0.484	0.485	0.579	0.494
Age at Outcome	Age 7-20	Age 10-23	Age 7-12	Age 10-15	Age 13-20	Age 16-23
Pre-Policy Outcome Mean	0.65	0.62	0.01	0.00	1.25	1.20
Δ Outcome Mean (Post - Pre)	2.87	4.50	1.50	3.25	4.16	5.68

Notes: Specification and variables are as defined in Table 2. Dependent variable $Grade_i$ was measured for a subset of individuals in follow-up panel surveys in either 2016 or 2019. Conventional robust standard errors in parentheses. Randomization inference p-values in square brackets.

were ages 0–5 pre-policy and fully exposed to post-policy school calendar. Meanwhile, I estimate null effects on household-farm work among the main study sample. Age is a likely explanation: those ages 0–5 pre-policy are only ages 10–15 in 2019 and thus still likely to be enrolled in school, while the main study sample are ages 16–23 in 2019 and are less likely to be enrolled in school. After children are finished with school, it no longer competes for their time allocation to household-farm work.

6 Policy Analysis

How potentially harmful to educational attainment was the increased overlap between school and farming calendars induced by Malawi's school calendar shift? I estimate that the policy increased shift-share overlap by 1.8 standard deviations for the average community in the sample. Hence, I estimate that the policy reduced schooling after four years by an average of 0.42 grades per youth in my sample. So while there are potentially beneficial reasons for changing the school calendar, this paper's results suggest that the policy potentially had detrimental effects as well.[43] In this section, I aim to address the next question a policymaker might ask: "What school calendar then is best for minimizing overlap with the farming calendar?" First, I simulate alternative school calendars and identify those that are most likely to minimize average overlap nationwide. Then, I project gains of switching to a nationwide overlap-minimizing calendar and compare school calendar reform to other types of educational interventions.

43. To be clear, I do not assign blame on the Government of Malawi (GoM) for two reasons: 1) these results were obviously unknown; and 2) there are other potentially beneficial reasons for realignment of a school calendar, which are not captured in this analysis. Rather, I commend the GoM for their ongoing support of household data collection that make this research possible.

6.1 Simulating Alternative School Calendars

To identify Malawi's overlap-minimizing school calendar, I simulate other potential school calendars and estimate their effects on shift-share overlap and grade advancement. The simulation maintains the same structure as the original post-policy school year but starts school on Monday in a different week in the year, making 52 potential school calendars. The simulation's main findings are summarized in Figure 3, which depicts the simulated policy impacts of alternative 2011 school calendars relative to the actual 2009 school calendar for the average sample community. The 52 simulated calendars are denoted by the week in which they start, where week 1 begins the first week of January and week 52 begins in the last week of December. Each calendar's counterfactual change in shift-share overlap (measured in standard deviations of $ssoverlap_\ell$) is measured using drop lines via the left vertical scale, while the projected effects on $Grade_i$ (approximated as a linear extrapolation of the primary results) are measured using bars via the right vertical scale.[44]

In Panel (a), Figure 3 shows the counterfactual measure of $ssoverlap_\ell$ and the corresponding projected effects on $Grade_i$ given results in Table 2. The simulation reveals that a Malawian school calendar starting in January is typically best to minimize overlap with the farming calendar. Assuming the structure of the 2011 calendar, a start date of January 10, 2011 (Week 2) would have minimized overlap. Overall, this suggests that Malawi's pre-policy school calendar was ideally situated.[45] Indeed, assuming the structure of the 2009 calendar, a start date of Monday, December 29, 2008 would have minimized overlap, though the actual start date of January 3, 2009 (one week later) was second best. Moreover, the post-policy 2011 school calendar that started in September was far from the worst calendar that could have been chosen. In terms of maximizing overlap with the farming calendar, March and October would be the worst times to start the school calendar, increasing shift-share overlap by an additional 78% and 63% (Week 13 and 40, respectively) more than the actual 2011 school calendar did.

In Panel (b), Figure 3 shows counterfactual measures of $ssoverlap\_sow_\ell$ and $ssoverlap\_harv_\ell$ and the aggregated projected effects on $Grade_i$, which was calculated by first estimating separate sowing and harvest effects given results from Table 6, column (1), and then summing them together. The simulation reveals that, when it comes to minimizing overlap's harmful effect on grade level, there are more gains by minimizing school calendar overlap with the sowing period relative to the harvest period.[46] Then, to minimize sowing overlap, policymakers should attempt to align school's end-of-year break (typically 1–2 months long) with the sowing season. For school calendars starting in January, the end-of-year break falls during November and December—the designated sowing season for most rainy

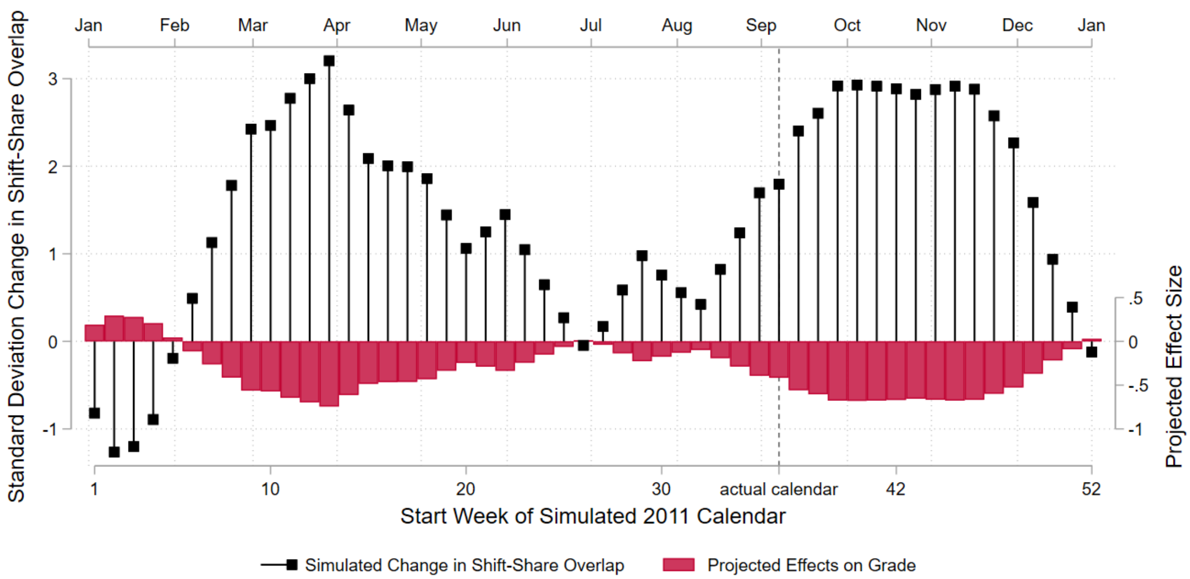
44. Additional details on the simulation and projected effects on $Farmed_i$ are located in Appendix C.2.

45. Given this, one might have expected the simulated change in shift-share overlap to be near zero in early January instead of negative, as seen here. This is primarily because the 2011 school year had five fewer days of school (one less week) than the 2009 school year, which is just over a one standard deviation change in shift-share overlap if occurring during a peak farming period.

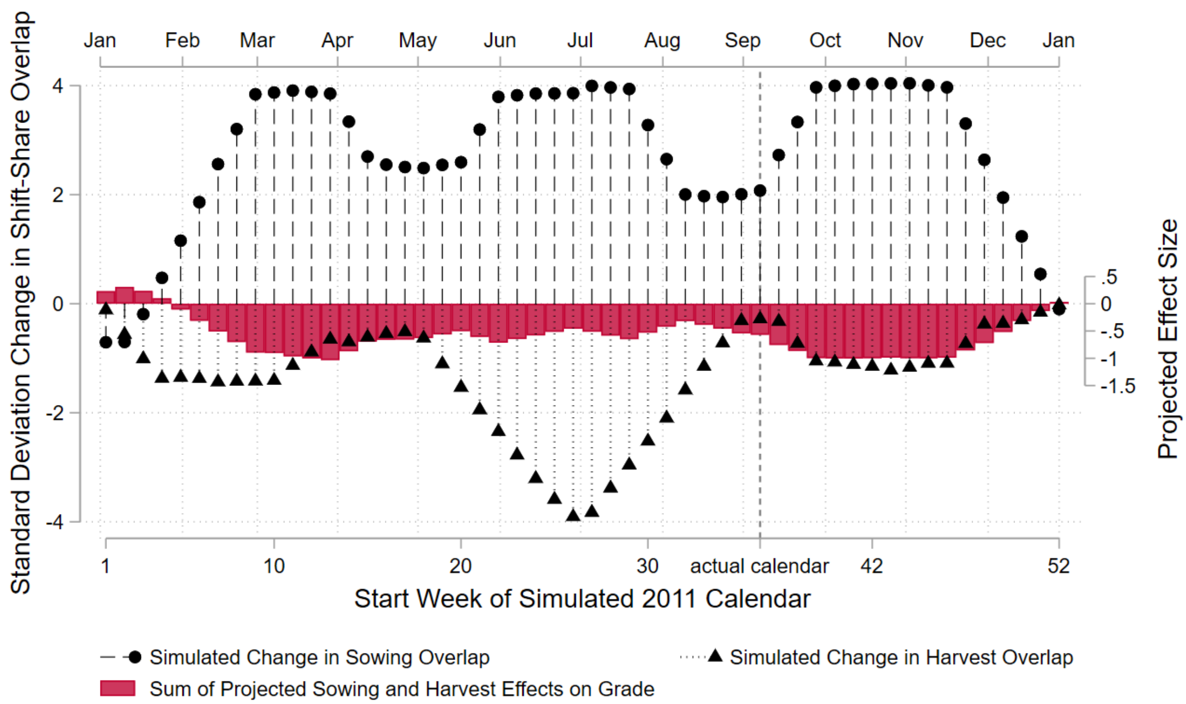
46. Note that for simulated post-policy school calendars beginning in early January—the start date for the "ideal school calendar"—the simulated change for sowing overlap is at its minimum but harvest overlap is near its maximum, and the projected effect size on $Grade_i$ is positive. On the contrary, in early July when the simulated change for sowing overlap is at its maximum and harvest overlap is at its minimum, the projected effect size on $Grade_i$ is still solidly negative.

Figure 3: **Simulated Policy Impacts of Alternative 2011 School Calendars**

(a) Overall Impact



(b) Sowing versus Harvest Impact



Notes: Figure depicts simulated policy impacts of alternative 2011 school calendars relative to the actual 2009 school calendar. Panel (a) plots the expected change in shift-share overlap for the average sample community using drop lines and the projected effects on $Grade_i$ using bars. Panel (b) plots these outcomes separately for both the sowing and harvest periods. Additionally, the top horizontal axis estimates the start of each month, and the vertical line denotes the actual 2011 school calendar that started on September 6, 2010.

season crops. Thus, school calendars starting in January free up students during their end-of-year break for the most concentrated period of peak labor demand. Overlap with the harvest period is also important, but harvest dates for rainy season crops are spread out from April through July, so labor demand is less concentrated.

Furthermore, deeper analysis of the simulation shows that the ideal school calendar varies by community because crop bundles (and hence the "farming calendar") vary by community. For the 2011 school calendar, while a January 10th start date would have minimized overlap across all communities *on average*, it actually turns out that the optimal start date is January 17th for 44% of communities, January 10th for 29% of communities, and other start dates for the remaining 27%. This suggests that while there may be a single optimal school calendar for Malawi on average, there are potential gains to granting communities some flexibility in setting the school calendar. The simulation reveals that shift-share overlap improves (falls) by an additional 12% when communities adopt their own overlap-minimizing school calendar rather than the one calendar that minimizes overlap across all communities on average.

6.2 Projected Gains and Comparison to Educational Interventions

The projected gains of a nationwide school calendar change are sizable, especially considering that this paper estimates *average* benefits that should apply to all students enrolled in public school. If the Government of Malawi chose to revert back to its pre-policy school calendar (as recommended by the simulation), then the potential policy impacts would be the inverse of the negative effects measured in this paper. With the policy increasing shift-share overlap by 1.8 standard deviations, implementing a nationwide overlap-minimizing school calendar is estimated to *increase* school advancement by 0.42 grades after four years for the average primary-school-aged child.[47] Given that Malawi had an estimated 4.9 million primary school students in the 2021/22 school year (Government of Malawi 2022), this beneficial effect could translate into an additional 2.05 million years of schooling (i.e., primary-school-aged children passing an additional grade that they would not have otherwise) in a four-year period.

However, the costs of school calendar reform are difficult to estimate, even to development practitioners actively advocating for SSA school calendar reforms (Alban Conto 2024; Music 2024). First, to the best of my knowledge, there is no record of an itemized budget from a former or planned nationwide SSA school calendar reform, likely because past SSA school calendar changes are relatively rare and, if implemented, were likely funded from larger government budgets without itemized record-keeping to separate necessary vs discretionary expenditures. Second, school calendar reforms may have indirect costs by increasing school participation. While results described in Section 5.1.1 do not find evidence of impacts on school enrollment, accelerated grade advancement could increase the class size of later grades causing negative congestion externalities. Thus cost estimates might also incorporate the cost of hiring additional teachers and supplies to offset increases in school participation in upper primary school.[48] Third, current

47. The calculation is: the effect of a one standard deviation increase in shift-share overlap from Table 2 column (1) of -0.232 grades · -1.8 standard deviation change in shift-share overlap from reversing the policy = 0.42 grades.

48. For example, Miguel and Kremer (2004) Section 8.3 estimates the cost of additional teachers to address positive school participation effects

efforts to reform school calendars in SSA are nascent and are being implemented at a sub-national level (UNESCO 2024), which means cost estimates are still uncertain and may come with additional costs due to decentralization. Therefore, in the exercise below, I will compare the projected impacts of school calendar reform to other educational interventions under different cost scenarios.

How might the impacts of a policy change to an overlap-minimizing school calendar compare with impacts from other educational interventions? I compare the projected effects of 2.05 million additional years of schooling with results from 14 interventions analyzed in Evans and Yuan (2019) in terms of equivalent years of schooling (EYOS) per \$100 spent by backing out how much a school calendar reform must cost to obtain a certain level of cost-effectiveness.[49]. In their analysis, the median intervention generated 7.55 EYOS per \$100, then 67.6 EYOS per \$100 at the 75th percentile, 165.9 EYOS per \$100 at the 90th percentile and 370.4 EYOS per \$100 at the maximum (Nguyen (2008)'s intervention that provided earnings information in Madagascar). To match an equivalent cost effectiveness to the median, 75th percentile, 90th percentile or maximum intervention in the Evans and Yuan (2019)'s comparison, the school calendar reform would need to cost approximately \$27.2 million, \$3.0 million, \$1.2 million, or \$550,000, respectively. So even at quite high implementation costs of around \$10 million, a school calendar reform remains more cost effective at increasing years of schooling than most of the other educational interventions analyzed by Evans and Yuan (2019).

6.3 Projected Impacts on Learning

Recent attention has been given to measuring the impact of educational interventions on learning rather than just attendance or years of schooling (Pritchett 2013; World Bank 2017). In lieu of a direct estimate of the effects of overlap on student learning and skill acquisition, which are not available in the data, I extrapolate the results on $Grade_i$ to project that a five-day increase in overlap during peak production can reduce the children's literacy skills in adulthood by between 0.02–0.04 standard deviations and, moreover, that school calendar reform in Malawi has potential to improve children's literacy in adulthood by between 0.07–0.11 standard deviations. While speculative, these cost-effectiveness comparisons suggest significant potential for school calendar change policies, in Malawi and elsewhere, to improve educational outcomes because their ability to be implemented at large scale.

The calculations for these estimates are detailed in Appendix F.3. In brief, I invert the Evans and Yuan (2019) method to restate my primary results in terms of equivalent changes in learning (whereas they convert learning impacts into equivalent years of schooling). Using baseline data for a sample of adults aged 25-64 who have 1-12 years of schooling, I use a simple regression to find that an additional year of schooling is associated with a 0.17 standard deviation gain in literacy while one additional year of just primary school is associated with a 0.27 standard deviation

in the context of a deworming intervention in Kenya.

49. The formula to estimate the cost-effectiveness per \$100 = total effect x 100 / total cost. Thus, to back out total cost for a given cost-effectiveness, the formula is: total cost = total effect x 100 / cost-effectiveness per \$100.

gain in literacy.[50] I then multiply -0.14—the negative effect on $Grade_i$ of a five-day increase in overlap during peak production—by these "conversion rates" of 0.17 and 0.27 to estimate losses in literacy between 0.02–0.04 standard deviations. Conversely, if implementing the nationwide overlap-minimizing school calendar improves school advancement for the average child by 0.42 grades, then it has potential to improve children's literacy in adulthood by between 0.07–0.11 standard deviations, placing it alongside other successful education interventions. While promising, such impacts on literacy will be highly dependent on the quality of education that children receive. Ultimately, testing of school calendar reform on learning outcomes are left to future research.

6.4 Implications for Sub-Saharan Africa

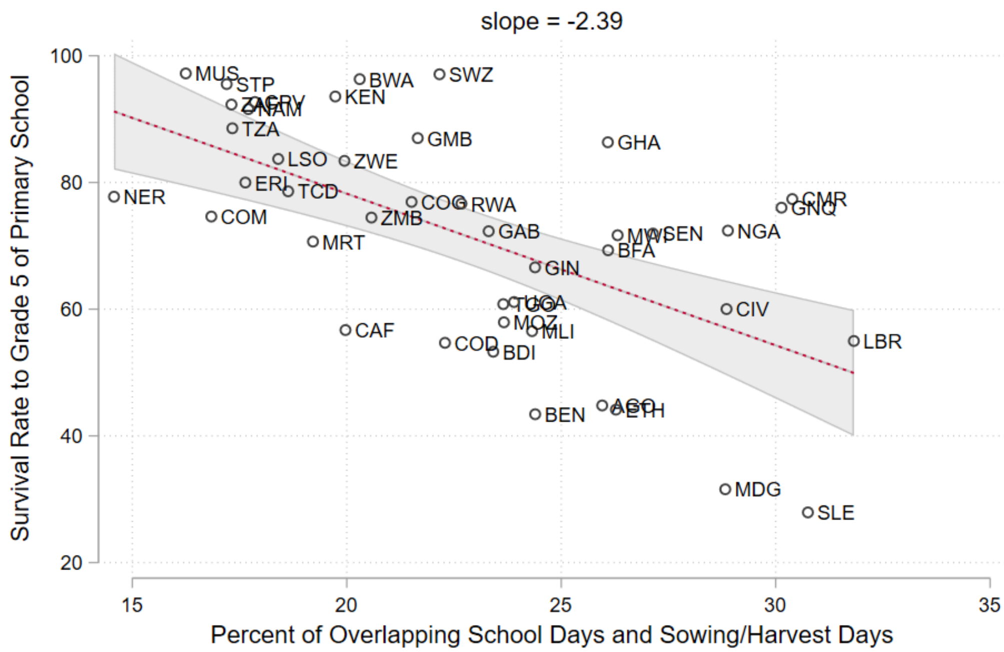
To show that these findings are relevant beyond Malawi, I collected data on school and farming calendars for sub-Saharan African (SSA) countries to estimate if how they are correlated with primary school advancement, as described in Appendix H. First, looking across SSA countries, Figure 4 presents some correlational evidence that overlap between the school and farming calendars may impede primary school completion. The figure shows a significant negative correlation between a country's rate of reaching the fifth grade (i.e., "survival rate", on the vertical axis) and the percent of school and sowing/harvest days that overlap (on the horizontal axis). For every additional percentage point increase in overlap, the survival rate is 2.39 percentage points lower, on average. With overlap's percent of the school and farming calendars ranging from 15% to 32% across SSA countries, overlap may help to explain large differences in advancement within primary school. Furthermore, I find a similar marginally significant negative correlation with a country's adult literacy rate (shown in Appendix H), suggesting that overlap in the school and farming calendar may also have impacts on human capital accumulation that persist into adulthood.

For educational policymakers in sub-Saharan Africa (SSA) and similar settings, the primary results provide evidence of a clear causal link between policies that constrain the time available for schooling and household-farm work and reductions in time spent on these activities. Additionally, the policy simulations suggest scheduling school outside the labor-intensive sowing and harvest periods if they wish to foster more school participation (while also factoring in the net effects of additional household-farm work on child welfare). Given this, one approach is to shift the start of the existing school calendar to best align school breaks, especially between end-of-year exams and the start of the next school year, with periods of peak farm labor demand like the sowing period.[51] Another approach is to allow for more flexibility and adaptation in the school calendar at the local level—for example, by allowing local school boards to declare up to two weeks of school holidays that can be made up at the end of the year. Analysis of such decentralized school scheduling policies is left as an ambition for future research.

50. This correlation is robust to adding individual-level controls for age, age-squared and sex. These estimates are consistent with Evans and Yuan (2019)'s own analysis using household survey data with detailed literacy assessments, which finds one additional year of schooling in grades 1–12 is associated with between 0.15 and 0.21 standard deviation gain in literacy across five countries.

51. Take caution not to accidentally align end-of-year exams with peak farming periods, as studied in Ito and Shonchoy (2020).

Figure 4: **Primary School Completion and Overlap between School and Farming Calendars in Sub-Saharan Africa**



Notes: Figure depicts the correlation with the linear fit (and 95% confidence interval) between primary school completion and overlap between the school and farming calendars in 43 sub-Saharan African countries, labeled by their ISO alpha 3 code. The y-axis shows a country's rate of reaching the fifth grade of primary school (i.e., known as the "Survival Rate to Grade 5", on the vertical axis) for both sexes, as reported in the most recent year available by UNESCO's Institute for Statistics (UIS). The x-axis shows the overlapping percent of total days in the country's school calendar and total days in the sowing and harvesting periods, as estimated from countries' official primary school calendars (in most cases) and country-level crop calendars from the Food and Agriculture Organization (FAO).

7 Conclusion

This paper analyzes a plausibly exogenous change to overlap between the school and farming calendars in Malawi that constrained total time available for schooling and household-farm work. Starting in 2009, Malawi shifted its school calendar by four months, effectively moving school days to a time of higher farm labor demand. Using household panel data from 2009/10 and 2013, I estimate the policy's impact by comparing outcomes between school-aged youth differentially exposed to the shock based on their community's pre-policy crop allocation via a shift-share estimation strategy and frontier randomization inference procedure. I find that, as predicted by theory, overlap between the school and farming calendars reduces both schooling and incidence of child labor on the household farm during peak production periods. Also, I find that the negative schooling effects are likely driven by reductions in school participation along the intensive margin and are more pronounced during the labor-intensive sowing period. Furthermore, overlap also result in small changes to household-farm production decisions but not farm profits or other types of child labor.

This paper makes several contributions to the study of household decision-making in low-income settings. First, I estimate households' trade-off between schooling investments and child labor by analyzing a natural experiment that shocked a child's time allocation to both activities while keeping the nominal returns to each activity fixed. Second, this paper uniquely identifies and analyzes the impact of a time constraint shock, which may be yet another binding constraint that poor households face in some developing settings. Third, the findings suggest that the school calendar itself may be an effective policy tool for increasing time in school in sub-Saharan Africa by adapting the school calendar to minimize overlap with peak farming periods, as I illustrate with a policy simulation. Rather than conceptualizing the time trade-off between schooling and child labor as a zero-sum game, policymakers have the ability via the school calendar to alleviate the constraint on total time available to both productive activities. Overall, policymakers aiming to increase rural school participation should do more to accommodate farm labor demand.

References

Acemoglu, Daron, Simon Johnson, and James A Robinson. 2001. "The colonial origins of comparative development: An empirical investigation." *American economic review* 91 (5): 1369–1401.

Adhvaryu, Achyuta R, and Anant Nyshadham. 2012. "Schooling, child labor, and the returns to healthcare in Tanzania." *Journal of Human resources* 47 (2): 364–396.

Admassie, Assefa. 2003. "Child labour and schooling in the context of a subsistence rural economy: can they be compatible?" *International Journal of Educational Development* 23 (2): 167–185.

Adão, Rodrigo, Michal Kolesár, and Eduardo Morales. 2019. "Shift-share designs: Theory and inference." *The Quarterly Journal of Economics* 134 (4): 1949–2010.

Alban Conto, Carolina. 2024. Personal Correspondence. Research and Development Program Manager, UNESCO-IIEP (International Institute for Educational Planning).

Atkin, David. 2016. "Endogenous Skill Acquisition and Export Manufacturing in Mexico." *American Economic Review* 106 (8): 2046–85. https://doi.org/10.3386/w18266.

Autor, David H., David Dorn, and Gordon H. Hanson. 2013. "The China Syndrome: Local Labor Market Effects of Import Competition in the United States." *American Economic Review* 103 (6): 2121–68. https://doi.org/10.1257/aer.103.6.2121. https://www.aeaweb.org/articles?id=10.1257/aer.103.6.2121.

Bai, Jie, and Yukun Wang. 2020. "Returns to work, child labor and schooling: The income vs. price effects." *Journal of Development Economics* 145:102466.

Bartik, Timothy J. 1991. "Who benefits from state and local economic development policies?"

Basu, Karna, and Maisy Wong. 2015. "Evaluating seasonal food storage and credit programs in east Indonesia." *Journal of Development Economics* 115:200–216.

Bau, Natalie, Martin Rotemberg, Manisha Shah, and Bryce Steinberg. 2023. *Human capital investment in the presence of child labor.* Technical report. National Bureau of Economic Research.

Becker, Gary S. 1962. "Investment in Human Capital: A Theoretical Analysis." *Journal of Political Economy* 70 (5, Part 2): 9–49. https://doi.org/10.1086/258724. eprint: https://doi.org/10.1086/258724. https://doi.org/10.1086/258724.

Beegle, Kathleen, Rajeev H Dehejia, and Roberta Gatti. 2006. "Child labor and agricultural shocks." *Journal of Development economics* 81 (1): 80–96.

Blanchard, Olivier Jean, Lawrence F Katz, Robert E Hall, and Barry Eichengreen. 1992. "Regional evolutions." *Brookings papers on economic activity* 1992 (1): 1–75.

Bolt, Jutta, and Dirk Bezemer. 2009. "Understanding long-run African growth: colonial institutions or colonial education?" *The Journal of Development Studies* 45 (1): 24–54.

Borusyak, Kirill, and Peter Hull. 2021. "Non-Random Exposure to Exogenous Shocks: Theory and Applications," Working Paper Series, no. 27845, https://doi.org/10.3386/w27845. http://www.nber.org/papers/w27845.

Borusyak, Kirill, Peter Hull, and Xavier Jaravel. 2022. "Quasi-experimental shift-share research designs." *The Review of Economic Studies* 89 (1): 181–213.

Burke, Marshall, Lauren Falcao Bergquist, and Edward Miguel. 2019. "Sell low and buy high: arbitrage and local price effects in Kenyan markets." *The Quarterly Journal of Economics* 134 (2): 785–842.

Bustillo, Ines. 1989. *Female Educational Attainment in Latin America : A Survey.* Washington, DC: World Bank.

Cattan, Sarah, Daniel A Kamhöfer, Martin Karlsson, and Therese Nilsson. 2023. "The long-term effects of student absence: Evidence from Sweden." *The Economic Journal* 133 (650): 888–903.

Cunha, Flavio, and James Heckman. 2007. "The technology of skill formation." *American economic review* 97 (2): 31–47.

de Janvry, Alain, Claire Duquennois, and Elisabeth Sadoulet. 2022. "Labor calendars and rural poverty: A case study for Malawi." *Food Policy* 109:102255.

Denning, Glenn, Patrick Kabambe, Pedro Sanchez, Alia Malik, Rafael Flor, Rebbie Harawa, Phelire Nkhoma, Colleen Zamba, Clement Banda, Chrispin Magombo, et al. 2009. "Input subsidies to improve smallholder maize productivity in Malawi: Toward an African green revolution." *PLoS biology* 7 (1): e1000023.

Dillon, Brian. 2021. "Selling crops early to pay for school A large-scale natural experiment in Malawi." *Journal of Human Resources* 56 (4): 1296–1325.

Dizon-Ross, Rebecca. 2019. "Parents' beliefs about their children's academic ability: Implications for educational investments." *American Economic Review* 109 (8): 2728–65.

Duflo, Esther. 2001. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment." *American Economic Review* 91 (4): 795–813.

Edmonds, Eric V. 2007. "Child labor." *Handbook of Development Economics* 4:3607–3709.

Edmonds, Eric V, and Maheshwor Shrestha. 2014. "You get what you pay for: Schooling incentives and child labor." *Journal of development economics* 111:196–211.

Evans, David, and Fei Yuan. 2019. "Equivalent years of schooling: A metric to communicate learning gains in concrete terms." *World Bank Policy Research Working Paper,* no. 8752.

Famine Early Warning Systems Network. 2013. *Malawi: Food Security Outlook.* Technical report. Distributed by Famine Early Warning System Network. Accessed January 25, 2024. https://fews.net/southern-africa/malawi/food-security-outlook/october-2013.

Fink, Gunther, B Kelsey Jack, and Felix Masiye. 2020. "Seasonal liquidity, rural labor markets, and agricultural production." *American Economic Review* 110 (11): 3351–92.

Fisher, Ronald A. 1935. "The logic of inductive inference." *Journal of the royal statistical society* 98 (1): 39–82.

Frye, Margaret. 2011. *Malawi School Calendar Change 2009-2001, Memo for the Tsogolo La Thanzi Centre.* Technical report.

Gibson, Matthew, and Jeffrey Shrader. 2018. "Time use and labor productivity: The returns to sleep." *Review of Economics and Statistics* 100 (5): 783–798.

Glaeser, Edward L, Rafael La Porta, Florencio Lopez-de Silanes, and Andrei Shleifer. 2004. "Do institutions cause growth?" *Journal of economic Growth* 9:271–303.

Goldsmith-Pinkham, Paul, Isaac Sorkin, and Henry Swift. 2020. "Bartik instruments: What, when, why, and how." *American Economic Review* 110 (8): 2586–2624.

Goodman, Joshua. 2014. *Flaking out: Student absences and snow days as disruptions of instructional time.* Technical report. National Bureau of Economic Research.

Government of Malawi. 2000. *Employment Act No. 6 of 2000.* Technical report. Malawi Government.

Government of Malawi. 2022. *Malawi Education Statistics Report.* Technical report. Malawi Government, Ministry of Education.

Government of Malawi, National Statistical Office. 2013. *Integrated Household Panel Survey 2010-2013.* Technical report. https://doi.org/10.48529/q5q1-2a34.

Ito, Seiro, and Abu Shonchoy. 2020. *Seasonality, Academic Calendar and School Drop-outs in Developing Countries.* Working Papers 2013. Florida International University, Department of Economics. https://EconPapers.repec.org/RePEc:fiu:wpaper:2013.

Jensen, Robert. 2010. "The (perceived) returns to education and the demand for schooling." *The Quarterly Journal of Economics* 125 (2): 515–548.

Jensen, Robert, and Nolan H Miller. 2017. *Keepin' 'em Down on the Farm: Migration and Strategic Investment in Children's Schooling.* Working Paper, Working Paper Series 23122. National Bureau of Economic Research. https://doi.org/10.3386/w23122. http://www.nber.org/papers/w23122.

Kadzamira, Esme, and Pauline Rose. 2003. "Can free Primary Education meet the Needs of the Poor?: Evidence from Malawi." *International Journal of Educational Development* 23 (5): 501–516. ISSN: 0738-0593. https://doi.org/https://doi.org/10.1016/S0738-0593(03)00026-9. https://www.sciencedirect.com/science/article/pii/S0738059303000269.

Kattan, Raja Bentaouet, and Nicholas Burnett. 2004. "User Fees in Primary Education." *World Bank Education Advisory Service.*

Kazianga, Harounan, Damien De Walque, and Harold Alderman. 2012. "Educational and child labour impacts of two food-for-education schemes: Evidence from a randomised trial in rural Burkina Faso." *Journal of African Economies* 21 (5): 723–760.

Lam, David, Cally Ardington, and Murray Leibbrandt. 2011. "Schooling as a lottery: Racial differences in school advancement in urban South Africa." *Journal of development economics* 95 (2): 121–136.

Mankiw, N Gregory, David Romer, and David N Weil. 1992. "A contribution to the empirics of economic growth." *The Quarterly Journal of Economics* 107 (2): 407–437.

Merfeld, Joshua D. 2023. "Does the timing of the school year affect child labor decisions in developing countries?"

Miguel, Edward, and Michael Kremer. 2004. "Worms: identifying impacts on education and health in the presence of treatment externalities." *Econometrica* 72 (1): 159–217.

Montenegro, Claudio E, and Harry A Patrinos. 2014. "Comparable estimates of returns to schooling around the world." *World Bank policy research working paper,* no. 7020.

Montero, Eduardo, and Dean Yang. 2022. "Religious Festivals and Economic Development: Evidence from the Timing of Mexican Saint Day Festivals." *American Economic Review.*

Music, Almedina. 2024. Personal Correspondence. Senior Economist, Education Global Practice in the Eastern and Southern Africa Region, World Bank.

Nguyen, Trang. 2008. "Information, Role Models and Perceived Returns to Education: Experimental Evidence from Madagascar."

Oviedo, Ana María, and Francis Muamba Mulangu. 2024. *Fighting poverty with education: Why school reforms are urgently needed in Madagascar.* World Bank Nasikiliza Blog. World Bank. Accessed June 12, 2024. https://blogs.worldbank.org/en/nasikiliza/fighting-poverty-education-why-school-reforms-are-urgently-needed-madagascar-afe-0324.

Pritchett, Lant. 2013. *The Rebirth of Education: Schooling Ain't Learning.* CGD Books.

Psacharopoulos, George, and Harry Anthony Patrinos. 2018. "Returns to Investment in Education: a Decennial Review of the Global Literature." *Education Economics* 26 (5): 445–458.

Ravallion, Martin, and Quentin Wodon. 2000. "Does child labour displace schooling? Evidence on behavioural responses to an enrollment subsidy." *The economic journal* 110 (462): 158–175.

Santos, Rafael J. 2014. "Not All that Glitters is Gold: Gold Boom, Child Labor and Schooling in Colombia." *SSRN, Documento CEDE No. 2014-31,* https://ssrn.com/abstract=2489203orhttp://dx.doi.org/10.2139/ssrn.2489203.

Schiefelbein, Ernesto. 1987. "Education Costs and Financing Policies in Latin America." *World Bank, Education and Training Department Discussion Paper EDT60* (January). https://documents1.worldbank.org/curated/en/975791468769506309/pdf/multi-page.pdf.

Shah, Manisha, and Bryce Millett Steinberg. 2017. "Drought of Opportunities: Contemporaneous and Long-Term Impacts of Rainfall Shocks on Human Capital." *Journal of Political Economy* 125 (2): 527–561. https://doi.org/10.1086/690828. eprint: https://doi.org/10.1086/690828. https://doi.org/10.1086/690828.

UIS and UNICEF. 2015. *Fixing the Broken Promise of Education for All: Findings from the Global Initiative on Out-of-School Children.* Technical report. https://doi.org/http://dx.doi.org/10.15220/978-92-9189-161-0-en.

UNESCO, International Institute for Educational Planning. 2024. *Rethinking national school calendars for climate-resilient learning.* Website. Accessed June 11, 2024. https://www.iiep.unesco.org/en/our-expertise/rethinking-national-school-calendars-climate-resilient-learning.

UNESCO, Institute for Statistics. 2022.

Watkins, Kevin. 2000. *The Oxfam Education Report.* Technical report. Oxfam.

World Bank. 2021. *Employment in Agriculture (% of Total Employment)(modeled ILO estimate).* Technical report.

———. 2010. *The Education System in Malawi.* The World Bank.

———. 2017. *World Development Report 2018: Learning to Realize Education's Promise.* The World Bank.

Yang, Dean, James Allen IV, Arlete Mahumane, James Riddell IV, and Hang Yu. 2021. *Knowledge, Stigma, and HIV Testing: An Analysis of a Widespread HIV/AIDS Program.* Technical report. National Bureau of Economic Research.

Online Appendix

For online publication: This is the online appendix for "Double-Booked: Effects of Overlap between School and Farming Calendars on Education and Child Labor".

An outline of this Appendix: Section A presents a household model of how overlapping school and farming calendars affect children's allocations to both time in school and farming. Section B provides additional details of the data cleaning process used for primary outcomes and crop calendars. Section C provides additional details on the paper's simulations. Section D shows the period-specific regression specification. Section E details alternative inference procedures used in Table A.6. Section F provides details of back-of-the-envelope calculations made in the Sections 5.1.2, 6.2 and 6.3 of the main text. Finally, Section H details the sub-Saharan Africa cross-sectional analysis.

A Theoretical Model

In this appendix, I present a simple household model and describe under what conditions an increase in overlap (holding fixed the length of each calendar) could be expected to decrease allocations to both time in school and farming.

A.1 Overlap in a Household Model

Consider a household consisting of *adults* and a *child*.

Child's Time Endowments. A child has a total time endowment T which is spent on schooling s, farm work h, and leisure ℓ such that:

$$s+h+\ell=T \tag{A.1}$$

The school year has $S\in(0,T)$ days and the farming season has $H\in(0,T)$ days. There is also an overlap between school and farming days captured by Θ: the number of days in which school is scheduled during the farming season. When the school year and the farming season overlap, a child can either attend school or farm, but not both. Therefore, an "overlap constraint" limits the amount of available time that a child can spend on either school or farm work, as follows:

$$s+h\leq S+H-\Theta \tag{A.2}$$

The following also holds:

$$\begin{aligned} s &\leq S \\ h &\leq H \\ \Theta &\leq min(S,H) \\ S+H-\Theta &< T \end{aligned} \tag{A.3}$$

The first two conditions specify that the child will attend at most S days of school and spend at most H days on the farm (though not both given Equation A.2 unless $\Theta=0$). Further, overlap Θ in school and farming days can be no more than the minimum value between S and H. Finally, the total number of school and farming days accounting for overlap is less than total time T, meaning I assume that there are days in which leisure ℓ is the only available activity.

Household Utility. Household income consists of adult income n and child income $w\cdot h$ where w is the child's wage. The household will spend income on consumption c, where:

$$c \leq w \cdot h + n \tag{A.4}$$

The household maximizes a utility function which is additive in the utility from consumption, schooling and child leisure, respectively:

$$U(c,s,l) = \underbrace{u_C(c)}_{\substack{\text{consumption} \\ \text{utility}}} + \underbrace{u_S(s)}_{\substack{\text{schooling} \\ \text{utility}}} + \underbrace{u_L(\ell)}_{\substack{\text{leisure} \\ \text{utility}}} \tag{A.5}$$

Each of the three sub-utilities is monotonic, concave and has infinite derivative at 0, which ensures an interior solution for the household's maximization problem.

I make the following two assumptions to simplify the analysis, and then discuss the implications of relaxing each assumption following the main theoretical result:

Assumption 1 *The household always prefers schooling and farming to leisure at the minimum allocation to leisure:*

$$\begin{aligned} u'_S(S) &> u'_L(T-S-H+\Theta) \\ wu'_C(w \cdot H+n) &> u'_L(T-S-H+\Theta) \end{aligned} \tag{A.6}$$

The maximum amount of time the child can spend in school and on the farm is $S+H-\Theta$. Therefore, $T-S-H+\Theta$ is time that a child must spend on leisure ℓ. Assumption 1 says that at this minimum level of leisure, the household prefers the child to spend additional time either in school or on the farm (earning wage income) rather than on even more leisure, and thus $s+h = S+H-\Theta$. This is a reasonable assumption because, in many contexts, the minimum level of leisure is still a significant amount of time. Given Assumption 1, the only conflict in time allocation arises between schooling and farm labor: the household will allocate $S+H-\Theta$ days to farming and schooling and the remaining $T-S-H+\Theta$ to leisure (which cannot be further reduced).

Assumption 2 *The household always prefers some positive level of both schooling and farming:*

$$\begin{aligned} u'_S(S) &< w \cdot u'_C(w(H-\Theta)+n) \\ w \cdot u'_C(w \cdot H+n) &< u'_S(S-\Theta) \end{aligned} \tag{A.7}$$

Given that households allocate $S+H-\Theta$ days to farming and schooling (Assumption 1), a household can either choose to maximize school by devoting S to schooling and $H-\Theta$ to farm labor; maximize farm work by devoting H to farm work and $S-\Theta$ to schooling; or somewhere in-between by allocating $s < S$ to schooling and $h < H$ to farm work such that $s+h = S+H-\Theta$. Assumption 2 ensures that the latter scenario prevails by stating that the household prefer farm work at the maximum level of schooling S, and prefer schooling at the maximum level of farm work H (via earning wages for consumption).

Given Assumption 2, the household has an *interior* maximizing solution s and h (i.e., S and H are non-binding constraints for these allocations). This can be solved by taking the first-order condition of the household problem with respect to schooling:[1]

$$u'_S(s) = w \cdot u'_C(w \cdot h + n) \tag{A.8}$$

1. To calculate, I substitute the overlap constraint into the budget constraint via h, then substitute the combined constraint into the total utility function, and take its partial derivative with respect to s.

This says that households optimize schooling and farm work where the marginal gains from schooling s equal the marginal gains of new consumption purchased with wage w from household-farm work h. The household will send the child to school for $S-\Theta < s < S$ days and will let them farm on $H-\Theta < h < H$ days such that the first-order condition is satisfied.

A.2 Analysis of an Increase in Overlap

I am mainly interested in comparative statics on the parameter Θ which is under the control of the policymaker who determines the placement of the school calendar relative to the known farming season.

Proposition 1 *Suppose the policy-maker increases overlap Θ in the school and farming calendar holding fixed the number of days in the farming calendar H and school calendar S. Then households respond by decreasing their child's time allocation to both schooling s and farming h.*

Proof. Recall that household preferences for a child's time spent schooling s and farming h are monotonic, concave and have infinite derivatives at 0. By Assumption 1, $s+h = S+H-\Theta$. Given that S and H are fixed, an increase in Θ must decrease $s+h$ by the same magnitude, which requires that at least s or h to decrease. Suppose a household decreases schooling from s to $\hat{s}$ to satisfy the new constraint. Concave preferences increase the marginal utility of schooling so that now it is greater than the marginal utility of farm work: $u'_S(\hat{s}) > w \cdot u'_C(w \cdot h + n)$. By Assumption 2, households optimize allocations to schooling and farming where $u'_S(s) = w \cdot u'_C(w \cdot h + n)$. So, if $u'_S(\hat{s}) > w \cdot u'_C(w \cdot h + n)$, then households would allocate time toward schooling s reducing its marginal utility, and time away from h raising its marginal utility, until both Assumption 1 and Assumption 2 hold. Therefore, an increase in Θ will reduce allocations to both s and h. ■

Relaxing Assumption 2 allows households to either allocate time in school at its maximum S or time in farming at its maximum H. In this case, an increase to overlap from Θ_0 to Θ_1 does one of two things: 1) if a household still prefers the maximum allocation of either schooling or farming at Θ_1, then it will only reduce its allocation to the other activity; or 2) if a household now prefers an interior solution at Θ_1, then it will no longer maximize either time in school or farming and will instead reduce its allocation to both activities.

Relaxing Assumptions 1 and 2 allows households to allocate additional time to leisure ℓ greater than $T-S-H+\Theta$. This means that households can choose time allocations to schooling, farm work and leisure that fall below the schooling-farming time allocation frontier such that $s+h < S+H-\Theta$. Again, an increase to overlap from Θ_0 to Θ_1 does one of two things: 1) if optimal leisure remains greater than minimum leisure at Θ_1 (i.e., $\ell^* > T-S-H+\Theta_1$), then the "overlap constraint" remains non-binding and the optimal allocation to schooling, farm work and leisure will remain the same; or 2) if optimal leisure is greater than minimum leisure at Θ_0 but is less than or equal to minimum leisure at Θ_1 (i.e., $T-S-H+\Theta_0 < \ell* \leq T-S-H+\Theta_1$), then the new "overlap constraint" is binding (i.e., $s+h = S+H-\Theta_1$) and households will have to reduce allocations to schooling and farm work in the same manner shown above but with smaller expected reductions since the initial allocation fell below the schooling-farming time allocation frontier.

B Data Cleaning Details

The appendix builds on Sections 3.3 and 3.4 to provide additional details on the data cleaning procedures.

B.1 Primary Outcomes

Outcome data come from Malawi's Integrated Household Panel Survey (IHPS) 2010-2013, which was implemented as part of the World Bank Living Standards Measurement Study – Integrated Surveys on Agriculture (LSMS-ISA) initiative.[2] While the IHPS 2010 was implemented during the school calendar change "transition year", key measures of schooling and agricultural production were recalled from the pre-policy period, and I use these whenever possible. At baseline, the IHPS sample was selected to be representative at the national and regional levels, covers 26 of Malawi's 28 districts and four urban areas (Government of Malawi 2012). The IHPS defined a "community" as the village or urban location surrounding an enumeration area; similarly, I define a "community" as each 16-household enumeration area. Outcome data include highest grade level completed and hours spent working on the household farm.

First, $Grade_i$ is the highest grade level completed for individual i for the reference academic year. These data come from the Education module of the Household Questionnaire. Highest grade for the current academic year at the time of surveying equals the integer reported for "What class are you in or what was the highest class level you ever attended?" if the individual reported NOT attending school in the current academic year; one less the integer reported for "What class are you in or what was the highest class level you ever attended?" if the individual reported WAS currently attending school; or zero if responded "No" to "Have you ever attended school?" or if both a reason for never attending school was given and class information was missing. Highest grade for the previous academic year was constructed similarly but using other questions that referred to the "last completed academic year". Then, I used the daily school calendar and the recorded date of the visit during which the Education module was administered to determine to which academic year each measured referred. The baseline value of $Grade_i$ refers to the pre-policy 2009 academic year, while the outcome $Grade_i$ refers to the 2013 academic year. A similar process was used to generate a dummy if an individual started school or enrolled in the referenced academic year, both of which are used in secondary analyses.

To clean $Grade_i$, I set $Grade_i$ equal to zero if missing for individuals under five years at the time of surveying (as they were not eligible for the Education module). Otherwise I perform no additional cleaning but check that the results are not driven by erroneous or improbable panel data in Table A.1. First, in column (1), I analyze a dummy variable equal to one if the difference in an individual's 2013 $Grade_i$ and 2009 $Grade_i$ is either less than zero or greater than five (i.e., improbable changes in highest completed grade level), and zero otherwise, which equals one for only 3.6% of the sample. I find that this dummy is not significantly correlated with shift-share overlap. Further, Table A.1 columns (2)-(8) show that the main result is robust to alternative cleaning procedures for $Grade_i$. Columns (2)-(4) assume the 2013 value is the "true" reference: column (2) prevents "grade regression" by setting the maximum 2009 value equal to the 2013 value; column (3) prevents "unrealistic grade progression" by setting the minimum 2009 value equal to the 2013 value minus 5 (preventing six or more grades completed in four years); column (4) does both. Columns (5)-(7) assume the 2009 value is the "true" reference: column (5) prevents "grade regression" by setting the minimum 2013 value equal to the 2009 value; column (6) prevents "unrealistic grade progression" by setting the maximum 2013 value equal to the 2009 value plus 5; column (7) does both. Column (8) drops the 3.6% of the sample with such inconsistent observations. The regressions show that the significantly negative effect on $Grade_i$ is robust to these alternative cleaning procedures.

Second, $Farmed_i$ is an indicator if individual i was reported to work any hours on the household farm during the rainy-season sowing and harvest periods. These data come from the Household Labor section of the Rainy Season Module of the Agriculture Questionnaire, which reports for each agricultural plot the number of weeks, days per week, and hours per day of work during the land preparation and planting (i.e., sowing) period and harvesting period for up to four household members. For each individual i, these time-use variables are multiplied together to generate plot-

2. Documentation can be found at: https://microdata.worldbank.org/index.php/catalog/2248/study-description.

level total hours worked and then summed across plots to construct *Farm Hours*$_i$ as the total of all hours worked on household plots during the sowing and harvest periods in the rainy season. *Farmed*$_i$ is equal to one if *Farm Hours*$_i > 0$ and zero otherwise. The baseline value refers to the 2009/10 rainy season, while the outcome itself refers to the 2012/2013 rainy season.

Third, *Farm Hours*$_i$, defined in the previous paragraph, is cleaned to address outliers. In the primary specification, I winsorize it by replacing any value beyond the 95th percentile with the value at the 95th percentile. Then in Table A.2, I present balance tests and main effects for alternative cleaning procedures. First, columns (1)-(4) follow the balance test specification described in Table 1 to regress *Farm Hours*$_i$ not winsorized in column (1), winsorized at the 95th percentile in column (2) and the 90th percentile in column (3), and with an inverse hyperbolic sine transformation in column (4). All four regressions reveal a *positive* correlation between shift-share overlap and these measures at varying degrees of significance. Second, columns (5)-(8) follow the main specification described in Table 2 to test for the effect of shift-share overlap on the 2013 post-policy measure of *Farm Hours*$_i$ similarly adjusted for extreme outliers. These regressions reveal significant *negative* effects that, relative to each estimate's corresponding baseline imbalance, are larger in columns where outliers are addressed.

B.2 Crop Shares

Crop shares are calculated using pre-policy levels of cultivated acres for each farm plot in the rainy season 2008/09, dry season 2009, and permanent crops from the IHPS Agricultural Modules. To match to the FAO Crop Calendars (see Section below), each crop c is defined as a unique combination of its altitude zone (high, medium, or low), season of production (rainy, dry, or permanent), and basic crop type (e.g., maize, soybean, etc.). Altitude zone is determined the households' elevation: high altitude is defined as greater than 1300 meters, low altitude is defined as less than 600 meters, and medium altitude is in between these cutoffs. Season is determined by the survey module from which the data derives. Crop type are reported in the data. Cultivated acres are reported directly for each crop for the rainy season 2008/09 and at the plot level for other seasons. In cases where multiple crops were cultivated on a single plot, I divide acres between crop types based on the question: "how much of the [PLOT] is under [CROP]?". Then, I aggregate cultivated acres for crop c within each community (i.e., the 16-household enumeration area in the IHPS) and calculate the share of cultivated acres devoted to crop c in community ℓ as the variable $acreshare_{c,\ell}$.

Table A.3 shows the most common crops c across all communities in the primary sample. An obvious outlier is "Medium rainy maize" (i.e., medium-altitude rainy-season maize", which makes up 41.4% of all cultivated acres in the sample. Other common crops that make up over 5% of the sample's cultivated acres include Low rainy maize, Medium rainy groundnut, High rainy maize, and Medium rainy tobacco. Table A.4 breaks down these shares by crop characteristics across all communities, showing that 69.5% are cultivated in the medium-altitude zone, 93.5% are cultivated in the rainy season, and that–across all altitude zones and seasons–maize itself makes up 60.4% of all cultivated acres.

B.3 Crop Calendars

Data on community crop production are matched to crop calendars from the Food and Agriculture Organization (FAO) Crop Calendar Tool, which provides start and end months for the sowing and harvest periods for 45 major crops in Malawi. The FAO Crop Calendar Tool defines a crop c as a unique combination of its altitude zone (high, medium, or low), season of production (rainy, dry, or permanent), and basic crop type (e.g., maize, soybean, etc.). The most common crops, such as maize, have different calendars for different altitudes and seasons; in general, crop calendars are longer at lower altitudes and in the rainy versus the dry season.

Crop calendars from the FAO Crop Calendar Tool match to 83% of pre-policy cultivated acres in the IHPS data. For the remaining 17% of cultivated acres, I use the modal sowing month and harvest month reported by IHPS households in 2010 (the earliest available). In using IHPS-based crop calendars from 2010, I assume that the modal crop calendars were unaffected by the one-month school calendar change between 2009 and 2010, or at least that any endogeneity has minuscule effect on my analysis after these crops are weighted by their relatively smaller share of total cultivated acres. In additional to possible endogeneity bias, I also do not prefer the IHPS calendars because they only measure the sowing and harvest period in monthly increments.

Table A.5 displays the crop calendar inputs for common crops cultivated in sample communities (same list as Table A.3). A quick comparison of the start and end dates of the sowing and harvest periods reveals that there is less variation in the timing of the rainy-season sowing period relative to the harvest. Sowing period start and end dates span from November 1 to January 16, though are mostly between November 15 and December 21. Meanwhile, harvest period start and ends dates span from January 1 to July 30—a six month period. Further, Table A.5 columns 6 & and 7 show the number of days that the crop's calendar overlaps with the pre- and post-policy school calendar, respectively. Finally, column 8 shows the change in overlap between the post- and pre-policy year, which is the crop-level shock in the shift-share estimation strategy.

Table A.1: **Grade Level in 2013 following Different Cleaning Procedures**

VARIABLES	Dummy: '13-'09 <0 or >5 (1)	Assume 2009 value is "true" reference: Fix Max '09 if '09>'13 (2)	Fix Min '09 if '13>'09+5 (3)	Both (2) & (3) (4)	Assume 2013 value is "true" reference: Fix Min '13 if '09>'13 (5)	Fix Max '13 if '13>'09+5 (6)	Both (5) & (6) (7)	Drop Obs if '09>'13 (8)
Shift-Share Overlap$_\ell$	-0.012	-0.236	-0.235	-0.239	-0.234	-0.234	-0.236	-0.259
	(0.014)	(0.104)	(0.107)	(0.095)	(0.107)	(0.108)	(0.099)	(0.096)
	[0.237]	[0.019]	[0.034]	[0.013]	[0.024]	[0.035]	[0.019]	[0.014]
Pre-Policy Outcome_*i*		1.805	1.530	1.638	0.894	0.840	0.913	0.987
		(0.102)	(0.066)	(0.063)	(0.031)	(0.041)	(0.028)	(0.026)
Observations	2,142	2,142	2,142	2,142	2,142	2,142	2,142	2,064
R-squared	0.041	0.704	0.710	0.775	0.686	0.682	0.734	0.760
Pre-Policy Outcome Mean		1.38	1.47	1.42	1.43	1.43	1.43	1.39
Δ Outcome Mean (Post - Pre)		2.35	2.26	2.31	2.35	2.26	2.31	2.33

Notes: Dependent variables relate to $Grade_i$, which receives minimal cleaning the primary analysis. Column (1) is a dummy variable equal to one if the difference in an individual's 2013 $Grade_i$ and 2009 $Grade_i$ is either less than zero or greater than five and is zero otherwise. Columns (2)-(8) tests robustness of the primary results after implementing different reasonable data cleaning procedures for $Grade_i$. Columns (2)-(4) assume the 2013 value is the "true" reference: column (2) sets the maximum 2009 value equal to the 2013 value; column (3) sets the minimum 2009 value equal to the 2013 value minus 5; column (4) does both. Columns (5)-(7) assume the 2009 value is the "true" reference: column (5) sets the minimum 2013 value equal to the 2009 value; column (6) sets the maximum 2013 value equal to the 2009 value plus 5; column (7) does both. Column (8) drops inconsistent observations from the sample. $ssoverlap_\ell$ and included controls defined in Table 2. Conventional robust standard errors in parentheses. Randomization inference p-values in square brackets.

Table A.2: **Household-Farm Hours: Balance, Effects, Robustness**

VARIABLES	Balance Test on Baseline Values: No Winz (1)	Winz 95 (2)	Winz 90 (3)	IHS (4)	Main Test on Post-Policy Values: No Winz (5)	Winz 95 (6)	Winz 90 (7)	IHS (8)
Shift-Share Overlap$_\ell$	7.899	2.234	0.885	0.101	-5.335	-3.932	-4.006	-0.261
	(3.915)	(1.590)	(1.075)	(0.137)	(5.239)	(3.750)	(2.902)	(0.165)
	[0.014]	[0.068]	[0.191]	[0.204]	[0.189]	[0.183]	[0.085]	[0.056]
Pre-Policy Outcome_i					0.162	0.227	0.298	0.189
					(0.057)	(0.092)	(0.123)	(0.086)
Observations	2,142	2,142	2,142	2,142	2,142	2,142	2,142	2,142
R-squared	0.072	0.128	0.140	0.152	0.131	0.207	0.231	0.255
Pre-Policy Outcome Mean					13.96	9.82	7.21	1.03
Δ Outcome Mean (Post - Pre)					23.65	21.49	19.59	1.06

Notes: Dependent variables relate to the continuous measure of household-farm hours $Farm\ Hours_i$. Columns (1)-(4) follow the balance test specification described in Table 1 to regress $Farm\ Hours_i$ not winsorized in column (1), winsorized at the 95th percentile in column (2) and the 90th percentile in column (3), and with an inverse hyperbolic sine transformation in column (4). Columns (5)-(8) follow the main specification described in Table 2 to test for the effect of shift-share overlap on the 2013 post-policy measure of $Farm\ Hours_i$ similarly adjusted for extreme outliers. $ssoverlap_\ell$ and included controls defined in Table 2. Conventional robust standard errors in parentheses. Randomization inference p-values in square brackets.

Table A.3: **Aggregated Crop Shares across Sample Communities**

CROP	Share (%)
Medium rainy maize	41.4
Low rainy maize	11.0
Medium rainy groundnut	7.7
High rainy maize	7.2
Medium rainy tobacco	6.7
Medium rainy pigeonpea	3.2
Low perm cassava	2.3
Medium rainy soybean	2.0
Medium rainy sorghum	1.4
Low rainy tobacco	1.0
Medium rainy bean	1.0
Low rainy rice	1.0

Notes: Crops are defined as a unique combination of its altitude zone (high, medium, or low), season of production (rainy, dry, or permanent), and crop type (e.g., maize, soybean, etc.). Table displays a crop's share of cultivated land for crops grown on at least 1% (after rounding to the tenth decimal place) of all cultivated land in sample communities. Crops grown in the sample at less than 1% include (sorted descending by share size): Medium rainy sweetpotato, Medium dry tanaposi, High rainy soybean, Medium rainy nkhwani, High rainy groundnut, Low rainy cotton, Low rainy pigeonpea, Medium dry maize, Low rainy sorghum, High rainy tobacco, Low rainy groundnut, High rainy bean, Medium rainy cotton, Medium perm cassava, High perm cassava, Medium rainy millet, Medium rainy rice, High rainy sweetpotato, Medium rainy sunflower, Low dry maize, Low dry bean, Low rainy millet, Medium perm banana, Low rainy nkhwani, Medium dry tomato, Low rainy other, Medium rainy tomato, Medium rainy potato, Low rainy bean, Medium dry nkhwani, Medium dry bean, High dry potato, Medium rainy peanut, High rainy nkhwani, High rainy millet, Low rainy sweetpotato, Medium rainy tanaposi, Medium rainy other, High rainy tomato, High dry bean, High rainy potato, Medium dry other, Low perm banana, Low rainy pea, Low dry other, Low rainy okra, Medium dry potato, Low dry nkhwani, High perm banana, Medium dry sugarcane, Low rainy soybean, Medium rainy paprika, High dry onion, High dry tomato, High dry wheat, Low rainy potato, Low dry tomato, Low dry sweetpotato, Medium rainy pea, High rainy sunflower, Low rainy tomato, High rainy other, Medium dry sweetpotato, Medium perm mango, High rainy paprika, Low dry tanaposi, High rainy pea, Medium dry cabbage, Low dry pea, High dry sugarcane, Medium dry pea, High dry maize, High dry tanaposi, Low dry okra, Low perm mango, Medium perm papaya, High perm mango, Medium dry pigeonpea, Medium perm avocado, Medium perm guava, Medium perm orange, Medium perm apple, Medium perm tangerine, Medium perm masau, Low perm papaya, High perm avocado, Low perm orange, Medium perm lemon, Medium perm other, Low perm masau, Low perm guava, Medium perm peach, Low perm other, High perm peach, High perm apple, Low perm lemon, Low perm avocado, High perm orange, Low perm apple, and High perm lemon. Crops not grown in the sample include: High rainy peanut, Low rainy sugarcane, High perm tangerine, Low rainy peanut, High dry cabbage, High rainy wheat, High rainy rice, Medium rainy wheat, High dry nkhwani, High dry other, Medium dry paprika, High rainy onion, Medium dry onion, Low dry rice, High rainy sorghum, High rainy pigeonpea, Low perm pineapple, Medium dry rice, Medium rainy okra, and High perm pineapple.

Table A.4: **Aggregated Crop Shares by Crop Characteristics**

ALTITUDE ZONE	Share (%)
Medium	69.5
Low	19.2
High	11.3
SEASON	Share (%)
Rainy	93.5
Perm	3.5
Dry	2.9
CROP TYPE	Share (%)
Maize	60.4
Groundnut	8.9
Tobacco	8.2
Pigeonpea	3.9
Cassava	3.2
Soybean	2.9
Sorghum	2.0
Bean	2.0
Sweet Potato	1.3
Rice	1.2
Nkhwani	1.2
Cotton	1.2
Tanaposi	1.0

Notes: The table breakdowns down crop shares by the characteristics that define each crop: altitude zone (high, medium, or low), season of production (rainy, dry, or permanent), and crop type (e.g., maize, soybean, etc.). Crop types grown in the sample at less than 1% (after rounding) include (sorted descending by share size): millet, tomato, potato, other, sunflower, ,banana, peanut, pea, paprika, sugarcane, okra, onion, wheat, mango, cabbage, papaya, avocado, orange, guava, apple, tangerine, masau, lemon, and peach. Crop types not grown in the sample include: pineapple.

Table A.5: **Crop Calendars & Shocks for Common Crops in Sample Communities**

	Sowing		Harvest		Overlap w/ School (days)		Shock:
CROP	Start	End	Start	End	Pre-Policy	Post-Policy	Change
Medium rainy maize	Nov 15	Dec 31	May 1	Jul 3	51	65	14
Low rainy maize	Nov 15	Dec 31	Apr 24	Jun 10	38	55	17
Medium rainy groundnut	Nov 15	Dec 15	May 1	May 15	16	30	14
High rainy maize	Nov 15	Dec 31	May 15	Jul 30	56	60	4
Medium rainy tobacco	Dec 1	Dec 31	Feb 1	Mar 31	42	51	9
Medium rainy pigeonpea	Nov 15	Dec 31	Jun 1	Jul 30	45	48	3
Low perm cassava	Nov 15	Dec 31	Aug 15	Nov 14	70	70	0
Medium rainy soybean	Nov 15	Dec 15	Mar 15	May 25	41	62	21
Medium rainy sorghum	Nov 1	Nov 30	Jun 1	Jun 30	37	44	7
Low rainy tobacco	Dec 1	Dec 31	Jan 1	Mar 31	62	72	10
Medium rainy bean	Nov 1	Nov 30	Mar 1	Apr 30	44	56	12
Low rainy rice	Dec 15	Jan 16	Apr 7	May 25	30	35	5

Notes: Table displays descriptive information for crops grown on at least 1% (after rounding to the tenth decimal place) of all cultivated land in sample communities (see Table A.3 for same list). Columns 2-5 show inputs into creating the agricultural calendar for each crops–the start and end dates of the sowing and harvest periods. Columns 6 & and 7 show the number of days that the crop's calendar overlaps with the pre- and post-policy school calendar, respectively. Finally, column 8 shows the change in overlap between the post- and pre-policy year, which is the crop-level shock in the shift-share estimation strategy.

C Simulation Details

C.1 Overlap Comparison

With its shift-share construction and normalization, changes in the overlap measure $ssoverlap_\ell$ can be hard to interpret in "real" terms. To put $ssoverlap_\ell$ into perspective, I ran a simulation and estimate that a 0.61 standard deviation increase in $ssoverlap_\ell$ is roughly equivalent to adding five days of school (equivalent to one week of school) of overlap during sowing and harvest of rainy-season maize in the average sample community.

To determine this, I averaged crop shares across the 135 sample communities, added new school days to the 2009 pre-policy school calendar, and re-calculated $ssoverlap_\ell$ to simulate the effect of increasing overlap on the $ssoverlap_\ell$ measure. I chose to add school days across five Saturdays, when school was not already scheduled in the pre-policy calendar, during periods of sowing (late November to late December) and harvest (early May to early June) of rainy-season maize. These periods alone accounts for over half of all planted acres in the sample (when summed across the three altitude zones) and thus likely represents periods of peak labor demand for most farming communities. By spreading out the additional school days over a five weeks in the peak sowing period and peak harvest period, I simulate adding five days of school to these peak periods, on average, rather than any single week, in particular.

I find that adding five days (equivalent to one week of school) spaced out during the peak sowing period increases normalized $ssoverlap_\ell$ by 0.68 standard deviations, while adding five days spaced out during the peak harvest period increases normalized $ssoverlap_\ell$ by 0.54 standard deviations—the difference caused by the fact that sowing periods across all crops are more concentrated from mid-November through December, whereas harvest periods across all crops vary from February through July depending on the length of the growing season. Then, I take the average of these estimates to determine that adding five days of school during peak farming periods increases normalized $ssoverlap_\ell$ by 0.61 standard deviations. Similarly, one can also interpret the results as the impact of adding ten days of school during peak farming periods by multiplying the coefficients by 1.21 standard deviations.

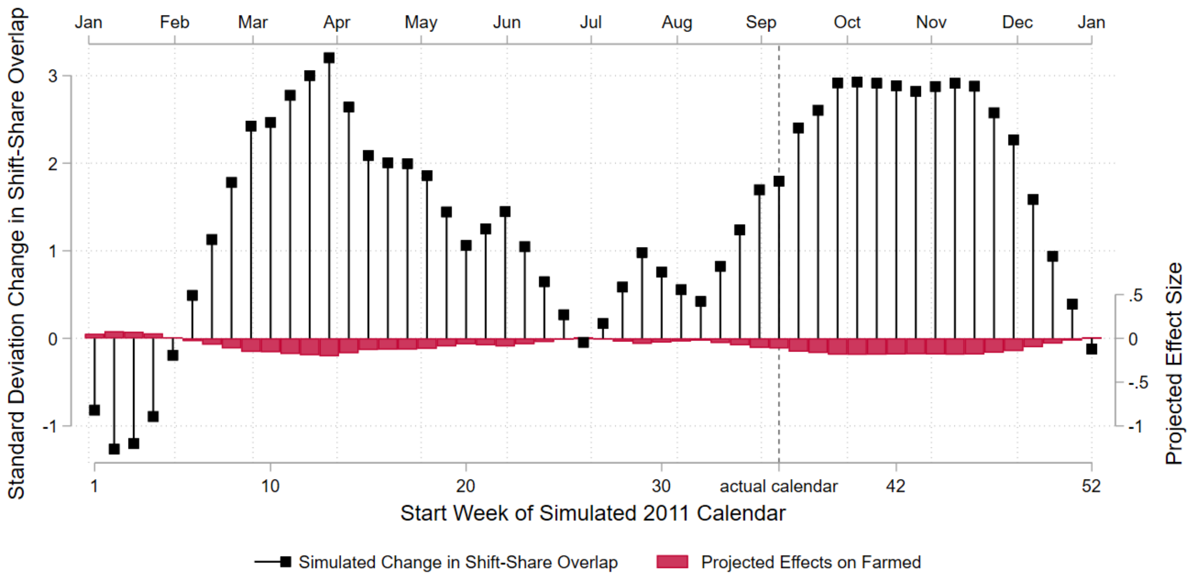
C.2 Policy Simulation

To identify Malawi's overlap-minimizing school calendar, I simulate 52 other potential school calendars that could have been used for the 2011 school year, each starting on a Monday and maintaining the structure and length of the original 2011 school calendar. This is done by effectively shifting the school calendar backward to previous Mondays in the year or forward to future Mondays using July 1st as the cutoff for the year. Then, for each of 52 simulated school calendars in 2011, I estimate the counterfactual change in shift-share overlap for each community relative to the original 2009 school calendar. Next, I multiply the counterfactual change in shift-share overlap by the coefficient in Table 2 column (1) to approximate the calendar's potential effect on $Grade_i$. A similar process simulates 52 other potential school calendars that could have been used for the original 2009 school year.

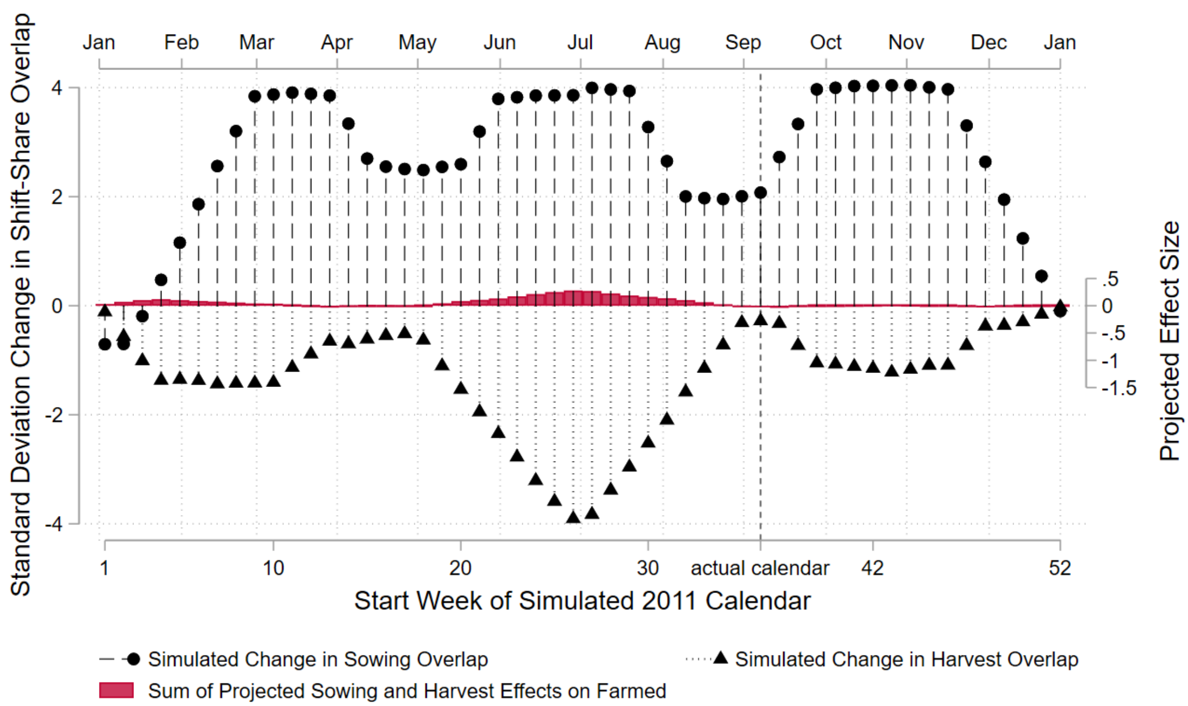
Additionally I use the policy simulation to estimate potential effects on $Farmed_i$, which are presented in Figure A.1 below. Projected overall effects on $Farmed_i$ in panel (a) follow a similar pattern to projected effects on $Grade_i$ in Figure 3 panel (a). However, different estimates in sowing and harvest periods from Table 6 creates divergent patterns in panel (b) across the two figures. Here, $Farmed_i$ is actually projected to increase under many alternative school calendars, especially when harvest-specific overlap is minimized given that harvest overlap appears to drive overlap's overall negative effect on $Farmed_i$.

Figure A.1: **Simulated Impacts on Household-Farm Labor**

(a) Overall Impact



(b) Sowing versus Harvest Impact



Notes: Figure depicts simulated policy impacts of alternative 2011 school calendars relative to the actual 2009 school calendar. The 52 simulated calendars are denoted by the week in which they starts, as each beings on a Monday and maintains the length and structure of the actual 2011 school calendar. Panel (a) plots the expected change in shift-share overlap (measured in standard deviations of $ssoverlap_\ell$) during peak farming periods for the average sample community using drop lines, and the corresponding projected effects on $Farmed_i$ using crimson bars estimated using the causal results in Table 2 column (2). Panel (b) plots the expected change in shift-share overlap separately for both the sowing and harvest periods (again measured in standard deviations of $ssoverlap_\ell$) using drop lines, and the corresponding projected effects on $Farmed_i$ using crimson bars calculated by summing the separate sowing and harvest effects estimated from results in Table 6 column (2). Additionally, the top horizontal axis estimates the start of each month, and a vertical line at week 36 the notes the actual 2011 school calendar that started on September 6, 2010.

D Period-Specific Regressions

Most secondary analyses will use the primary specification in Equation 4 but with alternative subsamples or outcomes. However, one secondary analysis will regress different specifications to estimate the causal effect of $ssoverlap_\ell$ at specific periods in the farming and school calendar. To compare overlap between the sowing and harvest periods, I regress:

$$Y_{i,\ell} = \alpha + \beta_1 ssoverlap\_sow_\ell + \beta_2 ssoverlap\_harv_\ell + X'_{i,\ell}\phi + \varepsilon_{i,\ell} \tag{D.1}$$

where $ssoverlap\_sow_\ell$ and $ssoverlap\_harv_\ell$ represent shift-share overlap during the sowing and harvest periods, respectively, and controls $X_{i,\ell} = \delta Y_{base,i,\ell} + \rho farmshare_\ell + w_{i,\ell}{}'\gamma$ are the same as those described in Equation 4. Here, I expect the impact of overlap in both the sowing period β_1 and harvest period β_2 to be negative, but do not make predictions about their relative effect size. Note that because $ssoverlap\_sow_\ell$ and $ssoverlap\_harv_\ell$ sum to $ssoverlap_\ell$, the latter does not serve a purpose in the regression.

Moreover, to measure overlap's effects during critical periods in the school year, I regress:

$$Y_{i,\ell} = \alpha + \beta_1 ssoverlap\_\ell + \beta_2 ssoverlap\_admis_\ell + \beta_3 ssoverlap\_exams_\ell + X'_{i,\ell}\phi + \varepsilon_{i,\ell} \tag{D.2}$$

where $ssoverlap\_admis_\ell$ and $ssoverlap\_exams_\ell$ represent shift-share overlap during the admissions and exam periods, respectively, and other variables $ssoverlap\_\ell$ and controls $X_{i,\ell} = \delta Y_{base,i,\ell} + \rho farmshare_\ell + w_{i,\ell}{}'\gamma$ are the same as described in Equation 4. Here, coefficients on the new regressors β_2 and β_3 are interpreted as interaction terms (i.e., overlap's effect during the first month of school is $\beta_1 + \beta_2$) and test how overlap during the first and last month of school differ from the rest of the school year. I do not make a formal prediction on β_2 and β_3. If β_2 and β_3 estimates are positive (negative) across the primary outcomes, then an interpretation is that overlap during this period is a less (more) binding constraint for the specified activity.

Finally, to check the robustness of the secondary results to the full specification, I regress:

$$\begin{aligned} Y_{i,\ell} = \alpha + {} & \beta_1 ssoverlap\_sow_\ell + \beta_2 ssoverlap\_harv_\ell + \\ & \beta_3 ssoverlap\_admis_\ell + \beta_4 ssoverlap\_exams_\ell + X'_{i,\ell}\phi + \varepsilon_{i,\ell} \end{aligned} \tag{D.3}$$

where variables are defined as described above.

E Alternative Inference Procedures

In this appendix, I provide additional details for the alternative inference procedures listed in Table A.6 below. This includes conventional ordinary least squares (OLS), several randomization inference (RI) procedures, and Borusyak, Hull, and Jaravel (2022)'s share-weighted shock-level regression technique. The fact that these alternative procedures produce similar p-values builds confidence in the primary results.

Table A.6 starts in row (a) with conventional ordinary least squares (OLS) estimates. OLS produces valid inference under certain asymptotic assumptions including the absence of omitted variable bias. However, OLS standard errors may be invalid in shift-shares settings due to unobserved correlation between observations with similar exposure shares (Adão, Kolesár, and Morales 2019; Borusyak, Hull, and Jaravel 2022). In my setting, this could be unobserved correlation between individuals living in different communities that have similar sets of crop shares. Of most concern, when residuals are positively correlated, conventional OLS estimation will likely overreject. Hence there is motivation for turning to other inference procedures.

Two types of inference that can account for this issue in shift-share settings are RI approaches (e.g., Borusyak and Hull (2021)) and shift-share asymptotic approaches (e.g., Borusyak, Hull, and Jaravel (2022)). Each type has its strengths and weaknesses. As described by Borusyak and Hull (2021), RI requires specifying the full shock assignment process used to generate shock counterfactuals, whereas asymptotic approximation only requires specifying its first moment. Yet, RI is valid even when asymptotic assumptions of homoskedasticity or distribution symmetry are violated in the data. One additional assumption for Borusyak, Hull, and Jaravel (2022)'s share-weighted shock-level regression approach is that of having many shocks such that the largest share in the regression converges to zero as the sample size increases, which ensures a large effective sample size for the shock-level regression. However, the prevalence of medium-altitude rainy-season maize in my data, which accounts for 41.4% of the sum of crop shares, threatens to violate this assumption. Indeed, I only estimate an effective sample size of 15.5, although Borusyak, Hull, and Jaravel (2022)'s simulations conclude that an effective sample size of 20 "may be considered satisfactory" with a rejection rate near 7% instead of 5%. Therefore, I pursued a RI approach ex ante, although it turns out that both procedures produce similar results.

E.1 Alternative RI procedures

In this section, I specify the different *shock assignment processes* used in alternative RI procedures, which are each estimated as follows. First, I use the shock assignment process to generate a set of crop-level shock counterfactuals. Second, I weight the shocks counterfactuals by the existing crop shares $share_{c,\ell}$, and sum across crops to estimate a counterfactual shift-share overlap measure for each location (as in Equation 3). Third, I run the regression specified in Equation 4, replacing only the actualy shift-share overlap measure with the counterfactual one, and collect the counterfactual β, called $\tilde{\beta}$. Then, I repeat these three steps 1000 times. Finally, I calculate Fisher exact p-values as the fraction of $\tilde{\beta}$ for which $|\tilde{\beta}| \geq |\hat{\beta}|$.

The various shock assignment processes described below are labeled according to their row in Table A.6. Row (b) randomly re-draws with replacement the crop-level shock from the actual distribution of shocks, which is the RI approach used in the primary analysis (and hence corresponds Table 2). This approach avoids putting additional structure on the data and assumes that 1) shock assignment is uncorrelated with crop characteristics, and 2) the actual distribution is representative of true distribution. Row (c) relaxes the first assumption by imposing that shocks are correlated by season by randomly re-drawing with replacement shocks from the actual distribution of shocks for crops within the same season (i.e., rainy, dry, permanent). Row (d) relaxes the second assumption by randomly re-drawing with replacement from a normal distribution defined by the actual distribution's first and second moments. Row (e) combines both features to randomly re-draws with replacement from a normal distribution defined by same-season crops' first and second moments.

Rows (f) and (g) take a different approach in specifying the shock assignment process. Rather than rely on information from the existing distribution, this approach recognizes Malawi's school calendar change as the underlying source of variation in shift-share overlap and thus generates shock counterfactuals from simulations of all possible school calendar changes that could have occurred between 2009 and 2011. First, for both 2009 and 2011, I construct 52 hypothetical school calendars, each starting a Monday and maintaining the structure and length of the year's original school calendar. This is done by effectively shifting the school calendar backward to previous Mondays in the year or forward to future Mondays. I use July 1st as the cutoff for the year (rather than January 1st) so that the simulated 2009, 2010, and 2011 school years do not intersect. Second, for each of 52 simulated school calendars for 2009 and 2011, I estimate the overlap between it and crop-level farming calendars as in Equation 1. Finally, for the 2,704 possible school calendar changes between 2009 and 2011 (i.e., 52 x 52), I estimate the shock counterfactual $\Delta overlap_c$ for crop c. Together, the $s = 2,704$ simulated shock counterfactuals form the shock distribution for crop c. In row (f), I assume that shock assignment is correlated by crop season (i.e., rainy, dry, permanent), so for each iteration I randomly re-draw with replacement the shock counterfactuals for crop c from the same simulation s for same-season crops. In row (g), I more conservatively assume that shock assignment is correlated across all crops, and thus for each iteration I draw all crop-specific shock counterfactuals from the same simulation s. Further, in row (g), rather than randomly re-drawing with replacement for 1000 iterations, I cycle through all possible 2,704 simulations when constructing the Fisher exact p-values.

E.2 Share-weighted shock-level regression

For completion and robustness, I also use share-weighted shock-level regression technique pioneered by Borusyak, Hull, and Jaravel (2022) to estimate exposure-robust standard errors. First, I transform my location-level outcome and shift-share overlap data into a dataset of exposure-weighted "crop-level" aggregates using their *ssaggregate* command, partialling out $Y_{base,i,\ell}$, $farmshare_\ell$, and $w_{i,\ell}$ via the controls option. This aggregatation is equivalent to the "recentering" method described in Borusyak and Hull (2021) for eliminating omitted variable bias when regressing a shift-share variable. Second, following their Proposition 5, I estimate a crop-share-weighted shock-level regression of:

$$\bar{Y}_c^{\perp} = \alpha + \beta ss\bar{overlap}_c^{\perp} + \mathbf{season}_c \mathbf{season}_c \mathbf{season}_{c\,\mathbf{season}'_c \gamma + \bar{\varepsilon}_c^{\perp}\,\text{(E.1)}}$$

where $ss\bar{overlap}_c^{\perp}$ is instrumented by crop-level shocks $\Delta overlap_{c,2011-2009}$, and $season_c$ is a vector of crop-level seasonal dummies (rainy, dry, permanent) and a dummy for crops classified as grains included as non-transformed crop-level controls. I cluster standard errors by season and further specify Stata's *ivreg2*'s small option, which requests small-sample statistics (F and t-statistics) and performs a finite sample adjustment. Estimates from Equation E.1 produce numerically equivalent estimates of $\hat{\beta}$ as well as exposure-robust standard errors reported in Table A.6.

Table A.6: **Main Results: P-values from Alternative Inference Procedures**

	VARIABLES	Grade (1)	Farmed (2)
(a)	Conventional OLS	0.044	0.080
(b)	Random draw (w/ replacement)	0.038	0.044
(c)	Random draw (w/ replacement): correlated by season	0.062	0.088
(d)	Random Normal shock	0.024	0.044
(e)	Random Normal shock: correlated by season	0.051	0.074
(f)	Simulated calendar changes: correlated by season	0.039	0.040
(g)	Simulated calendar changes: correlated across all crops	0.047	0.067
(h)	Share-weighted shock-level regression (BHJ)	0.105	0.111

Notes: Alternative inference procedures test the effect of $ssoverlap_{\ell}$ in same regressions as in Table 2 columns (1)–(3). I report p-values from (a) conventional ordinary least-squares (OLS); randomization inference procedures that perturb the crop-level shock by randomly re-drawing it from (b) the actual distribution of shocks with replacement (as in Table 2), (c) the actual distribution of shocks for same-season crops with replacement, (d) a normal distribution defined by the actual distribution's first and second moments, (e) a normal distribution defined by same-season crops' actual distribution's first and second moments, (f) a simulated distribution of shocks corresponding with the universe of possible school calendar changes between 2009 and 2011, assuming school starts on a Monday and a fixed length and structure of the school calendar, drawn for all crops with the same season, and (g) a simulated distribution of shocks as described above but for all crops together; and (h) the share-weighted shock-level regression as described in Borusyak, Hull, and Jaravel (2022).

F Calculation Details for Speculative Extensions and Projected Impacts

F.1 Perceived Value of Time in School

The appendix provides details for calculations in Section 5.1.2. In my reference estimate, I approximate the perceived value of one completed grade level for the average sample household at $50 USD. I start by equating effect sizes in Table 2 column (1) and the upper-bound of the 95% confidence interval for 3 column (3): 0.14 grades with, at most, three days of household-farm work (an upper-bound estimate of one day annually for three post-policy years). In Malawi's 2010 Integrated Household Survey (IHS3) sample, children under 15 who are hired to do farm labor are paid an average daily wage of 331 kwacha, or $2.35 in 2009 USD (following an exchange rate of 1 USD to 141 Malawian kwacha in 2009 (St. Louis Federal Reserve Bank, FRED Economic Data 2012)). Thus, 0.14 grades = 3 days of farm work = $2.35 * 3 = $7.05 $\implies$ 1 grade = $7.05 USD / 0.14 = $50.34, as an upper-bound estimate.

F.2 Projected Impacts on Lifetime Income

The appendix provides details for calculations in Section 6.2. First, I replicate a pre-existing "Mincerian" estimate of an additional year of schooling with a comparable analysis in my own data. Then, I detail how I approximate the present discounted value to an individual of an additional year of schooling using the estimated rate of return.

In a paper comparing returns to schooling around the world, Montenegro and Patrinos (2014) estimates the return to another year of schooling in Malawi at 9.8% in 2010 (SD of 4.5). As this estimates likely derives from the same dataset, I attempt to replicate their analysis to confirm its validity. Following their methodology (which itself is rooted in Mincer (1958) and Ben-Porath (1967)), I regress log wages on years of schooling, potential experience (age - years of schooling - 6), and potential experience squared for all waged employees. In the IHS3 data, I standardize wages into "daily wages" by assuming that those earning weekly wages work five days a week and those earning monthly wages work 21.7 days a month (5 days a week x 52 weeks per year / 12 months per year).

Results are presented in Table A.8. Columns (1)-(3) are the natural logarithm of an individuals' average daily wage as reported across a primary job, a secondary job, and ganyu (day) labor. Column (1) replicates Montenegro and Patrinos (2014)'s finding of a 9.8% rate of return to an additional year of schooling in 2010. Then, in column (2) and (3), I run the regression for the sub-sample of adults aged 25-64 who have 1-12 years of schooling, following the Evans and Yuan (2019)'s sample used in Section F.3 above—column (3) with additional controls for age, age-squared, and sex—both of which produce very similar results. Then, comparable regressions in columns (4)-(6) are on the natural logarithm of the daily wage from an individual's primary job only, and are thus only applicable to those with a formal primary job, which suggest even larger rates of return of 13-17%. Each sample's estimated average annual income from a primary job, a secondary job, and ganyu labor in 2009 USD (following an exchange rate of 1 USD to 141 Malawian kwacha in 2009 (St. Louis Federal Reserve Bank, FRED Economic Data 2012)), to which these rates of return apply, are placed in the last row of the table.

These regressions support the claim (perhaps conservatively) that the return to an additional year of schooling in Malawi was 9.8% at the time of the policy change. Assuming the average annual income of $444 USD in Table A.8 column (1), this equates to a nominal annual return of $44 USD. I then approximate the present discounted value to an individual of an additional year of primary or secondary schooling as $PDV = \text{Annual return} * [1-(1/(1+r))^n]/r =$ $1112, where I assume interest rate $r = 0.03$ and number of time periods $n = 48$ as the number of working-age years from ages 18–65. I will use these estimates in the main text to project the potential impacts of overlap between the school and farm calendars on the lifetime returns to income.

F.3 Projected Impacts on Learning

The appendix provides details for calculations in Section 6.3. I start by inverting Evans and Yuan (2019)'s measure of Equivalent Years of Schooling (EYOS) to restate my primary results in terms of equivalent changes in learning. Using household survey data from the World Bank's STEP Skills Measurement Program for five countries that included literacy assessments, Evans and Yuan (2019) find that a one standard deviation gain in literacy skill is associated with between 4.7 and 6.8 additional years of schooling, which inverted suggests that one additional year of schooling is associated with between 0.15 and 0.21 standard deviation gain in literacy. By this metric, a five-day increase in overlap during peak production that decreases $Grade_i$ by 0.14 can be projected to be equivalent to a 0.02–0.04 standard deviation loss in literacy after only four years.

I check that the Evans and Yuan (2019) estimate is consistent with a comparable analysis in my own data, despite a key limitation that my only learning metric is a self-reported binary measure of literacy. I define a literacy indicator as equal to one if an individual is reported to be literate in either English or Chichewa, and zero otherwise. Following Evans and Yuan (2019), I define my sample to be adults aged 25-64 who have 1-12 years of schooling. In this sample, the literacy indicator has a mean of 0.819 and standard deviation of 0.385. Then, using data from the baseline survey round, I regress the literacy indicator on years of schooling, which estimates the simple average of learning gains from grade 1 to grade 12 (equivalent to Evans and Yuan (2019)'s Method 1). This regression is reported in Table A.7 column (1) and with individual controls for age, age-squared and sex in column (2). To compare between primary and secondary school, I estimate the same regressions for the sub-sample of adults who have 1-8 years of schooling in columns (3)-(4) and 9-12 years of school in column (5)-(6).

My main result in column (1) suggests that one additional year of schooling is associated with a 6.4 percentage point increase the likelihood of reporting literacy. As reported in the last row of the table, this represents a 0.17 standard deviation gain in literacy, which is reassuringly consistent with the Evans and Yuan (2019) estimates. Results in remaining columns suggest that this association is robust to including individual controls and also that the association between additional years of schooling and literacy are stronger in primary school relative to secondary school both in magnitude and statistical significance. Specifically, one additional year of primary school is associated with a 0.27 standard deviation gain in literacy, which is notable as my sample and estimated effects of overlap are also concentrated in primary school. I will use these estimates in the main text to project the potential impacts of overlap between the school and farm calendars on literacy. Using these estimates, I extrapolate that a five-day increase in overlap during peak production that decreases $Grade_i$ by 0.14 can be projected to be equivalent to a 0.02–0.04 standard deviation loss in literacy after four years.

Table A.7: **Regression of Adult Literacy on Years of Schooling**

	Literacy Indicator					
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Years of Schooling	0.064	0.063	0.106	0.104	0.004	0.004
	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	2,058	2,058	1,474	1,474	584	584
R-squared	0.282	0.288	0.297	0.305	0.003	0.012
Individual Controls	N	Y	N	Y	N	Y
Grades in Sample	1-12	1-12	1-8	1-8	9-12	9-12
Outcome Mean	0.82	0.82	0.75	0.75	0.99	0.99
Outcome Std. Dev. (SD)	0.38	0.38	0.43	0.43	0.08	0.08
Coef. in SD of Grade 1-12 Outcome	0.17	0.16	0.27	0.27	0.01	0.01

Notes: Regression of literacy on years of schooling for a sample of adults aged 25-64 surveyed at baseline. Dependent variable *Literacy Indicator* equal to one if an individual is reported to be literate in either English or Chichewa in 2010, and zero otherwise. *Years of Schooling* is the highest reported completed grade as of 2009. "Individual Controls", if included, are age, age-squared, and sex. "Grades in Sample" reports if the specification includes adults with 1-12, 1-8 or 9-12 years of schooling. "Coef in SD of Outcome" divides the coefficient by 0.385 to report in units relative to the standard deviation of the literacy indicator for adults grades 1-12. Robust standard errors in parentheses.

Table A.8: **Regression of Log Wages on Years of Schooling**

	Log Avg Wage: Jobs & Ganyu			Log Wage: 1st Job Only		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Years of Schooling	0.098	0.099	0.098	0.130	0.149	0.171
	(0.003)	(0.006)	(0.027)	(0.005)	(0.010)	(0.041)
Potential Experience	0.032	0.033	0.034	0.031	0.043	0.056
	(0.002)	(0.008)	(0.017)	(0.004)	(0.011)	(0.027)
Experience Squared	-0.000	-0.000	-0.000	-0.000	-0.001	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	4,124	1,937	1,937	1,253	784	784
R-squared	0.254	0.131	0.135	0.401	0.245	0.246
Sample	All	Limited	Limited	All	Limited	Limited
Individual Controls	N	N	Y	N	N	Y
Outcome Mean	5.585	5.725	5.725	5.953	5.916	5.916
Outcome Std. Dev.	0.881	0.811	0.811	1.006	0.854	0.854
Avg. Annual Income in USD	444	449	449	1120	814	814

Notes: Dependent variables are defined as follows: columns (1)-(3) are the natural logarithm of an individuals' average daily wage as reported across a primary job, a secondary job, and ganyu (day) labor, and columns (4)-(6) are the natural logarithm of an individuals' daily wage for their primary job only. Reported wages are standardized into "daily wages" by assuming that those in formal job work five days a week or 21.7 days a month (5 days a week x 52/12 weeks per month). *Years of Schooling* is the highest reported completed grade as of 2009. *Potential Experience* is the age minus years of schooling minus 6, and *Experience Squared* is its square. The sample in each specification are limited to those who reported wage employment and are further defined as follows: columns (1) & (4) are "All" individuals who are employed, consistent with Montenegro and Patrinos (2014)'s sample definition, and columns (2)-(3) & (5)-(6) are "Limited" to adults aged 25-64 with 1-12 years of schooling to be consistent with Evans and Yuan (2019)'s sample used in Table A.7. Further, columns (3) & (6) include "Individual Controls" for age, age-squared, and sex. The last row shows each sample's estimated average annual income from a primary job, a secondary job, and ganyu labor in 2009 USD (following an exchange rate of 1 USD to 141 Malawian kwacha in 2009 (St. Louis Federal Reserve Bank, FRED Economic Data 2012)). Robust standard errors in parentheses.

G Long-Run Analysis

G.1 Data Details

Long-run outcome data come from Malawi's Integrated Household Panel Survey (IHPS) 2010-2016 and 2010-2019, both of which were also implemented as part of the World Bank Living Standards Measurement Study – Integrated Surveys on Agriculture (LSMS-ISA) initiative.[3] Citing an increasing number of households and budget/resource constraints, the IHPS tracked households from only half of baseline enumeration areas (EAs) starting in 2016. EA selection was stratified by region and urban/rural designation, and selection oversampled urban areas in order to secure reliable national estimates for both urban and rural areas (Government of Malawi 2017).

3. Documentation for 2010-2016 can be found at: https://microdata.worldbank.org/index.php/catalog/2939, and documentation for 2010-2019 can be found at: https://microdata.worldbank.org/index.php/catalog/3819.

Unfortunately, due to the reduced target sample and additional participant attrition, 2016 and 2019 data are only available for 43.5% and 38.6% of my sample. Additionally, retention into these surveys rounds from the IHPS 2010 is positively correlated with shift-share overlap. Regressing retention on shift-share overlap and controls (akin to Table 1's test of retention in the 2013 sample) produces a coefficient of 0.154 [RI p-value = 0.079] for 2016 retention and a coefficient of 0.124 [RI p-value = 0.123] for 2019 retention, suggesting slight over-sampling from locations that experienced greater overlap due to the school calendar change. To address both issues, I broaden my sample to include those ages 0-5 pre-policy who become school-aged in later years (ages 7-12 in 2016 and 10-15 in 2019). This sample includes 1,918 and 1,714 individuals in 2016 and 2016, respectively, and their retention is slightly less imbalanced. In the broader age 0-13 sample, shift-share overlap's correlation has a coefficient of 0.145 [RI p-value = 0.084] for 2016 retention and a coefficient of 0.116 [RI p-value = 0.136] for 2019 retention. Still, I interpret long-run results with some caution.

G.2 Additional Results

In Table 9, I present the results of overlap on $Farmed_i$ in 2016 and 2019 for three age groups: the full long-run sample ages 0-13 in 2009 (pre-policy); the new cohort who were ages 0-5 in 2009 (pre-policy) and became ages 7-12 in 2016 and 10-15 in 2019; and the main study sample who were ages 6-13 in 2009 and became ages 13-20 in 2016 and 16-23 in 2019. See brief description and summary of results at the end of Section 5.2.4.

Table A.9: **Long-Run Impacts on Farm Work**

	Full Long-Run Sample		**New Cohort**		**Children in Study Sample**	
	Pre-Policy Age 0-13		Pre-Policy Age 0-5		Pre-Policy Age 6-13	
VARIABLE: Farmed in	2016	2019	2016	2019	2016	2019
	(1)	(2)	(3)	(4)	(5)	(6)
Shift-Share Overlap$_\ell$	-0.054	-0.071	-0.099	-0.112	0.018	-0.009
	(0.049)	(0.056)	(0.068)	(0.079)	(0.070)	(0.076)
	[0.141]	[0.240]	[0.091]	[0.122]	[0.662]	[0.901]
Pre-Policy Outcome$_i$	-0.071	-0.053	0.055	-0.091	-0.018	-0.007
	(0.011)	(0.012)	(0.168)	(0.261)	(0.012)	(0.014)
Observations	1,918	1,714	929	834	989	880
R-squared	0.293	0.171	0.187	0.189	0.232	0.201
Age at Outcome	Age 7-20	Age 10-23	Age 7-12	Age 10-15	Age 13-20	Age 16-23
Pre-Policy Outcome Mean	0.65	0.62	0.01	0.00	1.25	1.20
Δ Outcome Mean (Post - Pre)	-0.15	-0.03	0.30	0.50	-0.56	-0.54

Notes: Specification and variables are as defined in Table 2. Dependent variable $Farmed_i$ was measured for a subset of individuals in follow-up panel surveys in either 2016 or 2019. Conventional robust standard errors in parentheses. Randomization inference p-values in square brackets.

H Sub-Saharan Africa Country-Level Analysis

This appendix provides data description and robustness checks for Figure 4, which shows across sub-Saharan African (SSA) countries[4] a negative correlation between overlap in the school and farming calendars and primary school survival rates. Specifically, it shows that as the overlapping percent of total school days plus total sowing/harvest days increases, the rate at which children who start school complete primary school decreases. Table A.10 summarizes the sources of the country-level data on school calendars, farming calendars, crop production and the outcome used for this analysis.

H.1 Data

School Calendars: To estimate the day-level overlap between school and farming calendars, I start by assembling a novel dataset of the daily primary and secondary school calendars for 82% (37) of SSA countries.[5] Table A.10 lists sources for school calendars. All school calendars start in either 2018 or 2019 and hence were announced well before and are not affected by the COVID-19 pandemic. Most school calendar data are taken from official announcements on government websites, Facebook pages, or correspondence with government officials. Detailed school calendar announcements include dates of all school holidays, breaks between terms, differences between primary and secondary schedules, and occasionally exam schedules. I also include school calendars from other reputable sources that provided start and end dates of major terms.

4. My target sample was 46 countries belonging to SSA according to the United National Development Programme, listed here: https://www.africa.undp.org/content/rba/en/home/regioninfo.html. Due to data limitations explained in this appendix, Guinea-Bissau, Seychelles, and South Sudan were unable to be included in the final sample.

5. I thank my amazing research assistants Danielle-Andree Atangana and Noelle Seward for their efforts in locating and digitizing school and farming calendars as well as Max Diaz, Flavia Lorenzon and Laston Manja for finding additional school calendars.

Table A.10: **Data Sources for Sub-Saharan Africa (SSA) Country-Level Analysis**

COUNTRIES	School Calendar	Crop Calendar	Production Data	Outcome Data	Included in Sample
Angola	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Benin	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Botswana	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Burkina Faso	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Burundi	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Cameroon	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Cape Verde	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Central African Republic	UIS Monthly	FAO/GIEWS	FAOSTAT	UIS	Yes
Chad	UIS Monthly	FAO/GIEWS	FAOSTAT	UIS	Yes
Comoros	UIS Monthly	WFP Report	FAOSTAT	UIS	Yes
Côte d'Ivoire	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Demc Repub of the Congo	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Equatorial Guinea	UIS Monthly	FAO CCT	FAOSTAT	UIS	Yes
Eritrea	UIS Monthly	FAO/GIEWS	FAOSTAT	UIS	Yes
Ethiopia	Reliable Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Gabon	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Gambia	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Ghana	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Guinea	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Guinea-Bissau	Official Daily	FAO/GIEWS	FAOSTAT	X	No
Kenya	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Lesotho	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Liberia	Reliable Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Madagascar	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Malawi	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Mali	Reliable Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Mauritania	Reliable Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Mauritius	Official Daily	FAO CCT	FAOSTAT	UIS	Yes
Mozambique	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Namibia	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Niger	UIS Monthly	FAO/GIEWS	FAOSTAT	UIS	Yes
Nigeria	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Republic of the Congo	UIS Monthly	FAO/GIEWS	FAOSTAT	UIS	Yes
Rwanda	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Sao Tome and Principe	UIS Monthly	FAO CCT	FAOSTAT	UIS	Yes
Senegal	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Seychelles	Official Daily	X	FAOSTAT	UIS	No
Sierra Leone	Reliable Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
South Africa	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
South Sudan	Official Daily	FAO/GIEWS	FAOSTAT	X	No
Swaziland / Eswatini	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Tanzania	Reliable Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Togo	Reliable Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Uganda	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Zambia	Official Daily	FAO/GIEWS	FAOSTAT	UIS	Yes
Zimbabwe	Reliable Daily	FAO/GIEWS	FAOSTAT	UIS	Yes

Notes: Target sample was 46 countries belonging to SSA according to the United Nations Development Programme. School Calendar: "Official Daily" is a daily calendar from a government announcement, "Reliable Daily" is a daily calendar from a reputable website or correspondence (e.g., in-country international schools), and "UIS Monthly" refers to monthly calendars from the UNESCO Institute for Statistics (UIS). Crop Calendar: "FAO/GIEWS" refers to country briefs written by the UN's Food and Agriculture Organization (FAO) Global Information and Early Warning System (GIEWS), "FAO CCT" refers to the FAO's Crop Calendar Tool, and the calendar for Comoros came from a World Food Programme (WFP) report. No crop calendar was found for Seychelles where farming is not common. All production data came from the FAO Statistics Division (FAOSTAT). All outcome data came from the UNESCO Institute for Statistics (UIS). Due to indicated data limitations, Guinea-Bissau, Seychelles, and South Sudan were unable to be included in the final sample.

As an example, Figure A.2 shows an excerpt of the official 2019/2020 school calendar for Malawi. For the few countries in which I was unable to locate a recent school calendar (after various attempts to contact government officials via email, phone, or third-parities), I use UNESCO UIS data for school calendar start month and end month in 2019 to estimate days in the school calendar. To do so, I estimate the average number of school days in each month as 5/7 of the number of days in the month in 2019 (i.e., not during a leap year) and assume that school operates for the start month, the end month, and each month in between.[6] I translate these school calendars into a vector $\{s_1, s_2, \ldots s_{365}\}$ of 365 indicator variables (each representing one day of the year) equal to one if school was scheduled on day d and zero otherwise. Further, I define country-level school requirements as $S = \sum_{d=1}^{365} s_d$, the total number of days during which school is scheduled.

Figure A.2: **Example of an Official School Calendar: Malawi**

The Primary, Secondary School and Teacher Training College academic year will comprise 41 weeks as follows:

2019/2020 ACADEMIC CALENDAR

TERM	OPENING	CLOSING	TOTAL WEEKS	HOLIDAY
1	16th September, 2019	20th December, 2019	14 weeks	2 weeks
2	6th January, 2020	3rd April, 2020	13 weeks	2 weeks
3	20th April, 2020	24th July, 2020	14 weeks	7 weeks end of year

Notes: The figure shows an excerpt of the official 2019/2020 school calendar for Malawi, which shows the daily schedule including breaks between terms. Not shown is the description of other school holidays.

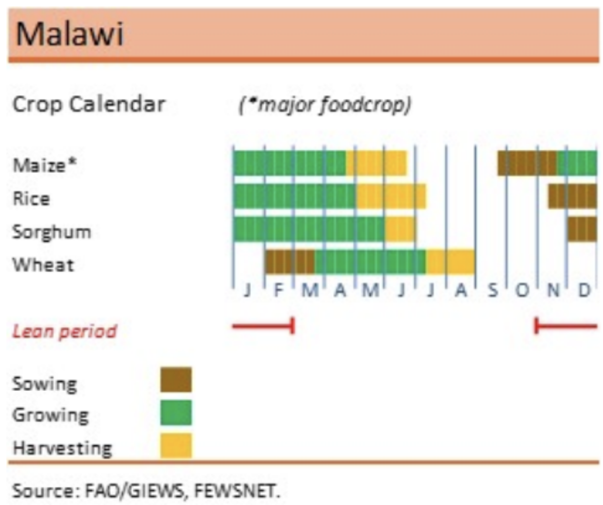
Farming calendars: Farming calendars are conceptualized as country-level crop calendars weighted by country-level crop production. Table A.10 lists sources for crop calendars. Crop calendars are mostly taken from "Country Briefs" written by the UN's Food and Agriculture Organization (FAO) Global Information and Early Warning System (GIEWS).[7] Each FAO/GIEWS crop calendar depicts the sowing, growing, and harvesting periods in roughly within-month 10-day increments for between three to seven locally important crops. Most crop calendars also highlight "major foodcrops", such as maize or cassava, specific to each country.

6. As this school calendar estimated with UIS data likely overestimates the total number of school days, I include an indicator for UIS-derived school calendars in regressions below to show the main finding is not driven by differences in data sources.

7. For three countries in which "Country Briefs" were not available, I use data on planting and harvest periods from the FAO's Crop Calendar Tool. Finally, the crop calendar for Comoros came from a World Food Programme (WFP) report: https://documents.wfp.org/stellent/groups/public/documents/ena/wfp085419.pdf.

As an example, Figure A.3 depicts the FAO/GIEWS crop calendar for Malawi. I translate these into vector $\{f_{c,1}, f_{c,2}, \ldots f_{c,365}\}$ of 365 indicator variables (each representing one day of the year) equal to one if either sowing and harvesting of crop calendar c occurs on day d and zero otherwise. Further, I define farming time requirements for crop calendar c as $F_c = \sum_{d=1}^{365} f_{c,d}$, the sum of the total number of days during which sowing or harvesting of the crop occurs. A crop calendar was not found for Seychelles where farming is not common, so Seychelles had to be excluded from the sample.

Figure A.3: **Example of FAO/GIEWS Crop Calendars: Malawi**



Notes: The figure depicts the most common source of crop calendars in the analysis. Crop calendars are mostly taken from "Country Briefs" written by the UN's Food and Agriculture Organization (FAO) Global Information and Early Warning System (GIEWS). Each FAO/GIEWS crop calendar depicts the sowing, growing, and harvesting periods in roughly within-month 10-day increments for between three to seven locally important crops. Most crop calendars also highlight "major foodcrops", such as maize or cassava, specific to each country. FAO's endorsement of this paper is neither stated nor implied.

Production Data: Crop production and land allocation data come from FAO Statistics Division (FAOSTAT). Production is measured in metric tons and land allocation in hectares. I use data only for crops with a crop calendar. In a few countries where some crops were assigned different calendars based on season or region, I assumed that land allocation and production were split 75% and 25% across primary and secondary growing seasons (respectively) and split evenly across regions.

Calculating Overlap: Using these data, I construct my measure of country-level overlap as follows. First, I calculate overlap for crop c as the product of the school and crop calendar indicators on day d, summed across all days to get the total number of days during which both school is scheduled and sowing/harvesting occurs – i.e., $overlap_c = \sum_{d=1}^{365}(s_d * f_{c,d})$. Second, I aggregate to the country-level by weighting crop-level farming time requirements F_c and $overlap_c$ by crop c's share of country-level production for listed crops. Finally, I divide the sum of country-level school and farming time requirements by country-level overlap to get Overlap Percent, the fraction of total school and farming days that overlap. Estimating overlap as a share of total schooling and farming time requirements, as opposed to just total number of days, effectively controls for possible correlations between length of school and/or farming calendar and the primary outcome.

Outcome Data: The primary outcome is the survival rate to grade 5 for both sexes from UNESCO's Institute for Statistics (UIS, 2022), which measures the fraction of a cohort of students enrolled in first grade who are expected to reach grade 5 of primary school.[8] Thus lower survival rates suggest lower level of retention and higher incidence of dropout within school. Additional outcomes I will test include survival rate to grade 4 and survival rate to the last grade of primary school, both from UIS, and also primary school complete rate from the World Development Indicators (WDI, 2022), but ultimately select survival rate to grade 5 as the primary outcome as the furthest-along consistent measure of primary school progress given that the number of grades in primary school varies across SSA countries. Furthermore, I look at overall, male, and female literacy rates for adults aged 25-64, who are very likely finished with their education.[9] Finally, I use the most recent data point available; data come from as recently as 2018, as far back as 2002, and from 2015 on average across countries. These data are not available for Guinea Bissau and South Sudan, which are hence excluded from the analysis.

Summary Statistics: Table A.11 presents summary statistics for the 43 countries for which the primary outcome data are available. Across these countries, the average survival rate to grade 5 is 71.6% (standard deviation of 17.8) and average adult literacy rate is 64.0% (standard deviation of 21.5). The Overlap Percent of total school and farming days has a mean of 22.8% (standard deviation of 4.5 percentage points), which is estimated by dividing the sum of total farm days and total school days by total overlap days. I use daily school calendars for 81% of sample countries. Other country-level variables show that 65% of countries are least developed countries, 35% are landlocked, and 9% are small island developing states. Descriptive statistics on last colonial status and region are also provided.

H.2 Analysis

Regression: Figure 4 visually depicts β in the following regression:

$$Y_j = \alpha + \beta \text{ Overlap Percent}_j + \varepsilon_j \tag{H.1}$$

where Y_j is the survival rate to grade 5 in country j; Overlap Percent$_j$ is the percent of country j's total school and farming time requirements that overlap as measured in days; and ε_j is an error term. I test the robustness of this result by also regressing Equation H.1 with the country-level controls summarized in Table A.11 as well as year fixed effects.

8. UIS calculates the outcome by using two consecutive years of enrollment data at each grade level to "reconstruct" a cohort's progression through primary school, and then divides the number of students expected to reach the last grade by the total number of the students in the cohort (i.e., those who originally enrolled in the first grade).

9. Adult literacy rates are calculated by 1) dividing the number of illiterate adults aged 25-64 from UIS by the estimated population aged 25-64 from UN World Population Prospects (United Nations, Department of Economic and Social Affairs, Population Division 2022) for the same country-year within each gender category to estimate a illiterate rate, and then 2) subtracting the illiteracy rate from 100 to obtain an estimate of the literacy rate.

Table A.11: **Summary Statistics for Sub-Saharan Africa Country-Level Analysis**

VARIABLE	N	Mean	SD	Min	Max
Survival Rate to Grade 4	43	77.73	15.15	37.55	98.50
Survival Rate to Grade 5	43	71.64	17.77	27.92	97.21
Survival Rate to Last Grade of Primary School	43	64.34	19.52	24.16	94.61
Primary Completion Rate	43	72.52	17.43	40.56	100.66
Adult Literacy Rate	43	63.96	21.52	21.04	95.38
Male Adult Literacy Rate	43	71.56	17.73	30.03	97.56
Female Adult Literacy Rate	43	56.66	25.89	12.26	92.82
Overlap Percent of School and Farm Days	43	22.75	4.51	14.57	31.82
Total Overlap Days between School and Farming	43	81.71	25.98	42.97	142.86
Total Farm Days	43	156.63	45.14	60.16	244.41
Total School Days	43	195.45	15.33	155.00	228.00
Indicator if Daily School Calendar was Found	43	0.81	0.39	0.00	1.00
Indicator if Least Developed Country	43	0.65	0.48	0.00	1.00
Indicator if Landlocked	43	0.35	0.48	0.00	1.00
Indicator is Small Island Developing State	43	0.09	0.29	0.00	1.00
Last Colonial Power: France	43	0.35	0.48	0.00	1.00
Last Colonial Power: Britain	43	0.35	0.48	0.00	1.00
Last Colonial Power: Other	43	0.30	0.46	0.00	1.00
Region: Eastern Africa	43	0.33	0.47	0.00	1.00
Region: Middle Africa	43	0.21	0.41	0.00	1.00
Region: Southern Africa	43	0.12	0.32	0.00	1.00
Region: Western Africa	43	0.35	0.48	0.00	1.00

Notes: Sample size of 43 (out of a possible 46) sub-Saharan African countries due to data availability. Summary statistics presented for outcomes, overlap measures, and other country-level variables. In the primary specification, the outcome of the survival rate to grade 5 of primary school is regressed on Overlap Percent, the sum of Total Farm Days and Total School Days then divided by Total Overlap Days.

To test for a correlation on similar outcomes, I adapt Equation H.1 by replacing Y_j with a country's survival rate to grade 4, survival rate to the last grade of primary school, and primary school completion rate, as shown in Table A.13. Finally, to test for a correlation with measure of literacy, I adapt Equation H.1 by replacing Y_j with the overall, male and female adult literacy rates, as shown in Table A.14.

Results: Regression results are presented in Table A.12. Column (1) shows the results visually depicted in Figure 4, finding that a one percentage point increase in the percent of total schooling and farming requirements that overlap is correlated with a 2.39 percentage point decline in an SSA country's survival rate to grade 5. With Overlap Percent$_j$ ranging from 14.6 to 31.8, the coefficient maps to a more than 40 percentage point gap across SSA countries in the survival rate to grade 5.

Table A.12: **Results for Sub-Saharan Africa Country-Level Analysis**

VARIABLES	Survival Rate to Grade 5						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Overlap Percent	-2.393***	-2.170***	-2.198***	-1.771**	-1.950***	-2.520***	-1.019**
	(0.527)	(0.604)	(0.605)	(0.664)	(0.556)	(0.660)	(0.403)
Observations	43	43	43	43	43	35	477
R-squared	0.370	0.718	0.812	0.784	0.818	0.837	0.541
Country-level Controls	N	Y	Y	Y	Y	Y	Y
Year FE	N	N	Y	Y	Y	Y	Y
Crop Shares Defined by	Metric Tons	Metric Tons	Metric Tons	Hectares	Metric Tons	Metric Tons	Metric Tons
Crop Selection	All Given	All Given	All Given	All Given	Major Food Crops	All Given	All Given
School Calendars Used	Best Given	Best Given	Best Given	Best Given	Best Given	Verified	UIS monthly
Standard Errors	Robust	Robust	Robust	Robust	Robust	Robust	Cluster
DV Mean	71.64	71.64	71.64	71.64	71.64	71.64	71.10
Overlap Mean	22.73	22.73	22.73	22.30	22.30	23.41	24.38

Notes: The table presents results of a regression of survival rate to grade 5 on Overlap Percent, the fraction of total school days and total farm days that overlap with each other, for 43 countries in sub-Saharan Africa. Column (1) shows the results visually depicted in Figure 4. Remaining columns show that the significant negative result is robust to other specifications: column (2) adds country-level controls, column (3) adds year fixed effects, column (4) define crop shares by land allocation (measured in hectares) instead of production, column (5) conceptualizes country-level farming calendars by only those crops listed by FAO/GIEWS as "major food crops", column (6) excludes countries defined by UIS-derived school calendars to assess the subsample of SSA countries with only verified daily school calendars, and column (7) analyze using only the UIS-derived school calendars as a panel from 1997 to 2018 with the same country-level controls and year fixed effects.

Remaining columns show that the significant negative result is robust to other specifications. Column (2) adds country-level controls. Column (3) adds year fixed effects. Column (4) define crop shares by land allocation (measured in hectares) instead of production. Column (5) conceptualizes country-level farming calendars by only those crops listed by FAO/GIEWS as "major food crops". Column (6) excludes countries defined by UIS-derived school calendars to assess the subsample of SSA countries with only verified daily school calendars. In Column (7), I analyze using only the UIS-derived school calendars; however, because these data are available for all countries in the sample annually from 1997 to 2018, I analyze the data as a panel including the same country-level controls and year fixed effects. These robustness checks build confidence in the relationship depicted in Figure 4.

Additionally, regression results on similar outcomes are presented in Table A.13. Outcomes include survival rate to grade 4 in columns (1)-(3), survival rate to the last grade of primary school in columns (4)-(6), and primary school completion rate in columns (7)-(9). In each set, the first column has no controls, the second adds country-level controls, and the third adds year fixed effects. Results are statistically significant in all but one regression, giving some assurance that the main finding is not specific to its outcome measure.

Finally, regression results on adult literacy rates are presented in Table A.14. Outcomes include the overall adult literacy rate in columns (1)-(3), the male adult literacy rate in columns (4)-(6), and the female adult literacy rate in columns (7)-(9). In each set, the first column has no controls, the second adds country-level controls, and the third adds year fixed effects. All coefficients are estimated with a negative sign, and those with the full set of controls in columns (3), (6) and (9) as well as in column (7) are statistically significant. The coefficient in column (1) is marginally significant (p-value of 0.131) and suggests that a one percentage point increase in the percent of total schooling and farming requirements that overlap is correlated with a 1.22 percentage point decline in an SSA country's overall adult literacy rate. With Overlap Percent_j ranging from 14.6 to 31.8, the coefficient maps to a more than 20 percentage point gap across SSA countries in the adult literacy rate. However, while the noisier estimates may be due to an uneven mapping of educational attainment to adult literacy across SSA countries (e.g., due to differences in school quality and curricula), it is also reason to interpret this correlation as mostly suggestive that overlap in the school and farming calendar may have impacts on human capital accumulation that persist into adulthood.

Table A.13: **Robustness of Sub-Saharan Africa Country-Level Analysis**

VARIABLES	Survival Rate to Grade 4			Survival Rate to Last Grade			Primary Completion Rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Overlap Percent	-2.017***	-1.922***	-2.018***	-2.457***	-2.172***	-2.210***	-1.189**	-0.941*	-0.637
	(0.466)	(0.569)	(0.634)	(0.560)	(0.613)	(0.715)	(0.533)	(0.464)	(0.584)
Observations	43	43	43	43	43	43	43	43	43
R-squared	0.361	0.695	0.759	0.323	0.721	0.763	0.095	0.652	0.729
Country Controls	N	Y	Y	N	Y	Y	N	Y	Y
Year FE	N	N	Y	N	N	Y	N	N	Y
DV Mean	77.73	77.73	77.73	64.34	64.34	64.34	72.35	72.35	72.35

Notes: The table presents results of different outcomes on Overlap Percent, the fraction of total school days and total farm days that overlap with each other, for 43 countries in sub-Saharan Africa. Outcomes include survival rate to grade 4 in columns (1)-(3), survival rate to the last grade of primary school in columns (4)-(6), and primary school completion rate in columns (7)-(9). In each set, the first column has no controls, the second adds country-level controls, and the third adds year fixed effects.

Table A.14: **Results on Literacy Rates for Sub-Saharan Africa Country-Level Analysis**

VARIABLES	Adult Literacy Rate			Male Adult Literacy Rate			Female Adult Literacy Rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Overlap Percent	-1.217	-0.711	-1.375**	-0.759	-0.693	-1.222**	-1.655*	-0.738	-1.527*
	(0.789)	(0.611)	(0.649)	(0.662)	(0.510)	(0.579)	(0.933)	(0.734)	(0.766)
Observations	43	43	43	43	43	43	43	43	43
R-squared	0.065	0.753	0.820	0.037	0.705	0.775	0.083	0.771	0.834
Country Controls	N	Y	Y	N	Y	Y	N	Y	Y
Year FE	N	N	Y	N	N	Y	N	N	Y
DV Mean	62.53	62.53	62.53	70.35	70.35	70.35	55.07	55.07	55.07

Notes: The table presents results of different literacy outcomes on Overlap Percent, the fraction of total school days and total farm days that overlap with each other, for 43 countries in sub-Saharan Africa. Outcomes include the adult literacy rate for those aged 25-64 in columns (1)-(3), the male adult literacy rate in columns (4)-(6), and the female adult literacy rate in columns (7)-(9). In each set, the first column has no controls, the second adds country-level controls, and the third adds year fixed effects.

References for the Online Appendix

Adão, Rodrigo, Michal Kolesár, and Eduardo Morales. 2019. "Shift-share designs: Theory and inference." *The Quarterly Journal of Economics* 134 (4): 1949–2010.

Ben-Porath, Yoram. 1967. "The Production of Human Capital and the Life Cycle of Earnings." *Journal of Political Economy* 75 (4): 352–365. ISSN: 00223808, 1537534X. http://www.jstor.org/stable/1828596.

Borusyak, Kirill, and Peter Hull. 2021. "Non-Random Exposure to Exogenous Shocks: Theory and Applications," Working Paper Series, no. 27845, https://doi.org/10.3386/w27845. http://www.nber.org/papers/w27845.

Borusyak, Kirill, Peter Hull, and Xavier Jaravel. 2022. "Quasi-experimental shift-share research designs." *The Review of Economic Studies* 89 (1): 181–213.

Evans, David, and Fei Yuan. 2019. "Equivalent years of schooling: A metric to communicate learning gains in concrete terms." *World Bank Policy Research Working Paper,* no. 8752.

Government of Malawi. 2012. *Malawi Integrated Household Panel Survey (IHPS) 2010-11 Basic Information Document.* Technical report. Malawi Government, National Statistical Office.

———. 2017. *Malawi Integrated Household Panel Survey (IHPS) 2016 Basic Information Document.* Technical report. Malawi Government, National Statistical Office.

Mincer, Jacob. 1958. "Investment in Human Capital and Personal Income Distribution." *Journal of Political Economy* 66 (4): 281–302. ISSN: 00223808, 1537534X. http://www.jstor.org/stable/1827422.

Montenegro, Claudio E, and Harry A Patrinos. 2014. "Comparable estimates of returns to schooling around the world." *World Bank policy research working paper,* no. 7020.

St. Louis Federal Reserve Bank, FRED Economic Data. 2012. *Exchange rate to U.S. dollar for Malawi.* https://fred.stlouisfed.org/series/FXRATEMWA618NUPN.

UNESCO, Institute for Statistics. 2022.

United Nations, Department of Economic and Social Affairs, Population Division. 2022. *World Population Prospects.* Technical report.

World Bank, World Development Indicators. 2022.

ALL IFPRI DISCUSSION PAPERS

All discussion papers are available **here**

They can be downloaded free of charge

INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE
www.ifpri.org

IFPRI HEADQUARTERS
1201 Eye Street, NW
Washington, DC 20005 USA
Tel.: +1-202-862-5600
Fax: +1-202-862-5606
Email: ifpri@cgiar.org