

Timing Matters: Estimating the Effects of Drought on School Attendance Across the Agricultural Cycle and School Year in Nigeria

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Abstract

Demographers have long recognized that the timing of events matters for understanding population processes. Building on this insight, this study examines two often overlooked dimensions of timing in research linking drought shocks to children's schooling in low-income agrarian contexts: exposure sensitivity, which arises when estimates differ across exposure windows within the agricultural calendar, and outcome sensitivity, which arises when estimates differ according to when schooling is measured relative to the shocks. The authors use panel data from Nigeria's Living Standards Measurement Study–Integrated Surveys on Agriculture (LSMS-ISA) linked to high-resolution Standardized Precipitation Evapotranspiration Index (SPEI) data and exploit repeated interviews at the start and midpoint of the school year. The authors find that the effects of droughts on school attendance vary with both the exposure window and the point in the school year when attendance is measured: droughts in the pre-planting period reduce start-of-year attendance, whereas planting-season droughts reduce mid-year attendance. These findings contribute to a broader demographic literature on the importance of timing for making sense of population processes, while also showing that identifying when shocks disrupt schooling requires attention to both which exposure window is used and when school attendance is measured.

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Introduction

Demographers have long recognized that the timing of events matters for understanding population processes. For example, research on fertility shows that delays or accelerations in the timing of births produce period fertility estimates that respectively underestimate (Bongaarts and Feeney 1998; Kohler and Ortega 2002) or overestimate (Parrado 2011) the quantum of fertility. Life course research similarly shows that the consequences of exposure to shocks depend on when exposure occurs relative to sensitive developmental stages, with well-documented evidence that exposure in utero or in early childhood has lasting effects on educational attainment and health outcomes (Almond 2006; Almond and Currie 2011; Lumey, Stein, and Susser 2011; Maccini and Yang 2009; Randell and Gray 2016). Building on this broader understanding of the importance of timing, this paper examines how estimates of drought on children's schooling depend on both when droughts occur and when schooling is measured. This question is demographically consequential given the central role education plays in shaping long-run demographic processes and socioeconomic outcomes, including mortality patterns (Gakidou et al. 2010), fertility behaviors and preferences (Behrman 2015; Bongaarts 2003; Caldwell 1980), migration trajectories (Massey et al. 1993), labor market participation (Schultz 1993), and intergenerational mobility (Mare 1981).

This emphasis on timing is particularly relevant in low-income agrarian settings where empirical evidence on the effects of climate shocks on schooling is mixed. Some studies report substantial declines in school attendance and attainment following droughts, rainfall deficits, and extreme temperatures (Agamile and Lawson 2021; Dhongde and Spyrou 2023; Joshi 2019; Prenhaca and Melo 2024; Randell and Gray 2019; Shah and Steinberg 2017), consistent with greater pressure for children to contribute to household livelihoods following climate-related

income losses (Zimmermann 2020). Others report insignificant effects or even modest increases (Calero, Bedi, and Sparrow 2009; Nordstrom and Cotton 2020; Randell and Gray 2019), suggesting that schooling may serve as an alternative when labor market opportunities decline following shocks. These inconsistent findings may reflect genuine differences in how households respond to climate shocks across diverse contexts. They may also arise because studies measure climate exposure over different periods and observe schooling at different points in the year—two sources of variation that have received less attention in prior research.

In this article, we use the term *temporal sensitivity* to refer to the variation in the effects of drought shocks on schooling that arises from when exposure and outcomes are measured. We distinguish two dimensions of temporal sensitivity. *Exposure sensitivity* occurs when estimates differ across points in the agricultural calendar, when labor demand, income flows, and food availability also vary seasonally. A drought during planting, for example, may affect school attendance differently from a drought during harvest, and therefore estimates may depend on the exposure window used. *Outcome sensitivity* occurs when estimates differ according to when attendance is measured relative to the shock. This is important because household surveys typically capture school attendance at a single point in the academic year, even though both the consequences of drought and school attendance can vary over time. The consequences of drought can unfold gradually as households adjust labor allocation, consumption, and schooling (Hill, Skoufias, and Maher 2019), while school attendance itself varies seasonally (Humphreys et al. 2015; Kumar and Saqib 2017).

In this paper, we examine how both dimensions of temporal sensitivity shape estimates of the effects of drought shocks on children’s school attendance in low-income agrarian contexts by exploiting an unusual feature of the Nigerian Living Standards Measurement Study–Integrated

Surveys on Agriculture (LSMS-ISA). In Waves 1 (2010–11) and 2 (2012–13), the same households were interviewed twice within each academic year—at the start and midpoint—and information on children’s schooling was collected at both visits. We link these child-level longitudinal data on schooling to high-resolution measures of climate variability and define sequential three-month exposure windows shifted weekly across the agricultural cycle. This design allows us to assess how drought shocks across the agricultural cycle affect school attendance at distinct points within the school year across years. Nigeria is a useful setting for this analysis because it is characterized by high rainfall variability, high reliance on rainfed agriculture, and rapid educational expansion. We find that conclusions about the effects of drought on schooling depend on both the exposure window within the agricultural cycle and when school attendance is measured.

Temporal sensitivity in the effects of climate shocks on schooling

In this paper, we examine two dimensions of temporal sensitivity – exposure sensitivity and outcome sensitivity – that may affect substantive conclusions about the effects of drought on schooling in low-income agrarian contexts. Although our empirical analysis focuses on drought, we draw on the broader literature on climate shocks and schooling to examine how existing studies define exposure windows and when they observe schooling outcomes and to discuss why exposure and outcome sensitivity matter. Throughout, we use the term “climate shocks” broadly to refer to both acute events such as droughts or floods and deviations in temperature or precipitation from local historical norms, consistent with how the term is used across the literature.

Exposure sensitivity

Much of the literature estimates the effects of climate shocks on schooling by linking climatic exposures defined over broad time windows to schooling outcomes measured at a single point in time (see Table 1 for a study-by-study summary). A large set of studies uses annual deviations in rainfall or temperature from long-run local means in the year of the survey or the one before, often expressed in logs, z-scores, or percentile thresholds (Adejuwon 2016; Björkman-Nyqvist 2013; Falaris 2023; Maddaloni et al. 2022; Shah and Steinberg 2017; Zimmermann 2020). Others use annual standardized or vegetation indices or counts of drought months within a fixed window (e.g. months-in-drought in the exam/census year) (Branco and Féres 2018; Joshi 2019; Prenhaca and Melo 2024). Difference-in-differences studies typically define exposure either over the interval between survey waves, which may span multiple years, or over 1–3-month disaster windows (Arceo-Gómez, Hernández-Cortés, and López-Feldman 2020; Baez and Santos 2020; Berhane, Zaitchik, and Badr 2015; Joshi 2019). A distinct cluster of studies instead relies on self-reported exposure to droughts or floods over fixed recall periods (e.g. six months, one year, or four years) (Berhane et al. 2015; Calero et al. 2009; Hyder Baloch and Behrman 2016; de Janvry et al. 2006; Nguyen and Pham 2018; Woldehanna and Hagos 2015). These approaches capture broad departures from local climatic conditions, but windows that span several months or years can combine shocks occurring at different stages of the agricultural cycle and thereby obscure important season-specific pathways linking climate shocks to schooling.

Many studies attribute the effects of climate shocks on schooling to changes in agricultural income or crop yields (Björkman-Nyqvist 2013; Joshi 2019; Shah and Steinberg 2017; Zimmermann 2020). However, exposure windows that span multiple agricultural seasons may combine periods in which the schooling response differs. This matters because children’s school attendance is tied to seasonal labor demands in agrarian contexts, and children are commonly

pulled out of school to help with harvesting in the absence of climate shocks (James 2024). A drought that reduces yields could therefore *increase* school attendance, because there is less harvesting to do and therefore less need for children’s labor. In contrast, a drought during planting seasons could *reduce* school attendance if households need extra labor to replant or must divert resources from schooling to agricultural inputs to secure a future harvest. Shocks at different points in the agricultural calendar could have effects on school attendance that run in opposite directions. Averaging exposure across seasons can combine these period-specific effects leading to estimates that differ from those for particular stages of the agricultural cycle. We refer to this as exposure sensitivity, when effects vary across exposure windows within the agricultural cycle in a way that is consistent with seasonal variation in household vulnerability.

The notion that the timing of climate shocks matters for their impacts parallels the life-course literature, which emphasizes that the consequences of exposure depend on when events occur relative to sensitive developmental stages. In this work, it is widely recognized that certain periods – such as in-utero or early childhood – are particularly sensitive for long-term outcomes (Almond 2006; Almond and Currie 2011; Lumey et al. 2011). Research on climate and schooling has leveraged this insight by examining exposure from gestation through adolescence and identifying developmental periods of increased vulnerability (Bazan 2023; Groppo and Kraehnert 2016; Nubler et al. 2021; Randell and Gray 2016, 2019; Shah and Steinberg 2017). However, less attention has been given to examining how the same climate shock has different effects depending on when it occurs within the agricultural cycle.

Few studies directly compare alternative exposure windows. Instead, studies often choose a season a priori – one assumed to matter more – and define exposure over that period. For example, Agamile and Lawson (2021) and Marchetta, Sahn, and Tiberti (2019) examine rainfall

deviations in the first agricultural season and in the rainy season, respectively; Dhongde and Spyrou (2023) and Nordstrom and Cotton (2020) focus on growing-season rainfall. While these choices reflect plausible assumptions about when climate shocks may matter more, they are rarely evaluated against alternative exposure windows. Because effects may differ across exposure windows, comparing seasonally defined exposure windows can reveal how substantive conclusions depend on which exposure window is used.

The study that comes closest to our approach is that of Randell and Gray (2016) who examine rainfall and temperature anomalies from the in utero period through the school ages across Ethiopia's three agricultural seasons. This study shows that milder temperatures in all three agricultural seasons are associated with better schooling outcomes, but rainfall during the summer rainy season – as opposed to the spring rainy season or summer dry season – is particularly consequential for school attendance, highlighting the need for more work that more explicitly examines exposure sensitivity. Related evidence from health research points to similar season-specific vulnerabilities: droughts during *meher* (main rainy) season significantly increase the risk of child stunting in Ethiopia, while droughts during other seasons do not (Dimitrova 2021). Our approach builds on this literature by reframing agricultural timing as a central measurement issue. Rather than selecting a single season a priori, we explicitly examine exposure sensitivity by systematically comparing sequential three-month exposure windows shifted weekly across the agricultural cycle to investigate how the effects of drought on schooling vary across exposure windows. This inductive approach allows us to examine droughts during conventionally salient periods such as planting and harvest, as well as droughts outside these periods that may still affect household resources and schooling decisions.

Outcome sensitivity

Studies of climate shocks and schooling typically do not account for within-year changes in attendance and often rely on a single observation within the school year (Table 1). Most commonly, schooling outcomes are observed at a single point in time – either at the survey interview (typically with a short recall window for attendance) or on a fixed administrative date (start-of-year enrolment or national exam month) (Björkman-Nyqvist 2013; Branco and Féres 2018). Several studies on school attendance use surveys that are fielded continuously over 12 months (Baez and Santos 2020; Zimmermann 2020) or pool surveys conducted in different months across years (Arceo-Gómez et al. 2020; de Janvry et al. 2006), combining observations from different points in the academic and agricultural calendars. Other studies use surveys fielded during a short window (e.g., 3-month) within one agricultural period (e.g., rainy season) (Agamile and Lawson 2021; Falaris 2023; Randell and Gray 2016) so they cannot show whether the effects of climate shocks on schooling are similar when schooling is observed at other times of the year. Because most studies rely on cross-sectional data or longitudinal data in which respondents are observed only once per year, little attention is paid to whether effects differ depending on when schooling is observed during the academic year relative to climate shocks. When schooling is observed at different points relative to the shock, the effects may differ as immediate and delayed responses unfold over time. We refer to this as *outcome sensitivity*, which we examine in greater detail below.

Climate shocks can generate both immediate disruptions to household income and labor demands and delayed effects that unfold as economic stress accumulates over time through food insecurity and rising demand for child labor (Hill et al. 2019). Households may initially buffer shocks by using savings, borrowing, selling assets, or informal support (Morduch 1995; Townsend 1994), postponing more disruptive responses. Evidence from agrarian settings suggests that

households may nevertheless be unable to fully smooth seasonal shocks (Behrman, Foster, and Rosenzweig 1997). As these short-term strategies are exhausted, impacts such as reduced consumption, increased debt, or children's school withdrawal may only emerge months later—for example, when liquidity constraints bind during the harvest period after a planting-season shock (Hill et al. 2019). The effect of the same climate shock on schooling may therefore differ depending on whether attendance is observed soon after the shock or several months later.

The Nigerian context

Nigeria, the most populous country in Africa, provides a compelling context for examining the impacts of climate shocks on schooling. Agriculture remains a central pillar of the economy and a primary source of employment, with approximately 42–45 percent of the labor force engaged in agricultural activities during the period of the study in the early 2010s (World Bank 2020). Agricultural production is overwhelmingly rainfed (Cervigni, Valentini, and Santini 2013), making rural livelihoods highly sensitive to climate variability, particularly fluctuations in seasonal rainfall. During the period covered by our study, Nigeria experienced delayed rainfall onset, dry spells, and episodic flooding that disrupted agricultural production (FAO 2024; FEWS NET 2013; GIEWS 2012). For example, in 2010 late rains and prolonged dry spells affected some northern areas while other areas experienced flooding; in 2012 rainfall was generally favorable but floods caused widespread losses (FAO 2024; GIEWS 2012). In 2013 instead rainfall deficits affected much of the country and harvests were below average in several regions (FEWS NET 2013; NOAA 2013).

Nigeria's predominantly rainfed agriculture follows a distinctly seasonal agricultural calendar, with planting and crop growth concentrated in the rainy season and harvest mainly in the

dry season. Planting typically occurs from March to July, and harvesting generally takes place from September to December (USDA 2025). The precise timing can vary across regions and crops (Shiru et al. 2019). For example, corn planted in the North is harvested as early as August, corn in the South may be planted as early as March and harvested in July–September, and wheat is planted in November and harvested in March.

School participation in Nigeria also varies with the agricultural calendar, particularly in rural areas where children contribute to household labor and households face education-related expenses. The school year runs from September to June, with attendance often declining during periods of peak labor demand or financial strain such as planting and harvesting seasons (Hadley 2010). Although basic education is formally free and compulsory for children ages 6–15 under the Universal Basic Education Act of 2004, participation remains uneven. Primary and secondary school completion rates are substantially lower in rural and northern areas, and households continue to pay for uniforms, supplies, transportation, and school contributions (Jenkins, Amala Anyabolu, and Bahramian 2019; UNICEF 2023). These costs make schooling sensitive to changes in household resources (Jenkins et al. 2019; Lincove 2009), consistent with evidence elsewhere in Africa that attendance responds to income shocks (Grimm 2011).

In this paper, we use climate conditions measured across the agricultural cycle preceding the academic year (from the harvest period through planting and the months immediately before the start of the school year) and school attendance at two different times in the academic calendar (the start and midpoint of the school year). Droughts during harvest can reduce crop output, income, and food stocks carried into the next year. Droughts during planting can trigger crop failure (Amondo, Nshakira-Rukundo, and Mirzabaev 2023), increase labor and input costs through replanting, and signal a poor harvest later in the year. Climatic conditions immediately before the

school year when households make key schooling decisions and incur education-related expenses may also matter, and reduced household income during this period may reduce school participation, particularly at the start of the school year. We assess school attendance at two points—the start and midpoint of the academic year—to examine whether the effects of drought differ depending on when attendance is measured. Together, our analyses provide an empirical test of how exposure and outcome sensitivity may shape substantive conclusions about the effects of droughts on school attendance in Nigeria.

Data and measures

We combine georeferenced, child-level data on school attendance from two waves of the Nigeria Living Standards Measurement Study – Integrated Surveys on Agriculture (LSMS-ISA) with data from the Standardized Precipitation Evapotranspiration Index (SPEI), a widely used measure of wet and dry conditions. SPEI values come from the Global Droughts Crops Monitoring dataset (Vicente-Serrano et al. 2023). In what follows we describe the survey and climate data in turn.

Individual-level data: LSMS-ISA project

Our individual-level data come from the Nigeria LSMS-ISA, a nationally representative household panel with a strong focus on agriculture production and rural livelihoods. In Nigeria, the LSMS-ISA expands the General Household Survey¹ (GHS) with a panel component of 5,000 households randomly selected across all the 36 states and the Federal Capital Territory, Abuja. For this study, we use Wave 1 (2010–11) and Wave 2 (2012–13), the only two waves that consistently include the school attendance questions required for our analysis. In each wave, households are

¹ The GHS is an annual survey carried out in February–March throughout the country on a sample of 22,000 households.

interviewed twice: at the start of the school year (Visit 1, September–October) and at the midpoint (Visit 2, February–March).² Across the two waves, this gives us four interview rounds: September–October 2010, February–March 2011, September–October 2012, and February–March 2013. Household locations are georeferenced, allowing us to link respondents to local climatic conditions.³

Our primary outcome variable is a binary measure of school attendance observed at each survey visit based on the timing of the survey visit within the academic year. Table 2 summarizes the timing and wording of the survey questions across waves and visits. Both questions refer to school participation in the same school year, but their wording slightly differs: Visit 2 directly measures school attendance at the midpoint of the school year, whereas Visit 1 asks whether the child plans to attend school that year. However, because nearly all Visit 1 interviews occurred after the school year had begun, we can confidently use this response as a measure for start-of-year attendance. Although the LSMS-ISA is a household survey, information on school attendance is collected for each household member aged 6–15; the attendance questions are not administered to children younger than age 6. For children younger than 12, a parent or guardian reports attendance; older children report for themselves. As a secondary outcome, we use a binary indicator of whether the child has worked in any capacity during the previous seven days.⁴ We also use age, sex, and region as controls and for the heterogeneity analyses.

² Visit 1 surveys began in late August and nearly all interviews (i.e., 98.0% in Wave 1 and 95.9% in Wave 2) were completed in September and October. Visit 2 surveys began in late January, with most interviews completed in February and March (i.e., 94.8% in Wave 1, 95.4% in Wave 2). The remaining households were interviewed as soon as was feasible after the original date of data collection, with some cases completed up to one year later. For simplicity, we refer to these periods as September–October for Visit 1 and February–March for Visit 2, respectively, which represent the months when nearly all interviews were conducted.

³ For confidentiality, GPS coordinates are randomly displaced by 0–2 km in urban areas and 0–5 km in rural areas (up to 10 km for 1% of rural cases).

⁴ The labor questions are asked for each household member aged 5 or older and may be answered by the individual or by another household member on the individual’s behalf. Children are coded 1 for labor activity if any of the following three questions is answered affirmatively for that child: “During the past 7 days, have you worked for someone who

Our unit of analysis is children aged 6–15 observed in both waves. We construct a pooled sample that combines their observations across the visits. Because the same children are observed at the start and midpoint of the school year in two separate waves, the panel structure allows us to compare within-child changes in attendance across different climate exposures while accounting for time-invariant individual characteristics. The final sample consists of 4,190 children from 2,171 households, with each child observed in all four survey rounds: Visits 1 and 2 of Waves 1 and 2 yielding 16,760 child-visit observations in total.⁵

We identify two potential problems in tracking children across survey rounds. First, for some children, the change in age across surveys does not always match the number of years between surveys which may indicate age-reporting errors or errors in linking children’s records across rounds. Second, attrition may occur when households are not reinterviewed or when children move or are temporarily absent between rounds. These concerns may affect our results if households affected by drought are more likely to drop out of the panel or if exposure to drought is associated with errors in linking children across rounds. To assess the sensitivity of our results, we re-estimate models (i) among respondents whose reported age changes consistently across survey rounds and (ii) using household- rather than individual-fixed effects. The results remain robust to these alternative specifications (see Figures A1 and A2 in the Supplementary Materials).

is not a member of your household, for example, an enterprise, company, the government or any other individual?” “During the past 7 days, have you worked on a farm owned or rented by a member of your household, either in cultivating crops or in other farming tasks, or have you cared for livestock belonging to yourself or a member of your household?” “During the past 7 days, have you worked on your own account or in a business enterprise belonging to you or someone in your household, for example, as a trader, shop-keeper, barber, dressmaker, carpenter or taxi driver?”

⁵ Of the 4,924 households in the original sample, 3,702 have children aged 6–15 with non-missing information on sex. When we restrict the sample to only children with non-missing information on attendance at all four survey rounds leaves 2,171 households in the analytic sample.

Climate data: SPEI from the Global Droughts Crop Monitoring Dataset

We retrieve the SPEI values for each respondent's household location from the Global Droughts Crop Monitoring Dataset. SPEI is widely regarded as the most complete and robust agricultural drought index in Africa (WMO 2012) that captures anomalies in the climatic water balance (Vicente-Serrano, Beguería, and López-Moreno 2010). The Global Droughts Crop Monitoring Dataset, which is based on the ERA5 reanalysis data, provides weekly SPEI values since January 1979 at a 0.5° spatial resolution.

There are four main advantages to using SPEI and the Global Drought Crops Monitoring dataset. First, SPEI is a standardized index allowing comparisons of dry and wet conditions across locations and time. Second, unlike precipitation-only indices, SPEI incorporates atmospheric demand for moisture through evapotranspiration which reflects the combined effects of temperature, wind speed, and other weather conditions, making it superior to other indices of dry and wet climatic conditions (Vicente-Serrano et al. 2010). Third, SPEI is a multiscalar index and can therefore be calculated over different time scales to capture short-, medium-, and long-term moisture conditions. Finally, the Global Drought Crops Monitoring dataset provides SPEI at multiple time scales (0.5, 1, 3, 6, 9, 12, 24, 36 and 48 months), allowing us to select the most relevant periods for our analysis.

In the main analysis, we use SPEI calculated over a three-month time scale (SPEI3) which captures short-term moisture conditions reflecting precipitation-evapotranspiration anomalies over the preceding three months. This scale approximates the duration of several key phases of the agricultural cycle, making it well suited to examining how climatic conditions occurring in different agricultural seasons affect schooling outcomes. By construction the SPEI3 has a mean of 0, with values below -1 indicating moderate-to-extreme dry conditions and values above +1

indicating moderate-to-extreme wet conditions. We assign climatic conditions to respondents by averaging SPEI3 across all grid cells within a 10-km buffer around each household's GPS coordinates to create a continuous measure of local moisture anomalies.⁶

To assess exposure sensitivity, we construct 37 three-month SPEI windows spanning the agricultural cycle before the school year. For each wave with Visit 1 beginning in September of year t , the windows are shifted weekly (to the 8th, 15th, 22nd, and last day of each month), from Interval 0 in the early harvest season, from September 1 to November 30, to Interval 36, from June 1 to August 31. Based on the agricultural calendar of Nigeria, we expect the most relevant windows to overlap harvest (i.e., Intervals 0 to 8 or Sept–Nov to Nov–Jan; though it can run later), planting (i.e., Intervals 20 to 32 or Feb–Apr to May–Jul; though it can start sooner), and the months immediately before the school year (i.e., Interval 33 to 36 or Jun–Aug). Figure 1 illustrates the exposure windows relative to the timing of the survey rounds.

For each interval, we use both a continuous measure of SPEI3 as well as a binary indicator for moderate-to-extreme dry conditions ($\text{SPEI3} \leq -1.0$). The continuous measure captures standardized moisture anomalies across the full SPEI3 distribution, while the binary measure isolates more severe drought conditions. In supplementary analyses, we test the robustness of our results to alternative SPEI thresholds for dry (≤ -0.5 , ≤ -1.5) and wet ($\geq +0.5$, $\geq +1.0$, $\geq +1.5$) conditions and to six-month SPEI (SPEI6) windows; the main conclusions remain consistent (Figures A3 and A4 in the Supplementary Materials).

⁶ This buffer accounts for the random displacement of GPS coordinates of households applied to protect respondent confidentiality and is likely sufficiently large to capture both respondents' dwellings and their land (e.g., agricultural plots).

Empirical model

Our empirical strategy is designed to identify and examine both dimensions of temporal sensitivity. We estimate an individual fixed-effects linear probability model that takes the following form:

$$\begin{aligned} \text{Eq. (1)} \quad y_{iwt} = & \beta_1 SPEI3_{it}^s + \beta_2 (SPEI3_{it}^s \times V_v) + \beta_3 V_v + \beta_4 AGE_{iwt} + \beta_5 (AGE_{iwt} \times V_v) + m_i \\ & + g_w + e_{iwt} \end{aligned}$$

where y_{iwt} is a binary indicator of school attendance for individual i in wave t measured at visit v . The key independent variable, $SPEI3_{it}^s$, measures local climatic conditions during exposure interval s . We estimate Eq. (1) separately for each of the 37 exposure intervals (Interval 0 to Interval 36). V_v is an indicator equal to 1 for Visit 2 and 0 for Visit 1. β_1 estimates the effect of SPEI3 on school attendance at Visit 1, while β_2 estimates how this effect differs at Visit 2. The inclusion of individual fixed effects, μ_i , allows us to control for all time-invariant individual, household, and community characteristics, so identification comes from within-individual changes in climate conditions across waves. Wave fixed-effects, γ_w , account for changes across waves. We also include a time-varying control for the respondent's age and interact age with the visit indicator to allow the association with attendance to differ between visits. Standard errors are clustered at the household level.

Because the main analysis restricts exposure windows to periods that precede both survey visits, the September–December harvest windows correspond to the harvest season preceding the current school year, leaving a long interval before Visit 2 in February–March. To examine harvest conditions that occur closer to Visit 2, we extend the exposure windows through December of the current school year (Intervals 37–52). Because these additional exposure windows occur after Visit

1, we estimate their effect on Visit 2 attendance only, using the same model in Eq. (1) without the visit indicator or its interaction. More details are provided in the Supplementary Materials.

Results

Descriptive results

Table 3 presents weighted and unweighted descriptive statistics for the key variables used in the analysis⁷. On average, 86% of respondents reported attending school at the start of the academic year during Visit 1, whereas 84% reported being in school at the midpoint during Visit 2 (paired t-test: $t = 5.4, p < 0.001$). Among respondents attending at Visit 1, 7.7% are not attending at Visit 2; among those not attending at Visit 1, 29.9% are attending at Visit 2. These differences between the start and midpoint of the academic year show that school attendance is not stable throughout the school year and changes for a substantial share of children in our sample.

Figure 2 plots the distribution of SPEI3 values by wave for four intervals spanning the agricultural cycle: Interval 0 (Sept–Nov), Interval 12 (Dec–Feb), Interval 24 (Mar–May), and Interval 36 (Jun–Aug) which correspond approximately to early harvest, pre-planting, planting, and the period immediately before the school year. The distributions of the SPEI3 are broadly similar across the two waves, but dry conditions are more common in Wave 1 around harvest (Interval 0 and 12) whereas wet conditions are more common in Wave 2 immediately before the school year (Interval 36). The distributions are centered around zero, indicating average conditions, but both negative and positive moisture anomalies occur across the agricultural cycle, with some clustering around the dry threshold ($\text{SPEI} \leq -1$) during planting (Interval 24).

⁷ The weights used for the summary statistics are the wave-specific household weights provided in the dataset, which adjust the sample to be representative at the population level. For the weighted descriptive statistics, we use the provided household weights for each individual in the household.

Regression results: exposure and outcome sensitivity

Figure 3 reports how the effects of climatic conditions on school attendance vary across exposure windows within the agricultural cycle and between attendance measured at the start and midpoint of the school year. The x-axis corresponds to the sequential SPEI3 intervals, from Interval 0 (September 1–November 30) to Interval 36 (June 1–August 31). The top panel presents separate estimates for Visit 1 (green) and Visit 2 (purple); the bottom panel presents the difference between the coefficients for Visit 2 and Visit 1. The left panels use a binary drought indicator ($\text{SPEI3} \leq -1.0$), and the right panels use continuous SPEI3. The figure reports coefficient estimates with 95% confidence intervals and highlights estimates statistically different from zero.

We start by assessing exposure sensitivity in the top row of Figure 3. First, examining the effect of the binary drought measure ($\text{SPEI3} \leq -1.0$; left panel) on school attendance at the start of the school year (green), we find no evidence of a significant effect of droughts during early harvest (i.e., Intervals 0–8, Sep 1–Nov 30 to Nov 1–Jan 31) or planting (i.e., Intervals 22–31, Feb 16–May 15 to Apr 23–Jul 22). By contrast, droughts during late harvest and pre-planting period (i.e., Intervals 9–21, Nov 9–Feb 8 to Feb 9–May 8) and immediately before the school year (i.e., Intervals 32–36, May 1–Jul 31 to Jun 1–Aug 31) have a negative impact on school attendance at the start of the school year. The largest declines are -4.8 percentage points ($p < 0.001$) in Interval 12 (Dec 1–Feb 28) and -5.0 percentage points ($p < 0.001$) in Interval 33 (May 9–Aug 8). These results provide evidence of exposure sensitivity, with the largest coefficients occurring outside the periods that might conventionally be considered most relevant.

For school attendance at the midpoint of the school year (Visit 2; purple), the effects of droughts during the planting season (Intervals 22–28; Feb 16–May 15 to Apr 1–Jun 30) are

consistently negative and different from zero. The largest effect occurs in Interval 26 (Mar 16–Jun 15), where school attendance declines by -4.5 percentage points ($p < 0.001$). These results provide further evidence of exposure sensitivity at the midpoint of the school year.

Similar patterns emerge when exposure is measured using continuous SPEI3 rather than the binary drought indicator, although significant effects are evident across a broader range of intervals. For Visit 1 (green), a one-unit increase in SPEI3 – indicating more favorable moisture conditions – across Intervals 5–27 (Oct 9–Jan 8 to Mar 23–Jun 22) increases school attendance by $+2$ to $+2.5$ percentage points with peaks in Interval 5 ($+2.5$ pp, $p = 0.003$), Interval 6 ($+3.2$ pp, $p < 0.001$), Interval 10 ($+2.6$ pp, $p < 0.001$), Interval 17 ($+2.5$ pp, $p < 0.001$), and Interval 23 ($+2.5$ pp, $p = 0.01$); effects are negligible when SPEI3 is measured outside these windows in Intervals 0–4 and 28–36. For Visit 2 (purple), higher SPEI3 values in windows overlapping the planting season (Intervals 17–30; Jan 9–Apr 8 to Apr 16–Jul 15) significantly increase school attendance, with effects peaking at approximately $+4.3$ percentage points ($p < 0.001$) in Interval 27 (Mar 23–Jun 22). The continuous measure broadly reinforces the pattern observed for the binary drought indicator that the effect on school attendance depends on the exposure window. One exception occurs immediately before the school year (Intervals 32–36): the binary drought indicator is statistically significant, whereas SPEI3 is not. Supplementary analyses help explain this discrepancy by showing that very-to-extremely wet conditions ($\text{SPEI} \geq +1.5$) during these intervals reduce Visit 1 school attendance (Figure A3). This combination of negative estimates at both dry and wet thresholds is consistent with a U-shaped relationship which can lead to a small average slope for continuous SPEI3.

Because the harvest windows (Intervals 0–4) in our analysis precede Visit 2 by many months, we test whether more recent harvest conditions matter for midpoint attendance. In a

supplementary analysis, we extend the intervals through September–December of the current school year (Intervals 37–52) and estimate their effects with Visit 2 attendance (Figure A5 in the Supplementary Materials). The effects of these more recent windows are also negligible.

Having shown that the effects vary substantially across exposure windows, the bottom panel of Figure 3 tests outcome sensitivity by comparing Visit 2 and Visit 1 coefficients. Droughts (left panel) in exposure windows beginning during late harvest and pre-planting (Intervals 9–21; Nov 9–Feb 8 to Feb 9–May 8), as well as immediately preceding the school year (Intervals 31–36; Apr 23–Jul 22 to Jun 1–Aug 31), have larger negative effects at Visit 1, with differences of 3 to 4 percentage points with peaks in Interval 12 (difference of 4.4 pp, $p < 0.001$), Interval 13 (difference of 4.0 pp, $p < 0.001$), Interval 33 (difference of 4.3 pp, $p < 0.001$), and Interval 34 (difference of 4.0 pp, $p = 0.001$). By contrast, droughts during mid-planting (Intervals 24–25; Mar 1–May 31 and Mar 9–Jun 8) have larger negative effects at Visit 2, with differences of 3.5 ($p = 0.003$) and 2.6 ($p = 0.029$) percentage points in Intervals 24 and 25.

The continuous SPEI3 results provide complementary evidence of outcome sensitivity (bottom right panel). More favorable moisture conditions in exposure windows beginning during harvest (Intervals 6–13; Oct 16–Jan 15 to Dec 9–Mar 8) increase Visit 1 attendance relative to Visit 2 by up to 2.4 percentage points ($p = 0.003$) in Interval 8 (Nov 1–Jan 31), whereas more favorable moisture conditions during mid-to-late planting (Intervals 24–31; Mar 1–May 31 to Apr 23–Jul 22) increase Visit 2 attendance relative to Visit 1 by up to 2.6 percentage points ($p = 0.002$) in Interval 24 (Mar 1–May 31). These results provide evidence of outcome sensitivity: the effect of climatic conditions in the same exposure window depends on when during the school year attendance is measured.

Regression results: heterogeneity analyses

A central explanation in the literature for a negative effect of droughts on school attendance is that droughts affect school attendance by changing household labor demand and children's participation in work. We therefore examine labor participation as a secondary outcome to assess whether the attendance patterns are accompanied by changes in children's labor. Figure 4 presents the same analysis as Figure 3 but uses labor participation during the previous week as the outcome variable. Among children who report working, 89% report work on a farm owned or rented by a household member. The effects generally mirror those for school attendance: drought conditions in intervals that predict lower school attendance tend to predict higher labor participation, while less dry or wetter conditions that predict higher school attendance tend to predict lower labor participation. These patterns are consistent with children's labor as one pathway through which drought may affect school attendance.

Prior research on intrahousehold allocation shows that the effects of seasonal resource constraints can differ across children by age and sex (Behrman 1988b, 1988a). We thus assess differential vulnerability to drought across groups of children by examining whether the effects differ by age and sex (Figures A6 and Figure A7 in the Supplementary Materials).⁸ These results suggest that our findings are concentrated among younger children, especially ages 6–7 and 8–10. While the trends are similar for older children ages 11–15, they generally have smaller and less precisely estimated effects. Similarly, the effects of drought are more consistently negative among female than male children. These findings may reflect more stable labor responsibilities among older boys, whereas school attendance among younger children and girls may be more responsive to changes in household labor needs as climatic conditions change.

⁸ For the analyses in this section, we extend Eq. (1) to allow the effects of SPEI3 at each visit to vary across groups.

Next, we assess whether the patterns of exposure and outcome sensitivity are driven by regional differences in agricultural timing (Figure A8 in Supplementary Materials). We distinguish North Central, North East, North West, and a South region that combines South West, South East, and South South.⁹ The regional results show that exposure sensitivity is not simply a consequence of pooling regions: within regions, the effects continue to vary across exposure windows although the windows with the largest effects differ across regions. We also find evidence of outcome sensitivity, which is clearest in North Central and the South, where the effects of climatic conditions differ between attendance measured at the start and midpoint of the school year. Estimates for the North East and North West are less precise and show less consistent differences between visits.

Discussion and conclusions

This study examines two often overlooked dimensions of temporal sensitivity in research linking climate shocks to children’s schooling in low-income agrarian contexts. First, exposure sensitivity refers to variation in effects depending on when climate shocks are measured within the agricultural calendar. Second, outcome sensitivity refers to variation in effects depending on when schooling is measured relative to the occurrence of climate shocks. The Nigeria panel dataset allows us to examine both by combining sequential exposure windows with repeated attendance measures within the same school year.

Our first finding is that exposure sensitivity matters. Drought or moisture deficits – captured by both the binary indicator and continuous SPEI3 – during planting reduce school attendance at midyear, whereas climate conditions in other periods have no effects. Dry conditions

⁹ We combine the three Southern regions into a single South region to preserve statistical power.

during planting are likely especially consequential because they can trigger crop failure, increase replanting costs, and provide signals of income shortfalls later in the year, potentially prompting households to adjust schooling decisions in advance of the academic year (Amondo et al. 2023; Haile, Gizaw, and Biazin 2022). Likewise, we find that drought or moisture deficits during the pre-planting period reduce school attendance at the start of the academic year. This finding is particularly noteworthy because pre-planting conditions were not among the periods we initially expected to matter most which underscores the value of an inductive approach that does not impose strong assumptions about when drought conditions matter. One possible explanation is that drought conditions before planting constrain household resources or signal poor agricultural prospects, leading households to adjust schooling decisions before the school year begins. By contrast, we find little evidence that adverse conditions during harvest affect school attendance, including when we extend the analysis to more recent harvest windows. This finding suggests that drought conditions at harvest – when planting outcomes are more fully realized – may be less influential for schooling decisions—decisions that are often made earlier in the agricultural cycle in response to expected rather than realized income.

Our second finding is that outcome sensitivity also matters. The effects of the same drought exposure can differ when school attendance is measured at the start rather than the midpoint of the school year. For example, droughts during pre-planting and immediately before the school year have significantly larger negative effects on school attendance at the start relative to the midpoint of the school year, whereas droughts during mid-planting have significantly larger negative effects on school attendance at the midpoint relative compared to the start of the school year. These findings imply that studies relying on a single measure of attendance may reach a different substantive conclusion depending on when schooling is observed. More broadly, they highlight

the need for greater attention to when climate exposure and schooling outcomes are measured when estimating the effects of climate shocks on schooling.

Our findings align with a growing body of work that measures climate shocks during agriculturally salient periods for school attendance (Agamile and Lawson 2021; Dhongde and Spyrou 2023; Marchetta et al. 2019). However, we extend this literature by explicitly examining how effects vary across many windows and therefore identify those exposure windows in which the effects of droughts on school attendance are largest. In doing so, our results partially echo Randell and Gray (2016), who show that rainfall anomalies during certain seasons in Ethiopia (in their case the main rainy season) have stronger consequences for school attendance than shocks occurring at other points, but our results also identify pre-planting windows in which drought has a particularly large negative effect on start-of-year attendance. At the same time, our findings highlight the importance of context, as agricultural calendars vary across settings. These results suggest that the effects of drought on school attendance depend on which exposure window is used and when school attendance is measured, and that broader annual measures may mask important seasonal heterogeneity in how drought affects schooling outcomes. More broadly, our findings connect to a long tradition in demography emphasizing the importance of timing for understanding population processes. Just as shifts in the timing of births can distort period fertility measures (Bongaarts and Feeney 1998; Kohler and Ortega 2002), and life-course research shows that the consequences of shocks depend on the developmental stage at which exposure occurs (Almond 2006; Almond and Currie 2011; Lumey, Stein, and Susser 2011; Randell and Gray 2016), our results demonstrate that the effects of drought shocks on school attendance can vary across exposure windows and with when attendance is measured.

Although our analysis focuses on drought exposure and school attendance, the dimensions of temporal sensitivity we identify may apply more broadly to other environmental shocks and socio-demographic outcomes. Drought is a slow-onset shock whose consequences unfold gradually through agricultural, income, and food security pathways, so its effects may depend on which exposure window is used and when the outcome is observed. These considerations may also extend to other climate shocks and to outcomes that exhibit strong intra-annual variation because they are shaped by seasonal fluctuations in food availability, disease exposure, and household economic stress, including mortality, fertility, and child wasting (Dorélien 2016; Wolfle and Channon 2023).

Our findings have implications for how researchers measure environmental exposures and socio-demographic outcomes. Analytical decisions about how to define exposure windows should be shaped by contextual information. For example, identifying sub-annual exposure periods that align with local planting, growing, or harvest cycles requires knowledge of local agricultural calendars. When possible, researchers should first assess temporal heterogeneity by testing whether effects vary across exposure periods (for example, across seasons or months). Doing so may reveal patterns similar to those documented here, where effects are concentrated in particular exposure windows. In addition, studies should report the timing of interviews and describe how outcomes vary over the course of the year in the study setting, because effects may differ depending on when outcomes are observed.

Finally, our findings have implications for policies aimed at protecting children's education in climate-vulnerable settings. In our analysis, droughts during pre-planting and planting were particularly consequential for children's school attendance. Policies designed to buffer households against droughts may therefore be most effective if they coincide with these periods. This is

particularly important from a demographic perspective as education is closely linked to later-life outcomes including fertility, health, and intergenerational mobility, and disruptions to children's schooling may therefore have longer-term implications not only for individual well-being but also for broader demographic and development trajectories.

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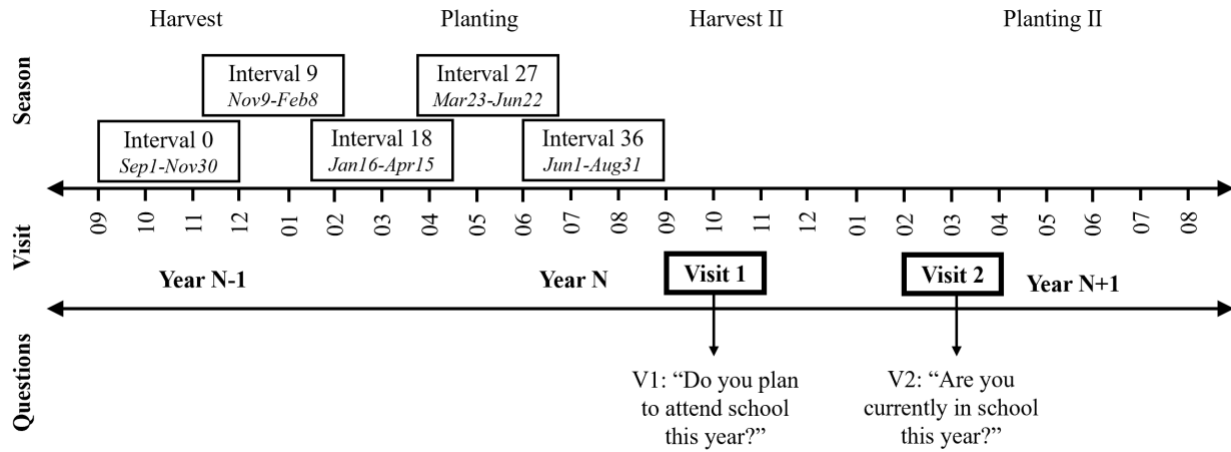
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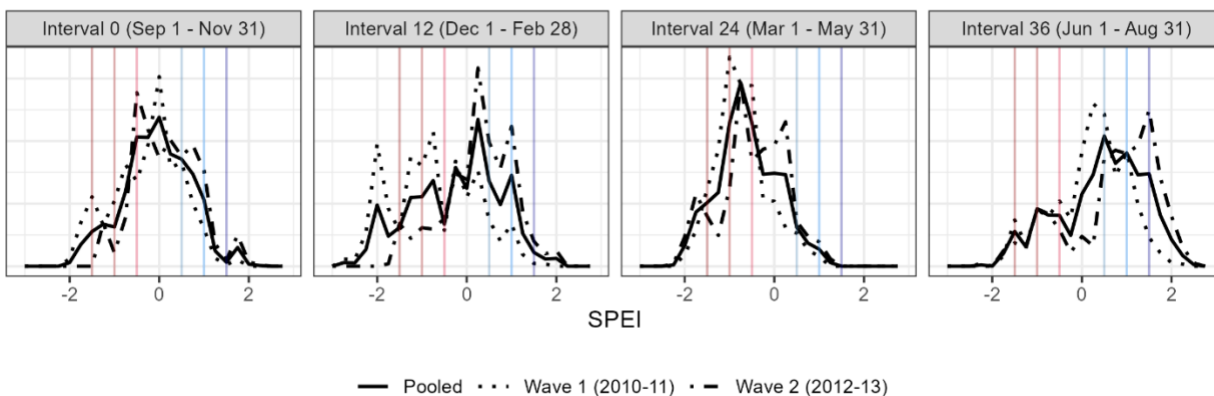
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Figure 1: Timeline of example seasonal SPEI3 intervals and schooling questions by visit



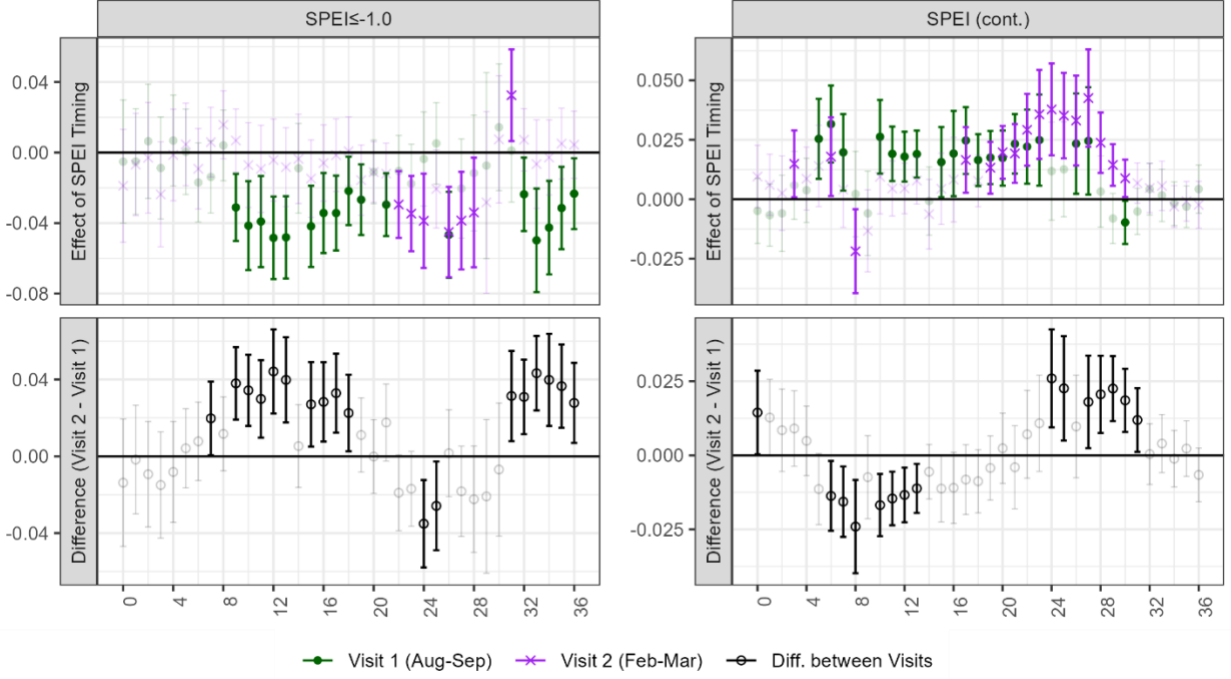
Notes: SPEI3 exposures are defined over sequential three-month windows shifted weekly (8th, 15th, 22nd, and last day of each month), to measure conditions from Interval 0 (Sept 1 to Nov 30) during early harvest to Interval 36 (Jun 1 to Aug 31) right before the academic year. The figure displays selected windows for illustration; for example, Interval 27 spans March 23 to June 22 and falls in the latter part of the planting season. Each LSMS wave include two visits: post-planting (Sept–Oct) and post-harvest (Feb–Mar). Visit 2 directly measures current school attendance at the midpoint of the school year. Visit 1 asks whether the child plans to attend school that year; however, because Visit 1 interviews in our sample occurred after the school year had begun, we can confidently use this response as a measure for start-of-year attendance. Table 2 gives the exact wording of the schooling questions.

Figure 2: Distribution of SPEI3 across selected seasonal intervals, by wave



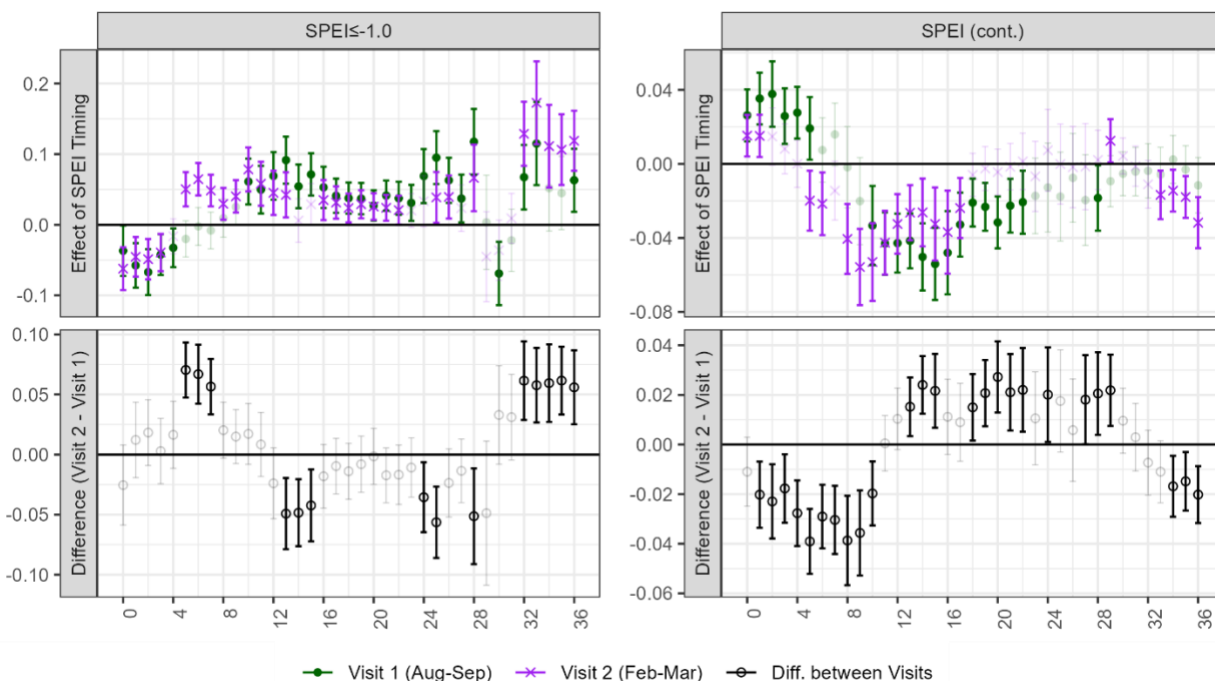
Notes: Red and blue lines mark the dry and wet thresholds used in the analysis. The figure shows SPEI3 distributions for selected intervals corresponding to early harvest (Interval 0), late harvest/early planting (Interval 12), planting (Interval 24), and the period immediately before the school year (Interval 36).

Figure 3: Estimates of the effect of sequential SPEI3 exposure windows (x-axis) on school attendance in Visit 1 (green) and Visit 2 (purple) for $\text{SPEI3} \leq -1.0$ (left) and continuous SPEI3 (right).



Notes: For each SPEI3 interval, we estimate one model with an interaction term with visit (Eq. 1). The x-axis shows sequential three-month windows shifted weekly, from Interval 0 (Sept 1 to Nov 30) to Interval 36 (Jun 1 to Aug 31). Harvest occurs mainly from September to December and overlaps with Intervals 0–12; planting occurs mainly from March to July and overlaps with Intervals 16–32. Standard errors are clustered by errors. Estimates statistically different from zero at $p < 0.05$ are highlighted.

Figure 4: Estimates of the effect of sequential SPEI3 exposure windows (x-axis) on child labor in Visit 1 (green) and Visit 2 (purple) for $\text{SPEI3} \leq -1.0$ (left) and continuous SPEI3 (right).



Notes: For each SPEI3 interval, we estimate one model with an interaction term with visit (Eq. 1). The x-axis shows sequential three-month windows shifted weekly, from Interval 0 (Sept 1 to Nov 30) to Interval 36 (Jun 1 to Aug 31). Harvest occurs mainly from September to December and overlaps with Intervals 0–12; planting occurs mainly from March to July and overlaps with Intervals 16–32. Standard errors are clustered by errors. The sample is restricted to individuals that responded to both attendance questions *and* both labor questions in each wave. Estimates statistically different from zero at $p < 0.05$ are highlighted.

Table 1: Summary of previous studies

Study	Country/Region	Type of climatic shock	Shock measurement	Timing of shock measurement	Education outcome(s)	Timing of education measurement
Adejuwon (2016)	Zamfara state, Nigeria	Rainfall and temperature anomalies	Deviations of annual rainfall and temperature from long-term mean	Annual rainfall and temperature; specific timing unspecified	School enrolment and attendance	Annual school records
Agamile and Lawson (2021)	Uganda	Rainfall shocks	Deviations of seasonal rainfall from long-term seasonal mean	Current first agricultural season (Jan–Jun)	School attendance	At the time of survey rounds: at various times throughout the year but within the same agricultural season
Arceo-Gómez et al. (2020)	Mexico	Drought	Monthly drought episodes	Drought exposure in any of the months between first and last interview	School attendance	At quarterly survey visits
Baez et al. (2020)	Mozambique	Drought and flood	One-month SPI standardized relative to long-term average and rainfall totals expressed as z-score deviations from long-term average	Jan–Feb 2005 drought and Dec 2006–Feb 2007 floods	School attendance	At time of survey waves: July 2002–June 2003, September 2008–August 2009, and August 2014–July 2015; attendance in 2 weeks prior to interview in each wave
Baloch and Behrman (2016)	Ethiopia	Drought and floods	Self-reported exposure to drought and floods	During the previous four years	School enrolment and test scores	At the time of survey waves
Bazan (2023)	Peru	Flooding	Cumulative rainfall anomalies relative to long-term mean	Flood exposure during the in-utero period	Completing primary or secondary education	At the time of survey rounds
Berhane et al. (2015)	Ethiopia	Drought	Self-reported exposure to drought	Drought exposure between first and second survey waves and between second and third survey waves	Test score	At time of the second and third survey waves

Björkman-Nyqvist (2013)	Uganda	Rainfall shocks	Deviation of log annual rainfall from the long-term mean	Rainfall in the current calendar year [with t-1 used as a robustness check] for enrolment and rainfall in the year before for test scores	School enrolment and test scores	Enrolment at the start of the school year; test scores at the end of the school year
Branco and Féres (2018)	Brazil	Drought	Share of months in a year with rainfall more than one standard deviation below the long-term mean	January through November (i.e. the exam month) of the exam year	Achievement scores	Exam month
Calero et al. (2009)	Ecuador	Drought (and other shocks)	Self-reported exposure to shocks including drought	During the previous year	School enrolment	Week preceding survey interview
de Janvry et al. (2006)	Mexico	Drought	Self-reported exposure to shocks including drought	Last 6 months prior to each interview	School attendance	Every six months between November 1997 and November 2000
Dhongde and Spyrou (2023)	Ethiopia	Drought	Seasonal Standardized Precipitation Evapotranspiration Index	During grain growing seasons (Belg and Kiremt) of survey year	School attendance, grade completion, relative grade	At time of survey rounds
Falaris (2023)	Ethiopia	Rainfall	Total annual rainfall relative to long-term average annual rainfall	Year before survey	School attendance, extended absence from school	At time of survey (February 2016) for school attendance; prior semester (pre-February 2016) for extended absence
Garg et al. (2020)	India	High temperature	Number of days above specific temperature thresholds	Calendar year prior to the year of the test and between successive tests	Test scores	At the time of survey rounds
Grosso and Kraehnert (2017)	Mongolia	Drought based on normalized differenced vegetation index (NDVI)	Deviations of summer NDVI from long-term average	Exposure during schooling years	Completed at least mandatory school	At the time of survey rounds

Joshi (2019)	Maharashtra, India	Drought (with rainfall anomalies + Standardized Precipitation Index)	Deviations of rainfall for 2012 from long-term mean and SPI	Year specific	Maths and reading scores	At the time of survey rounds
Maddaloni et al. (2022)	Brazil	Drought	Deviations of annual rainfall from long-term mean	Annual measures of rainfall in the year of survey	Dropout rate, passing rate, and over-age rate	At the time of census years
Marchetta et al. (2019)	Madagascar	Rainfall deviations	Rainfall deviations during rainy season from long-term average	Rainy season rainfall (November t–1 to April t) matched to school year beginning September t–1	School attendance and test scores	School year starting September t–1
Nguyen and Pham (2018)	Ethiopia, India, Peru, and Vietnam	Drought and floods	Self-reported exposure to drought and floods	During the previous four years	School enrolment, number of completed grades, and test scores	At the time of survey waves
Nordstrom and Cotton (2020)	Zimbabwe	Drought	Rainfall in 2015–16 growing season relative to long-term mean	Growing-season rainfall prior to endline survey	Test score and attendance	Baseline (Oct 2013–Feb 2014) and endline (Nov–Dec 2016)
Nübler et al. (2021)	Northeast Kenya	Rainfall shocks	Annual rainfall expressed as z-score deviations from long-term averages	Annually from first year of life to 7	Ever enrolled in any level of primary schooling, ever enrolled by age eight, number of completed grades, and test scores	During adolescence and at the time of survey waves
Park et al. (2020)	United States	Heat	Mean annual temperature and number of days above a specific temperature threshold	One, two, three, and four years prior to the exam (split into school days vs weekends/summer)	Test scores	At the time of assessment

Park et al. (2021)	58 countries + United States	Heat exposure	Number of days above a specific temperature threshold	Year of the test and each of five lagged years (split into school days vs weekends/summer) and for the US June–February before spring exam (school days vs non-school days separated)	Test scores	At the time of assessment
Prenhaca and Melo (2024)	Northeast Brazil	Drought based on the vegetation health index (VHI)	Months in drought per year	Annual VHI	Student performance and progression	At the time of census years
Randell and Gray (2016)	Ethiopia	Temperature and rainfall variability	Seasonal temperature and rainfall expressed as z-score deviations from long-term averages	Early life (from birth year through age 6), during school ages (age seven to current age), and in utero (the year before birth); all across three seasonal cycles (Belg, Kiremt, Bega)	Completion of at least one grade and school attendance	Between March and September every year
Randell and Gray (2019)	Global tropics	Temperature and rainfall anomalies	Mean annual temperature and total annual rainfall expressed as z-score deviations from long-term averages	Cumulative early-life window (from year before birth through age 5)	Highest grade/level of formal schooling completed	At time of survey interview
Shah and Steinberg (2017)	India	Rainfall shocks	Deviations of annual rainfall from long-term mean	Annual (year t and t–1) and cumulative early-life window (from in utero to age 4)	School attendance and test scores	At the time of survey rounds
Woldehanna and Hagos (2015)	Ethiopia	Drought	Self-reported exposure to shocks including drought	Unspecified	School dropout	At the time of survey waves

Zimmermann (2020)	India	Rainfall	Positive/negative shocks defined as rainfall below 20th percentile or above 80th percentile of average	Rainfall in the year of survey (1986, 1995, and 2007)	School enrolment and school attainment	At time of survey rounds: July 1986–June 1987, 1995–1996, and 2007– 2008
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Table 2: School attendance questions by wave and visit

Wave	Visit	School attendance questions
Wave 1 (2010–11)	V1: Sept–Oct 2010	Do you plan to attend school in the next (2010–11) school year?
	V2: Feb–Mar 2011	Are you presently in school this (2010–11) school year?
Wave 2 (2012–13)	V1: Sept–Oct 2012	Do you plan to attend school in the next (2012–13) school year?
	V2: Feb–Mar 2013	Are you presently in school this (2012–13) school year?

Notes: Visit 2 directly measures current school attendance at the midpoint of the school year. Visit 1 asks whether the child plans to attend school that year; however, because Visit 1 interviews in our sample occurred after the school year had begun, we can confidently use this response as a measure for start-of-year attendance.

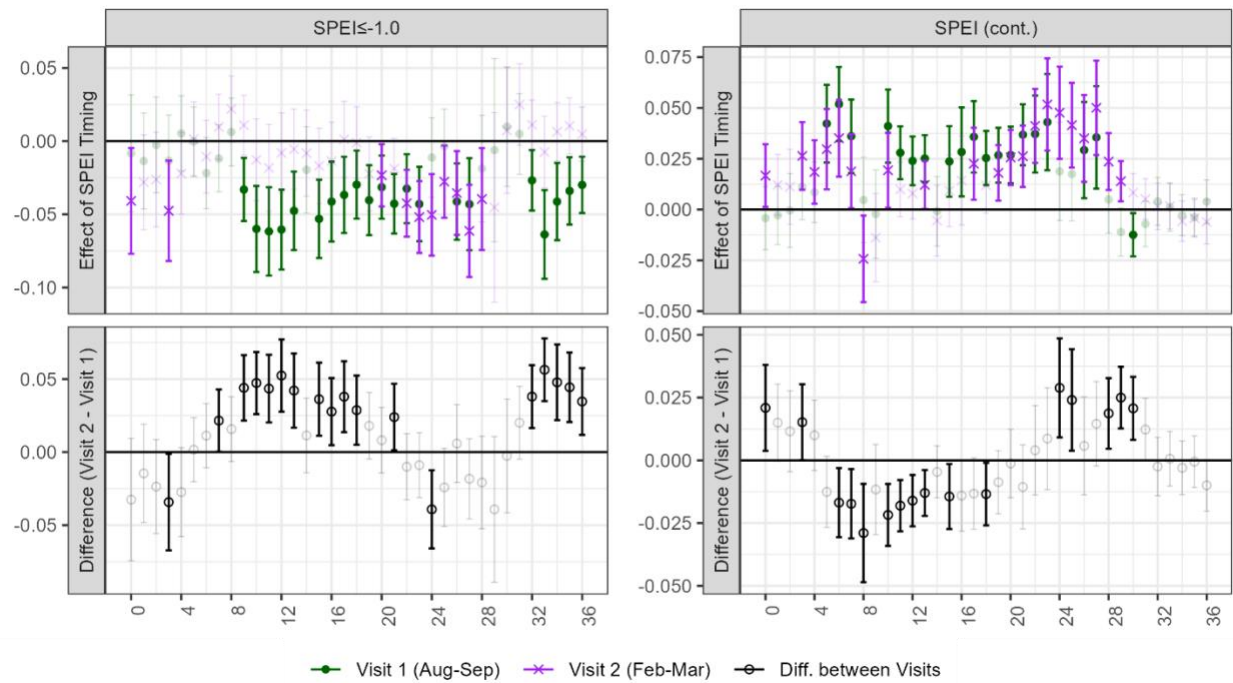
Table 3: Descriptive Statistics

	Wave 1 Visit 1		Wave 1 Visit 2		Wave 2 Visit 1		Wave 2 Visit 2	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Visit 1: Do you plan to attend school this year?	0.87	0.34			0.85	0.36		
Visit 2: Are you currently attending school?			0.84	0.36			0.84	0.37
Labor: Did you work during the past 7 days?	0.19	0.39	0.11	0.32	0.14	0.35	0.09	0.28
Sex (M=1; F=2)	1.45	0.50	1.46	0.50	1.45	0.50	1.45	0.50
Age (range:6-15)	9.27	2.44	9.27	2.44	10.85	2.51	11.16	2.47
SPEI3: Interval 0 (till Nov30)	-0.31	0.77	-0.31	0.77	0.10	0.71	0.10	0.71
SPEI3: Interval 12 (till Feb28)	-0.69	0.90	-0.69	0.90	0.32	0.75	0.32	0.75
SPEI3: Interval 24 (till May31)	-0.70	0.58	-0.70	0.58	-0.45	0.69	-0.45	0.69
SPEI3: Interval 36 (till Aug31)	0.22	0.77	0.22	0.77	0.67	0.98	0.67	0.98
<i>N</i>	8,380				8,380			

Notes: *N* refers to child-visit observations. The analytic sample contains 4,190 children observed in both visits of both waves, yielding 8,380 observations for Visit 1 and 8,380 for Visit 2 across the two waves. These children answered the attendance questions in all four survey rounds. Of these, 4,001 children also answered the labor question in all four survey rounds yielding 16,004 child-visit observations for the labor analysis. SPEI3 statistics are shown for selected intervals corresponding approximately to early harvest (Interval 0), late harvest/pre-planting (Interval 12), planting (Interval 24), and the period immediately before the school year (Interval 36).

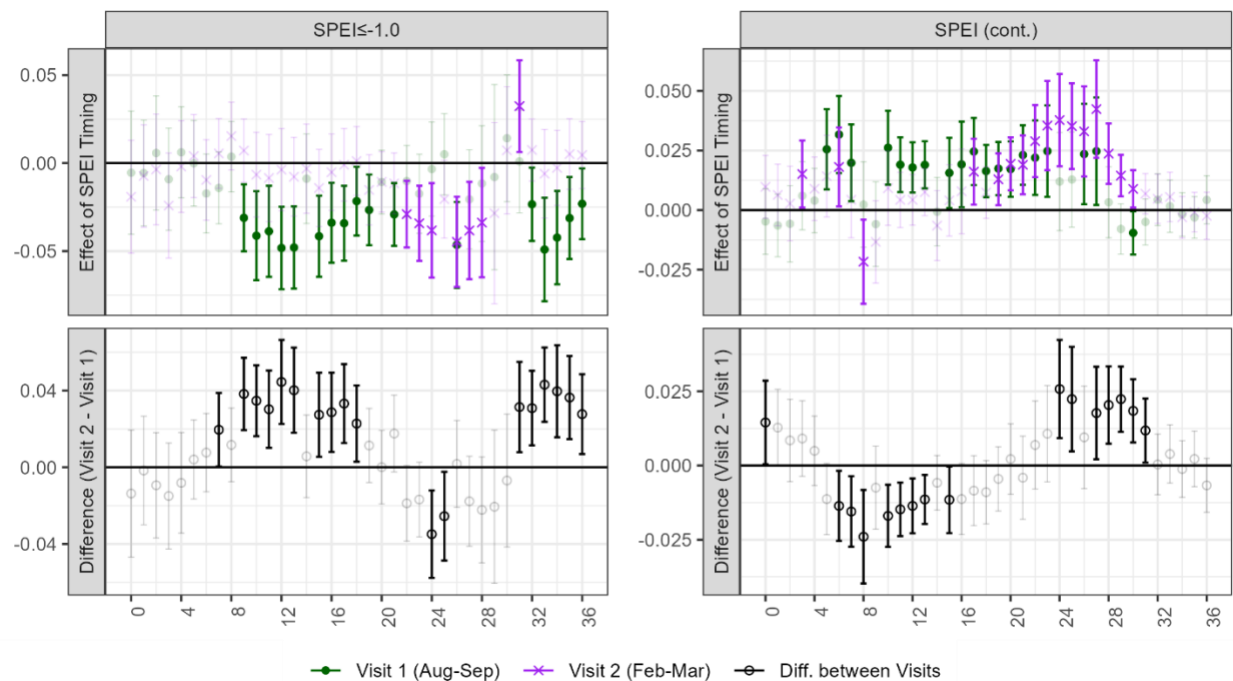
SUPPLEMENTARY MATERIALS

Figure A1: Estimates of the effect of sequential SPEI3 exposure windows (x-axis) on school attendance in Visit 1 (green) and Visit 2 (purple) for $\text{SPEI3} \leq -1.0$ (left) and continuous SPEI3 (right), individual fixed-effects on *age-consistent* sample



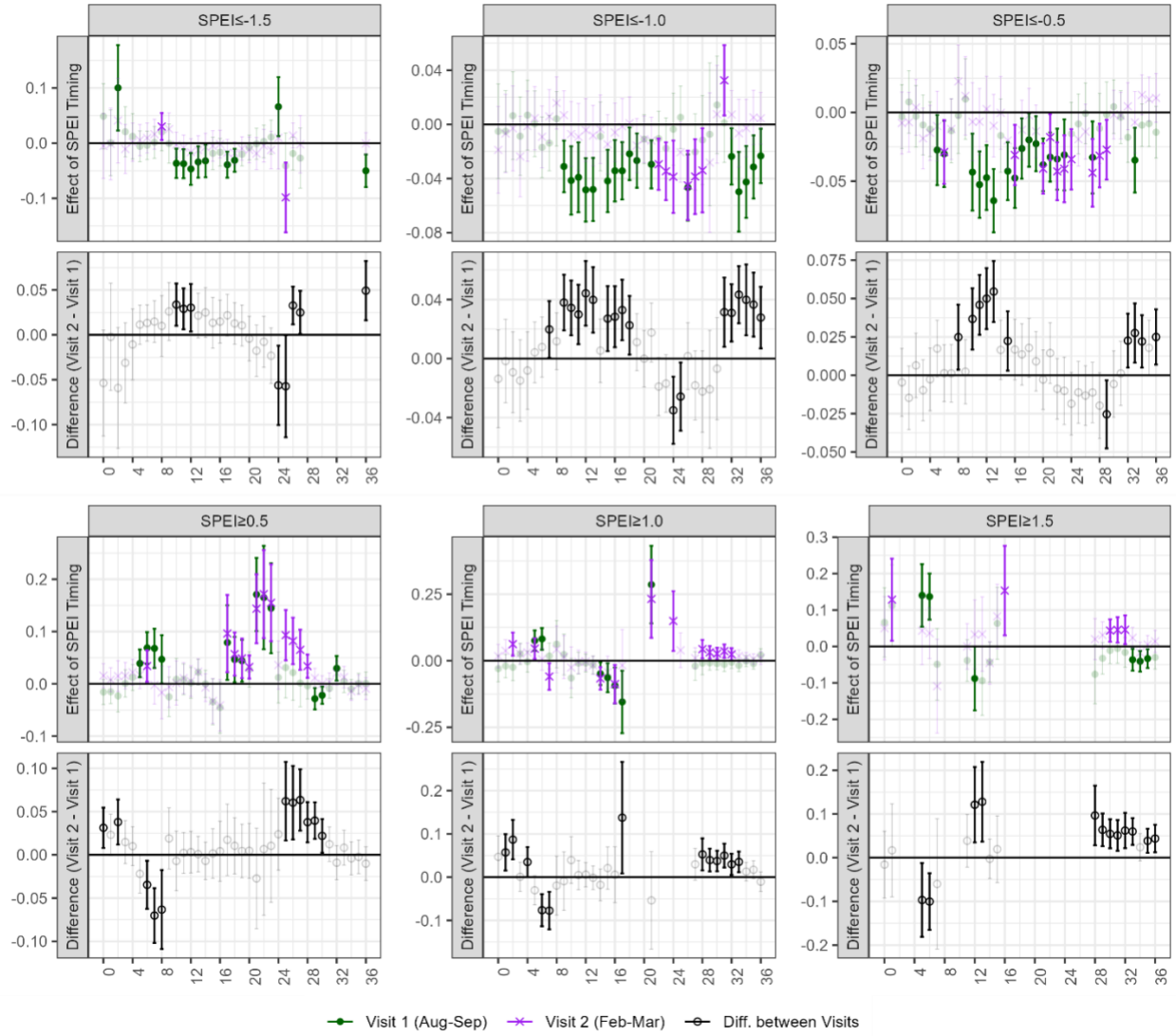
Notes: This figure replicates Figure 3 using an age-consistent sample. We restrict the sample to respondents whose reported age differs by less than four years between Waves 1 and 2 and by less than one year between Visits 1 and 2 within each wave. Each model has the same number of observations ($N = 11,616$).

Figure A2: Estimates of the effect of sequential SPEI3 exposure windows (x-axis) on school attendance in Visit 1 (green) and Visit 2 (purple) for $\text{SPEI3} \leq -1.0$ (left) and continuous SPEI3 (right), household fixed-effects instead of individual fixed-effects



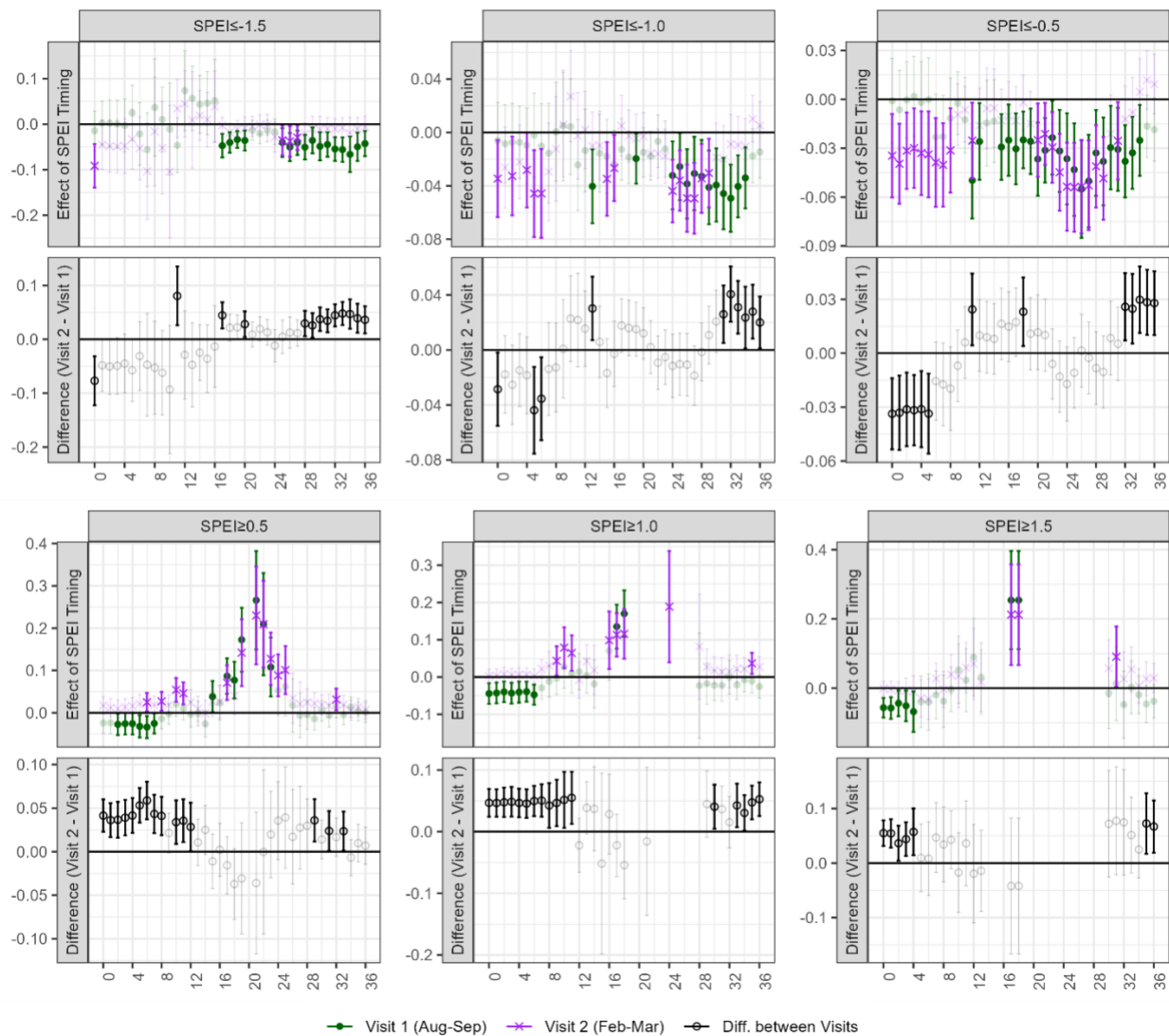
Notes: This figure replicates Figure 3 using household-fixed effects instead of individual fixed-effects. Each model has the same number of observations ($N = 16,760$).

Figure A3: Estimates of the effect of sequential SPEI3 exposure windows (x-axis) on school attendance in Visit 1 (green) and Visit 2 (purple) across six SPEI3 thresholds



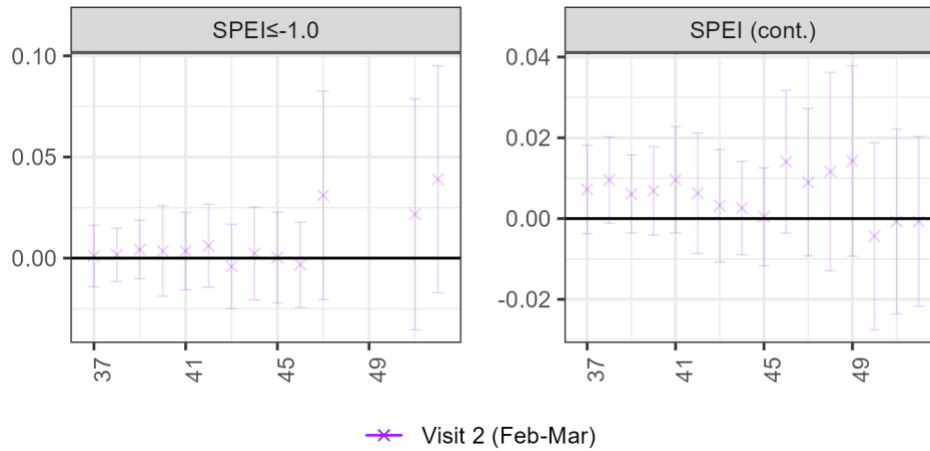
Notes: This figure replicates Figure 3 using alternative SPEI3 thresholds for dry (≤ -1.5 , ≤ -1.0 , and ≤ -0.5 ; top row) and wet ($\geq +0.5$, $\geq +1.0$, $\geq +1.5$; bottom row) conditions. Models are not shown when fewer than 0.5% of observations meet the binary threshold. Each model has the same number of observations ($N = 16,760$).

Figure A4: Estimates of the effect of sequential SPEI6 exposure windows (x-axis) on school attendance in Visit 1 (green) and Visit 2 (purple) across six SPEI6 thresholds



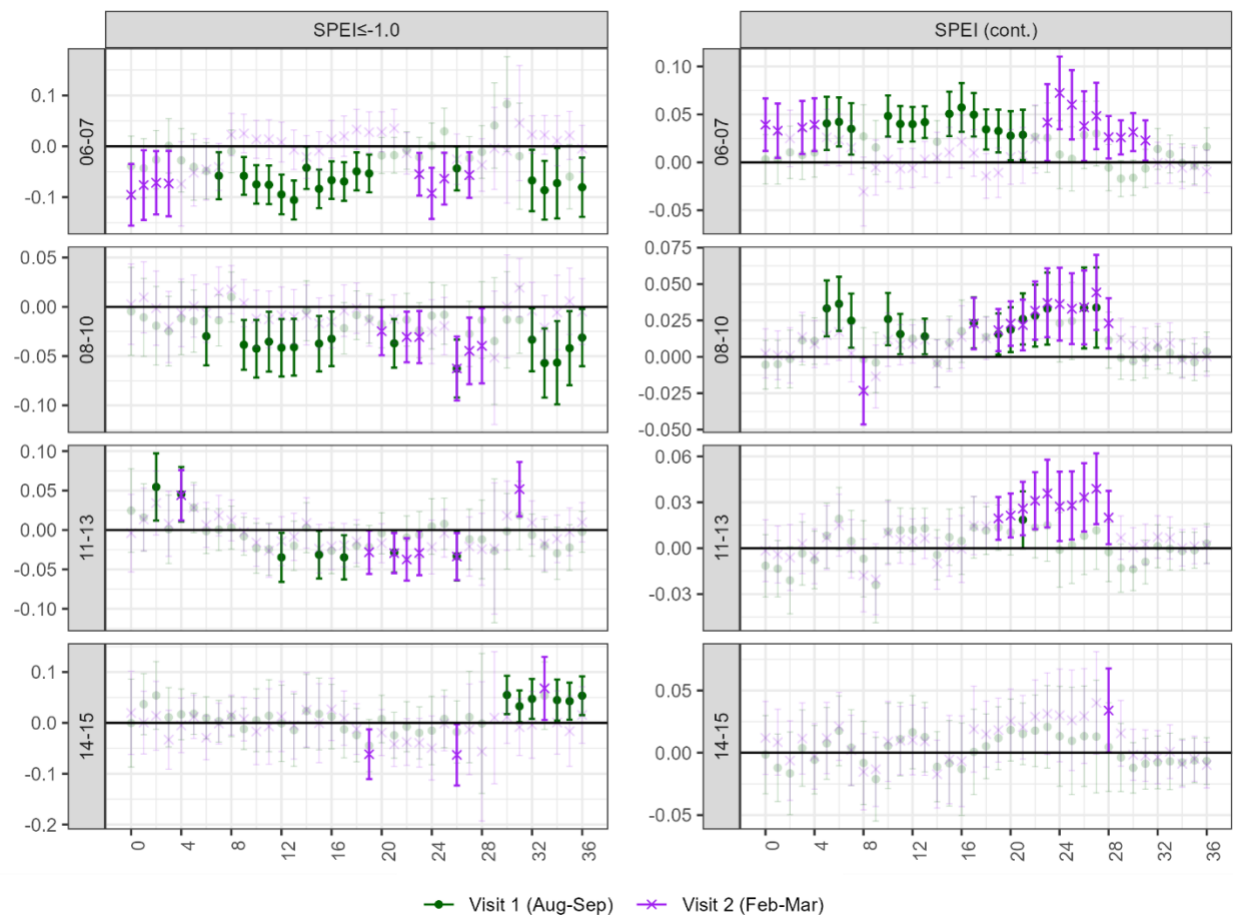
Notes: This figure replicates Figure A3 using six-month SPEI rather than three-month SPEI. Each SPEI6 interval is aligned with the same end date as the corresponding SPEI3 interval so that Interval 0 for SPEI6 is Jun 1–Nov 30, and Interval 36 is Mar 1–Aug 31. Each model has the same number of observations ($N = 16,760$).

Figure A5: Estimates of the effect of sequential SPEI3 exposure windows preceding Visit 2 (x-axis) on school attendance at Visit 2 for $\text{SPEI3} \leq -1.0$ (left) and continuous SPEI3 (right)



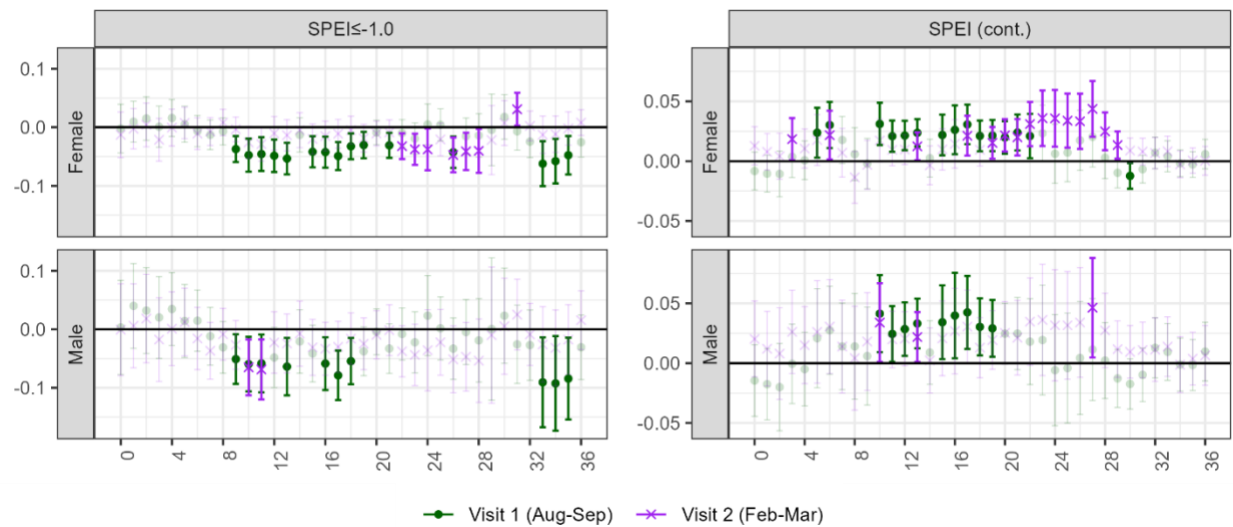
Notes: Figure 3 uses SPEI3 intervals from Interval 0 (Sep 1–Nov 30) to Interval 36 (Jun 1–Aug 31) so that all exposure windows precede both Visit 1 (Sep–Oct) and Visit 2 (Feb–Mar). This figure extends the exposure windows to Interval 37 (Jun 9–Sept 8) through Interval 52 (Oct 1–Dec 31) to assess the effect of more recent exposure windows, including the harvest directly preceding Visit 2, on school attendance at Visit 2. Each model has the same number of observations ($N = 8,380$).

Figure A6: Estimates of the effect of sequential SPEI3 exposure windows (x-axis) on school attendance in Visit 1 (green) and Visit 2 (purple) for $\text{SPEI3} \leq -1.0$ (left) and continuous SPEI3 (right), by age groups



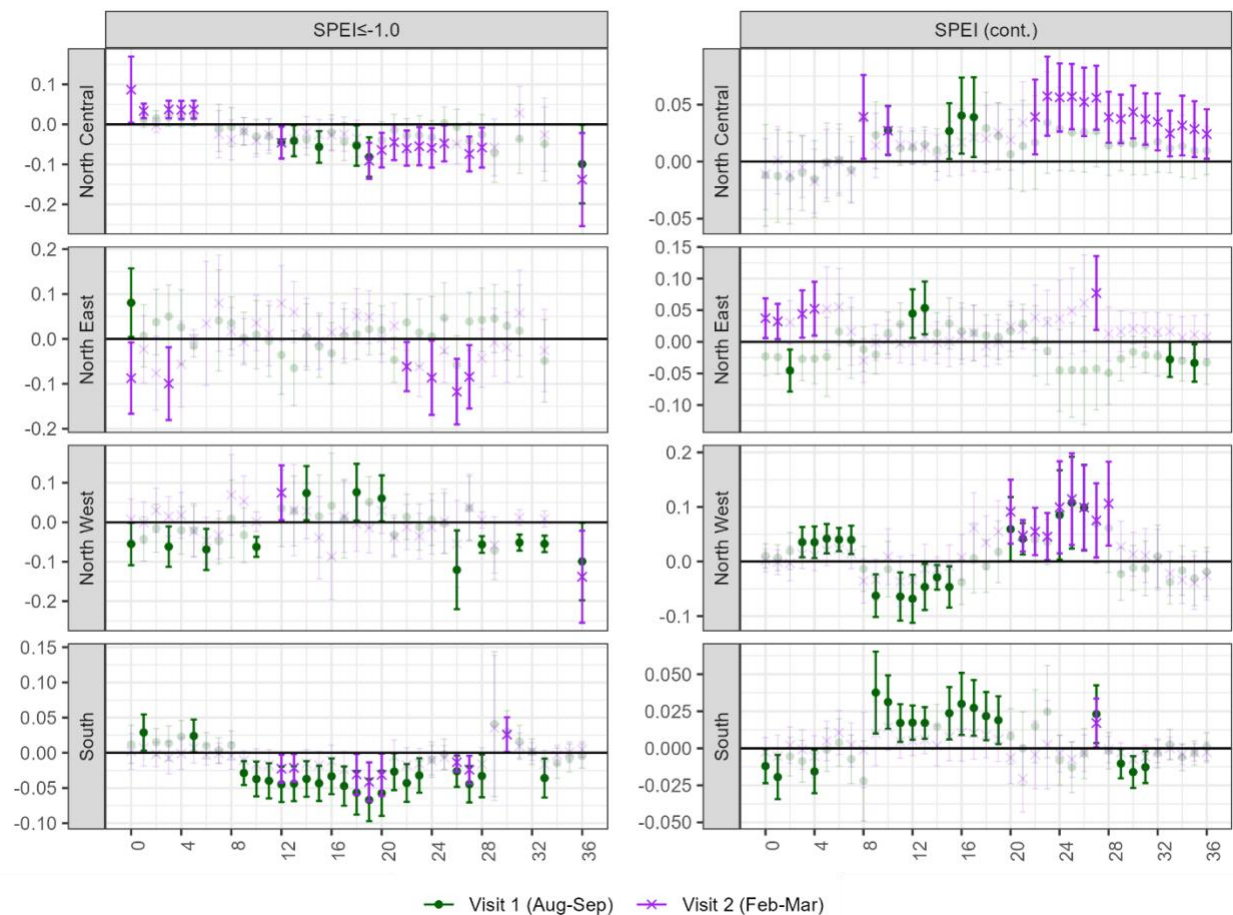
Notes: This figure estimates the model in Eq. (1) and includes an additional interaction term between SPEI3, visit, and age group. Each model has the same number of observations ($N = 16,760$) split across ages 6–7 ($N = 3,149$), ages 8–10 ($N = 6,676$), ages 11–13 ($N = 4,815$), and ages 14–15 ($N = 2,120$).

Figure A7: Estimates of the effect of sequential SPEI3 exposure windows (x-axis) on school attendance in Visit 1 (green) and Visit 2 (purple) for $\text{SPEI3} \leq -1.0$ (left) and continuous SPEI3 (right), by sex



Notes: This figure estimates the model in Eq. (1) and includes an additional interaction term between SPEI3, visit, and sex. Each model has the same number of observations ($N = 16,760$) split across male ($N = 8,921$) and female ($N = 7,839$).

Figure A8: Estimates of the effect of sequential SPEI3 exposure windows (x-axis) on school attendance in Visit 1 (green) and Visit 2 (purple) for $\text{SPEI3} \leq -1.0$ (left) and continuous SPEI3 (right), by region.



Notes: This figure estimates the model in Eq. (1) and includes an additional interaction term between SPEI3, visit, and regions in Nigeria. Each model has the same number of observations ($N = 16,760$) split across North Central ($N = 3,132$), North East ($N = 3,400$), North West ($N = 4,444$), and South ($N = 5,784$).