Rainfall shocks, cognitive development and educational attainment among adolescents in a drought-prone region in Kenya

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Table A1. Rainfall z-scores and shocks in Wajir County, by location and year

	AGI-K clusters		*HSNP census locations*	
Year	Rainfall z-score	Rainfall shock (% locations z-score < -1)	Rainfall z-score	Rainfall shock (% locations z-score < -1)
2000	-1.23	97.5	-1.23	91.6
	(0.12)		(0.20)	
2001	-0.65	0.0	-0.62	1.1
	(0.12)		(0.13)	
2002	0.46	0.0	0.41	0.0
	(0.17)		(0.20)	
2003	0.40	0.0	0.55	0.0
	(0.33)		(0.28)	
2004	-0.15	0.0	-0.02	0.0
	(0.21)		(0.24)	
2005	-1.04	57.5	-1.01	50.0
	(0.15)		(0.14)	
2006	0.88	0.0	1.07	0.0
	(0.45)		(0.49)	
2007	-0.42	1.3	0.37	0.0
	(0.28)		(0.29)	
2008	-0.49	0.0	-0.44	0.0
	(0.15)		(0.18)	
2009	-0.44	3.8	-0.47	2.6
	(0.24)		(0.24)	
2010	-0.53	1.5	-0.50	1.5
	(0.13)		(0.18)	
2011	0.07	0.0	0.22	0.0
	(0.33)		(0.53)	
2012	-0.62	2.5	-0.50	0.7
	(0.23)		(0.30)	
2013	0.22	0.0	0.44	0.0
	(0.33)		(0.38)	
2014	-0.24	0.0	-0.25	0.0
	(0.18)		(0.20)	
2015	-0.07	0.0	-0.03	0.0
	(0.14)		(0.16)	
N	80	80	274	274

Notes: Location-specific z-score calculated using location historical mean and standard deviation calculated from 1980-1999. Standard deviations shown in parentheses. Rainfall shock (%) is percentage of locations where z-score < -1.0 in that year.

Table A2. Summary statistics for 2013 HSNP census (girls and boys 9–14)

	Age						
	9	10	11	12	13	14	All
Girls							
Ever enrolled (=1)	0.467	0.475	0.502	0.487	0.523	0.514	0.491
Currently enrolled (=1)	0.266	0.272	0.283	0.279	0.286	0.278	0.276
Completed grades	1.825	2.090	2.493	2.755	3.350	3.706	2.616
	(2.203)	(2.448)	(2.759)	(3.112)	(3.530)	(3.928)	(3.049)
N	6,027	11,884	4,305	8,606	5,632	5,703	42,157
Boys							
Ever enrolled (=1)	0.528	0.564	0.602	0.602	0.631	0.632	0.590
Currently enrolled (=1)	0.314	0.343	0.368	0.364	0.378	0.374	0.356
Grades completed	2.040	2.510	3.001	3.386	4.041	4.528	3.178
	(2.209)	(2.515)	(2.805)	(3.121)	(3.506)	(3.882)	(3.120)
N	6,574	14,461	5,362	10,651	6,773	7,440	51,261

Note: Standard deviations shown in parentheses for non-binary variables.

Additional notes on context, data and further robustness considerations

AGI-K is a study examining the effects of four sequentially layered interventions including violence prevention, education, health and wealth creation, on education and reproductive health outcomes of young adolescent girls. Implemented as a randomized trial by the Population Council and the African Population and Health Research Center, AGI-K targeted girls ages 11–14 in two marginalized areas of Kenya, rural Wajir County and the urban informal settlement of Kibera, Nairobi. The Wajir baseline survey examined in this paper was collected prior to the start of the program interventions and therefore not influenced by them (Austrian *et al.*, 2016). This paper uses the 2015 baseline survey data as available in 2019.

Primary school in Kenya is from grade 1 through 8, after which students enroll in secondary school, normally four years (Forms 1 through 4). We calculated completed grades as the total number of primary and secondary grades completed. For the literacy measurement, the Swahili sentences were: 1) *Ukulima ni kazi ngumu*; and 2) *Mtoto anasoma kitabu*. The English sentences were: 1) Parents love their children; and 2) Farming is hard work. Girls enrolled in boarding school at the time of the baseline survey (< 3 percent of all girls) were excluded from the sample as they could not fully participate in the AGI-K intervention.

As discussed in the text, measurement error in ages or location is a potential concern in the analyses. For the HSNP data, truthful responses were required for program eligibility, increasing confidence in the accuracy of the information provided in the census survey. For the HSNP census, however, there is no historical migration information and we make the assumption that the children lived in the same locations throughout their lives. If those who have migrated to their current locations on average migrated to places with lower probability of rainfall shocks (consistent with evidence from other contexts that low rainfall may induce out-migration), the misclassification error introduced by such mismeasurement would likely lead to positive bias on the (negative) rainfall shock estimates.

The HSNP census sample covers a larger area in Wajir, including Wajir north. This suggests an additional reason one might expect the effects of shocks to be smaller in magnitude when estimated using the HSNP census sample: overall lower schooling levels and availability. Consistent with this, results excluding Wajir North where education levels are lower generally show even larger effects of rainfall shocks than the ones presented in table 4 in the main text.

A parallel analysis examining the effect of 2005 rainfall shocks on livestock ownership measured in the 2013 HSNP census yielded qualitatively similar findings to those reported in the text for the AGI-K sample. Both sets of findings for historical rainfall shocks also indirectly confirm the likely contemporaneous importance of rainfall shocks for household resources in the specific Wajir context, which because there were no contemporaneous shocks we cannot explore directly.

Both individual-level samples include only the surviving adolescents. The under-5 infant mortality rate (per 1,000 births) in Wajir is estimated to have declined from 80.2 in 1995 to 46.6 in 2013, however, and is currently modestly lower than the national average of 54.5 (Macharia *et al.*, 2019). The relatively low rates suggest that bias due to mortality selection is not likely to be substantial.

Finally, we note that there is seasonal variation in rainfall throughout the year in Wajir, with the so-called long rains from March–May, and short rains from October–December. Aggregation of average rainfall during only those two periods yielded a measure nearly perfectly correlated with the overall annual measure used. Therefore, results are not sensitive to modifications in the specification focused on specific rainy seasons.

Note on potential interaction of rainfall shocks and the HSNP programme

1. Introduction

As is true for many low resource settings, the program and policy landscape in Wajir is complex. In particular, it includes the relatively large Hunger Safety Net Programme (HSNP) which provides one of the data sources we analyze, but may also have influenced the studied outcomes.[1] Because it was not possible to use the same identification strategy from the main article to estimate average program effects for this important – but non-randomly allocated – program, we outline in this appendix some relevant findings briefly summarized in the section 6.3.[2] The findings provide a partial robustness check on the main results in the paper by accounting for the HSNP program, suggestive evidence regarding the influence of the program, and a potential framework for similar future analyses of this and other related programs.

2. Background

During its pilot phase, which operated in non-randomly selected clusters throughout the four counties from 2009–2012, HSNP made payments every other month to beneficiary households. When the pilot began, the bimonthly payment was 2,150 Kenyan shillings (KSh) (~US$21.50 and approximately 12 per cent of average household consumption), but by 2012 it had increased to KSh3,500 (~US$35). Approximately 60,000 households benefited and the take-up rate for eligible households was above 90 per cent (Merttens *et al*., 2013). The program expanded in a second phase in 2013, reaching nearly 100,000 households overall, including approximately 20,000 in Wajir County, about one-eighth of the population (Merttens *et al*., 2018).

[1] Less than 1 per cent of households benefited from a different cash transfer program prominent elsewhere in Kenya, the Orphans and Vulnerable Children Programme. We therefore do not consider its role.
[2] Precisely because HSNP is non-randomly allocated, we do not in the primary analyses control for its presence which may have been due to prior rainfall or other shocks in the different locations.

A randomized evaluation of the pilot phase of HSNP (with 12 clusters in Wajir) demonstrated positive effects related to several of its primary objectives, including increased food consumption and total expenditure and corresponding reductions in poverty, as well as some evidence of higher livestock retention. There was no significant impact on school enrollment or school expenditures, but a modest effect on grades attained for those already enrolled in school prior to the program (Merttens *et al*., 2013). A more recent non-experimental evaluation of the second phase of HSNP revealed broadly similar results such as increased food expenditure and livestock ownership but did not reveal any overall average impacts on schooling (Merttens *et al*., 2018).

Recent research has demonstrated that safety net programs have the potential to counteract the effects of contemporaneous shocks (Hou, 2010; Asfaw *et al*. 2017; Dietrich and Schmerzeck, 2019). Analysis of the randomized HSNP pilot itself has explored the effect of the program on household-level food diversity and adult equivalent nutrient availability, with the spotlight on heterogeneity of effects across areas with different degrees of market access and reductions in vegetation coverage related to poor rainfall (Dietrich and Schmerzeck, 2019). Measures of vegetation coverage represent alternative proxy measures to the ones used in our paper for weather shocks.[3] Although the analysis examines different outcomes and covers a different time period for the 12 clusters in Wajir and 36 clusters in three neighboring counties, it is complementary to our study in assessing heterogeneous program impacts related to contemporaneous local weather conditions. Dietrich and Schmerzeck (2019) find modest average HSNP program impacts on food diversity after about one year and evidence that impacts were larger for less isolated communities that had experienced greater reductions in vegetation, likely

[3] We did not consider for our analyses the vegetation coverage proxy measure they use because it is not available with the long history we are able to construct for rainfall measures.

operating through agricultural prices. They conclude that unconditional cash transfers may be constrained in how effectively they can mitigate shocks to food availability in settings with weak markets.

There is also a related strand of literature for conditional cash transfer programs (CCTs). In addition to mitigating the effects of contemporaneous shocks, transfer programs may have beneficial effects for individuals who experienced negative shocks earlier in life prior to program availability, as seen for the programs in Mexico and Colombia (Adhvaryu *et al*., 2018; Duque *et al*., 2019). This evidence underscores the possibility that in addition to household compensatory behaviors, social programs can help lead to catch-up. The research also indicates impacts may be context specific.

3. Potential mechanisms

Unconditional cash transfers augment household resources when they are received, without necessarily changing relative prices or opportunity costs as conditional cash transfers do (Skoufias and Parker, 2001). Consequently, transfers can positively influence child outcomes. For example, relaxing household resource constraints when children are at school-entry ages can increase the likelihood of enrolling or remaining enrolled in school.

Cash transfers, therefore, can offset some of the negative consequences of rainfall shocks. This can happen contemporaneously, i.e., when households receive transfers at the same time or shortly after they experience the shocks. As suggested by Adhvaryu *et al*. (2018) and Duque *et al*. (2019) for CCTs, it can also happen because of the possible persistence of earlier shocks on later outcomes (in particular, through the household-resource channel), potentially offsetting the negative effects of previous shocks. For child educational outcomes, this might manifest as catch-up (relative to where the child with an earlier shock would be in the absence of the transfer). For example, additional resources available after the shock can enable households to make compensatory investments in those children who suffered early life shocks. That possibility underscores how the net effects of shocks on education after several years can incorporate both short- and long-term coping strategies and reflect all responses over the intervening period (Frankenberg and Thomas, 2018).

4. Methodology

We used HSNP program administrative data to determine whether at least one household in the location was covered by the non-randomly allocated program in each year to determine if the program was available there and define a location-level indicator of program availability.[4] With the data we have it was not possible to examine whether exposure to HSNP during early life affects later, school-age educational outcomes since the program only began in 2009 and the outcomes are measured in 2013 and 2015. Another potentially important period considered in our paper, however, is when children are about to start primary school, since late school starts have been linked to lower educational outcomes. We define $H_{ij,6-9}$ as a binary indicator for whether HSNP was available in child i's resident location j at any point between the ages of 6 and 9, combined to cover the common school entry ages in Wajir and to increase power. $H_{ij,6-9}$ represents potential (and not actual) household level participation or receipt of HSNP during those ages. Although this is an intent-to-treat measure and the models we estimate include location- or household-level fixed effects, because of non-random program placement we refrain from interpreting the coefficient on this indicator as a causal estimate of the average program

[4] The HSNP began operations in 2009 and rollout progressed steadily. By 2013, about one-half of the HSNP locations were at least partially covered.

effect of HSNP program exposure.

Given the timing of rainfall shocks (mainly in 2000 and 2005, as seen in appendix table A1) and the 2009 start of HSNP, in this setting it is also not possible to explore directly the contemporaneous role the program might play in mitigating the effects of shocks as there is minimal contemporaneous overlap of program availability and the rainfall shocks. It is possible, however, to examine whether the program might offset possible persistent detrimental effects of rainfall shocks experienced earlier in life, for example enabling some catch-up relative to those not experiencing such shocks. Moreover, power to detect such offsetting effects is arguably strengthened exactly because there are no contemporaneous shocks. We introduce a second additional term, the interaction of exposure to HSNP during ages 6–9 ($H_{ij,6-9}$) with whether the child experienced a rainfall shock in early life during ages 0–2, indicated by $S_{ij,0-2}$ in equation (A1).[5] The interaction, or difference-in-difference estimate, allows determination of whether transfers made later in childhood help mitigate the negative impact of a rainfall shock experienced in early life (ages 0–2).

$$y_{ij} = \beta_0 + \sum_{a=a_0}^{T} \delta_a S_{ij,a} + \boldsymbol{X_{ij}\beta_1} + \beta_2 H_{ij,6-9} + \beta_3 H_{ij,6-9} S_{ij,0-2} + u_{ij} \qquad \text{(A1)}$$

The δ_a capture the impact of rainfall shocks on y_{ij} at each age a. In the analyses, estimation of the parameter β_2 for the HSNP availability indicator alone is subject to non-random program placement bias and therefore we do not argue that it reflects the estimate of the average program effect. The interaction with exogenous rainfall, however, plausibly identifies β_3 capturing whether the program had a differential intent-to-treat impact for those experiencing rainfall shocks in early life.

[5] $S_{ij,0-2}$ is a binary indicator for whether $S_{ij,a} = 1$ for a $= 0, 1$ or 2. Combining over different ages increases power as well as possibly mitigating potential biases from age misclassification.

5. Results

Results for *ever enrolled* and *completed grades* are shown in table A3 where for increased power we use the HSNP sample and girls and boys are pooled. For models including location-level fixed effects (columns 1 and 3,) there is continued evidence of the importance of earlier shocks, though in particular for *ever enrolled*, some of the shocks are no longer significant when compared with table 4 in the main text.[6] The interaction term for early life shocks and HSNP exposure is positive and significant, though only marginally in the case of *ever enrolled*. When we control most stringently for household-level fixed effects, so that estimates are based on within-household comparisons of children exposed to rainfall shocks or to HSNP at different ages, the interaction effect remains positive and significant for *completed grades*, though modest in size, 0.13 grades. The point estimates suggest the availability of HSNP in the village led to an increase of approximately 0.1–0.2 grades for children who had experienced early life rainfall shocks between ages 0–2 relative to those who had not.

6. Discussion

An intent-to-treat difference-in-difference approach focusing on those children who suffered early life rainfall shocks and for whom HSNP was available in their locality when they were at school-entry ages suggests a potential role of a cash transfer program in mitigating the effects of shocks. Because the outcome measurements are taken in years when there were no contemporaneous rainfall shocks as we measure them, we put the spotlight on whether the

[6] Including only a single control for availability of HSNP at ages 6–9 without the interaction also does not change the substantive conclusions regarding the role of prior rainfall shocks. Additionally, we considered estimates of equation (2) including a binary indicator for HSNP availability in the location and, although point estimates on the indicator are generally positive and significant, the effect of the 2005 shock on the outcomes is unchanged (not shown).

program mitigated the effects of past shocks. Consistent with findings from the conditional cash transfer literature, we find suggestive evidence that a program providing transfers without conditions had modest significant intent-to-treat effects for individuals who had suffered early life shocks relative to those who had not.

Although it had a number of other impacts on household wellbeing, the formal evaluation of the HSNP program found that children in beneficiary households were not more likely to enroll in or attend school on average (Merttens *et al.*, 2013). Our results hint at the possibility that HSNP instead may have had heterogeneous effects on schooling outcomes for some of the most vulnerable children: those who had experienced negative shocks early in life. Under the plausible assumption that the overall average impact of HSNP on schooling was non-negative, the net effect for those experiencing early life shocks was positive and offsets about one-fifth of the reduction for a child who had suffered a shock in their first few years. In addition, because the program covered less than half of the sample, had more households been covered it is probable that the intent-to-treat impacts would likely be even larger.[7]

The findings in this appendix note demonstrate first that the main results regarding the effects of shocks on educational outcomes are generally robust to (imperfect) controls for HSNP, an important program in the region. They also suggest the program operated in part as an ex post strategy against risk. Consequently, there are additional potential benefits for such cash transfers or similar social protection programs, increasingly common tools in development strategies (Brück *et al.*, 2019). At the same time, however, the results suggest that such ex post social protection transfers alone are unlikely to address the full range of factors leading to delayed

[7] The HSNP began operations in 2009 and rollout progressed steadily. By 2013, about one-quarter of the 80 AGI-K survey locations and one-half of the HSNP locations were at least partially covered.

schooling enrollment and low human capital in fragile environments. Interventions coincident to the shocks themselves – including if the shocks occur in early life – may be required to fully redress the negative impacts.

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Table A3. Rainfall shocks, educational outcomes and HSNP (HSNP census girls and boys 9–14)

	Ever enrolled (=1)		Completed Grades	
	(1)	(2)	(3)	(4)
Rainfall shock age 1	-0.092***	-0.059	-0.833***	-0.765***
	(0.034)	(0.038)	(0.183)	(0.205)
Rainfall shock age 2	-0.075**	-0.036	-0.656***	-0.485**
	(0.034)	(0.038)	(0.181)	(0.203)
Rainfall shock age 3	-0.069**	-0.055	-0.477**	-0.498**
	(0.035)	(0.039)	(0.187)	(0.208)
Rainfall shock age 4	-0.062*	-0.033	-0.228	-0.118
	(0.034)	(0.038)	(0.180)	(0.200)
Rainfall shock age 5	-0.059*	-0.035	0.068	0.160
	(0.033)	(0.037)	(0.170)	(0.191)
Rainfall shock age 6	-0.064**	-0.001	-0.064	0.257
	(0.030)	(0.035)	(0.162)	(0.180)
Rainfall shock age 7	-0.014	0.027	-0.061	0.003
	(0.032)	(0.036)	(0.173)	(0.189)
Rainfall shock age 8	-0.068***	-0.026	-0.405***	-0.222
	(0.026)	(0.029)	(0.155)	(0.161)
Rainfall shock age 9	-0.041	0.022	-0.407**	-0.166
	(0.028)	(0.033)	(0.171)	(0.190)
Female	-0.104***	-0.119***	-0.581***	-0.651***
	(0.003)	(0.004)	(0.018)	(0.021)
HSNP age 6-9	0.004	0.016*	0.032	0.063
	(0.008)	(0.008)	(0.042)	(0.046)
HSNP age 6-9 X Early life shocks	0.017*	0.003	0.192***	0.134**
	(0.010)	(0.011)	(0.056)	(0.061)
N	93,418	93,418	93,418	93,418
P-value overall F test	<0.001	<0.001	<0.001	<0.001
Fixed Effects	Location	Household	Location	Household

Notes: *** indicates significance at p<0.01, ** at p<0.05 and * at p<0.10. All models include but do not show age dummy variables and location or household fixed effects as indicated. Standard errors are calculated allowing for clustering at the household level.