Can spatial mobility insure families against long-term impacts of economic shocks?

Evidence from drought and disability in South Africa

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Geographic, institutional and political barriers to mobility may constrain a household’s ability to cushion income shocks through labor migration. This paper asks whether these constraints have long-term consequences. I combine cross-sectional variation in South Africa’s apartheid-era mobility restrictions with cross-sectional and temporal variation in drought to investigate whether migration barriers prevent families from fully insuring against long-term health impacts of local shocks. Triple difference estimates using Census data indicate that childhood drought exposure in restricted areas raises male disability rates by 17%, while drought induces two to four times more adult outmigration from areas with the weakest mobility restrictions. Importantly, I show that remittances rise in response to local droughts. Together, these results identify a specific health channel through which barriers to mobility impose long-run economic costs. [127 words]

Key words: spatial mobility, labor migration as insurance, local economic shocks, long-term health outcomes, disability, drought, South Africa

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Geographic, institutional and political limits on mobility often constrain a household’s ability to insure against income shocks using labor migration. Such constraints are key features of many developing countries that contribute to income volatility in the poorest households. This paper investigates a novel channel through which restrictions on spatial mobility can impose long-run economic costs and undermine the growth potential of local economies. I ask: do barriers to labor migration exacerbate negative effects of local economic shocks on long-term health outcomes? Said differently: Can spatial mobility help families insure against the long-run health effects of childhood exposure to local shocks?

Answering this question is empirically challenging since any study of the causal effects of migration runs into endogeneity concerns. Migration decisions typically depend on the expected returns to migration (Sjaastad 1962; Kennan and Walker 2011) which in turn depend on individual-specific costs and benefits that are often unobserved or unobservable. Selection bias is likely to contaminate comparisons of outcomes across migrant and non-migrant families or across high- and low-migrant areas. Likewise, it would be difficult to identify the long-term effects of spatial mobility by studying the effects of policies shifting a country towards more spatially integrated labor markets. Such a research design risks confounding changes in spatial labor mobility with more general changes in market access.

My paper solves these identification challenges by estimating how differences in the long-term effects of local shocks differ across more and less mobility-restricted areas, controlling for average observed and unobservable differences between these areas (a difference-in-difference-in-differences research design). I examine what happens to health outcomes and to adult migration in response to highly prevalent local weather shocks across areas facing different levels of mobility restrictions. Given a set of externally imposed limits on spatial mobility, I ask

2 Rosenzweig (1988) writes: “Any barriers to the reallocation of labor resources accompanying economic development are potentially critical impediments to further income growth”. Clemens (2011) argues that barriers to international emigration from poor countries have first order effects welfare effects in these areas.

3 Uncertainty about returns in a distant location may also affect the calculation of expected returns to migration (Bryan, Chowdhury and Mobarak, 2012).

4 De Brauw and Giles (2008) provide a good discussion of selection bias in migration studies.

5 For example, Redding and Sturm (2008) analyze the total population effects of the reunification of East and West Germany which changed market access at a very broad level.
whether drought exposure in early childhood differentially affects later-life health outcomes (disability rates) among individuals born into differentially restricted areas. To provide direct evidence on the migration mechanism, I ask whether outmigration responds differently to local drought across more and less mobility-restricted regions, and whether remittances respond to local drought events.

My analysis centers on South Africa during the apartheid period when a host of policies restricted African rights of movement, residence and employment in the modern sectors of the economy. Between 1948 and 1986 the South African government consigned a majority of Africans to live in pockets of land – called homelands – that were spatially isolated from the modern economy. As I explain in Section 1, legal restrictions on movement accumulated throughout apartheid and the oldest rural homelands— the TBVC states6 – ended up facing the highest externally imposed barriers to permanent labor migration for the longest time. My basic identification strategy compares the effects of multiple local droughts in different years across rural TBVC and non-TBVC homelands, controlling for differences between these areas in non-drought years and for year fixed effects. To implement this strategy, I use 1996 Census data to measure health and migration outcomes for individuals who have ever lived in the former homelands and match this to measures of exogenous local drought.

The validity of this triple difference research design and the interpretation of results depends on two crucial assumptions. The first is that, controlling for year and district fixed effects, there are no contemporaneous shocks to health or to outmigration that are coincident with drought and that differ by TBVC and non-TBVC areas. The second is that TBVC and non-TBVC areas differ primarily in limitations on migration: there should be no additional differences between areas that systematically affect health or outmigration during drought years.

I provide support for these assumptions in four different ways. First, I show that TBVC and non-TBVC districts have similar socio-economic and demographic characteristics. Second, I control for any constant differences between TBVC and non-TBVC districts using district fixed effects in all regressions. Third, I use many separate natural experiments for economic shocks identified

6 TBVC stands for Transkei, Bophuthatswana, Venda and the Ciskei.
by drought events across years and districts. These multiple drought events minimize concerns that confounding shocks correlated with drought drive results. Finally, I control for differences in key baseline district-level variables that could explain my results: historical population density, the suitability of land for maize production, and district remoteness. Results are robust and in some cases, strengthened, when controlling for interactions of these district-level control variables with drought measures.

The empirical evidence indicates that limited spatial mobility exacerbates negative health effects of early childhood drought exposure. Cumulative drought exposure from the *in utero* period up to age four raises disability rates for African males in all districts, yet these negative effects are twice as large for African males in TBVC relative to non-TBVC areas. For African males from TBVC areas, drought exposure at birth significantly raises the probability of serious disability by about 0.9 percentage points relative to males from non-TBVC areas. Vision and physical disabilities among males account for most of this result.

These estimated disability effects are large and economically meaningful. Drought in TBVC areas raises disability rates by 17% relative to the mean, consistent with a large literature on the fetal and childhood origins of health. Drought-induced changes in cohort size (through fertility and mortality adjustments) suggest that these estimates likely underestimate the total disability burden experienced by males born into TBVC homelands, and the effects are particularly striking given the relative youthfulness of the sample, aged 10 to 48.

The empirical evidence on outmigration confirms that the TBVC status of a district captures differences in external limits on free labor mobility. Using the same triple difference strategy, I

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estimate rates of permanent adult outmigration in drought years from TBVC and non-TBVC areas during *apartheid*, controlling for differences in outmigration rates in non-drought years and for year and district fixed effects. Since there are no comprehensive historical data on migration in South Africa, I present a novel method to learn about migration histories using cross-sectional Census data. Using information about a person’s “last move” and her current and prior districts of residence, I construct a pseudo-panel dataset capturing each person’s place of residence between 1948 and 1986 and the year that they moved out. I match this outmigration data to drought indicators at the district-year level and estimate how outmigration at the district-year level responds to drought. I show that measurement error in migration status (which only arises for individuals who move more than once) biases outmigration and health estimates downwards with sufficiently small fractions of repeat movers, as is the case in South Africa.

Drought induces significantly more adults to move away from non-TBVC areas than from TBVC areas: 0.08% to 0.09% more adults leave these non-TBVC districts during drought, or in the year following a drought. This represents a greater than 30% increase in outmigration relative to the annual mean. In contrast, the increase in outmigration from TBVC areas is substantially lower, at only 10-12% of the annual mean. A back-of-the envelope calculation that combines the disability estimates with these outmigration effects implies that for each potential adult migrant who was unable to migrate away from a TBVC district during a drought, there are between 4.3 and 7.8 additional disabilities among male children. These effects are large but not implausible given the prevalence of extended family households and the magnitude of remittance contributions to total household income in these rural areas.

As the development literature on migration emphasizes, remittances are the primary connection between permanent adult outmigration and welfare in households left behind. Migrant incomes earned in distant, unaffected labor markets can be remitted back home to smooth incomes in anticipation of, or in response to, income shocks (Rosenzweig 1988). While historical data on

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8 Rosenzweig (1988) discusses remittances in his review of labor markets in developing countries; Yang (2011) provides an overview of the remittance channel in the context of international migration. Evidence from international migration suggests that cross-country remittances smooth incomes in Botswana (Lucas and Stark 1988), Mexico (Amuedo-Durantes and Ponzo 2011) and the Philippines (Choi and Yang 2007).
remittances during *apartheid* does not exist, data from more recent years reflects the continued importance of these migrant and remittance linkages in South Africa. I use the same Census data to show that remittances in 1996 respond to drought events in 1995. Controlling for historical drought prevalence in each district and the presence of any migrant worker attached to the household, I estimate that households in former homeland districts are between 13% and 16% more likely to receive remittances in 1996 if there was a drought in their district in 1995. Remittance flows into households with migrant workers drive this effect. These remittance results provide recent evidence that drought-induced adult outmigration from homelands during *apartheid* likely facilitated flows of money in the opposite direction.

My paper makes four main contributions. First, I extend the development literature on migration as insurance by showing that limits on spatial labor mobility negatively affect long-term outcomes that undermine human resources for economic growth: the prevalence of disabilities in later life. The higher prevalence of vision and physical disabilities among drought-exposed male cohorts in mobility-restricted areas in South Africa likely imposes long-term costs on local economies by limiting the productivity of prime-age workers. My results motivate an even larger insurance role for spatial mobility than the literature has previously emphasized, and also resonate with a more recent literature on the welfare costs of global warming and climate change when migration is limited.

Second, I connect this migration as insurance literature with the research on early-life health shocks by highlighting migration as one specific mechanism through which families can at least

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9 In one of the first studies of migration as insurance, Rosenzweig and Stark (1989) structurally model how marriage migration in rural India implicitly insures households against agricultural income risk and find evidence for their model in the six ICRISAT villages. Taking a different angle on migration, Jayachandran (2006) shows that limited access to outside labor markets exaggerates negative wage effects of local productivity shocks by swelling the ranks of local labor supply. In contrast to this focus on the immediate and short-run benefits from labor mobility, my paper emphasizes how labor migration can insure against negative long-term effects of these economic shocks.

10 Desmet and Rossi-Hansberg (2012) argue that any migration frictions limiting adaptive responses to climate change will exacerbate the welfare costs of these changes. The South African case illustrates that negative long-run health effects may be an important component of these welfare costs.
partially mitigate the impact of negative economic shocks on children.\textsuperscript{11} Third, I provide evidence on the specific channels through which spatial mobility can insure against the impact of local income shocks: outmigration and remittance inflows in response to drought. Fourth, I exploit exogenous variation in local economic shocks and exogenous variation in mobility restrictions across areas to establish the long-term health effects of limited mobility and to identify the migration channel. The South African case allows me to focus on mobility restrictions that are exogenous to the individual, thereby getting around some of the most difficult selection issues associated with migration. This context presents a unique opportunity to identify a specific channel through which barriers to spatial mobility impose long-run economic costs.

The main results have implications for policy. \textit{Apartheid}-era restrictions on labor migration represent one example of spatial segregation. Similar legal restrictions have controlled internal migration in China, Israel, Malaysia, Russia, and the Ukraine in the past. Most countries restrict travel and employment for foreigners using external passport and visa controls. In other settings, the nature of the physical terrain or inadequate transport infrastructure spatially segregates labor markets. The empirical evidence from South Africa demonstrates that spatial integration of labor markets may generate significant welfare gains for poor countries.\textsuperscript{12} By enabling family members to more easily work in distant labor markets and send remittances homewards, such spatial mobility may reduce the negative health impacts of highly prevalent environmental events like drought.

The paper begins by setting out the historical background on labor mobility restrictions in South Africa. Section 2 describes my identification strategy, Section 3 describes data, key variables and

\begin{footnotesize}
\textsuperscript{11} Strauss and Thomas (1995) discuss parental decisions about child health investments in response to shocks although there is as yet little work on these mitigating mechanisms in the early-life health shocks literature (Almond and Currie 2011a, Currie and Vogl 2013).

\textsuperscript{12} This point is related to Burgess and Donaldson (2010), who suggest that price reductions and quantity volatility brought about by more open markets can theoretically mitigate the impact of negative weather events. However, when farming households subsist on agriculture or livestock, product market integration is unlikely to provide substantial protection against large negative income effects associated with drought. Labor migration may therefore help families respond to drought even if product markets are integrated.
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measurement error issues and Section 4 presents the main results for disability, outmigration, remittances, and population composition effects. Section 5 concludes.

1. Labor markets and restricted mobility in South Africa during apartheid

Throughout the years of formal apartheid (1948-1994) in South Africa, Africans were never entirely free to move around the country for work or other reasons. The white government implemented highly organized systems of documentation and control to allocate African labor to white firms, farms and households. For example, all Africans were legally required to carry pass books with up-to-date work and travel permissions and to produce them on demand. Job seekers were required to register at local labor bureaux for permission to work rather than being able to search freely in the labor market.

The creation of ten homeland “states” or Bantustans during the 1950s, 1960s and 1970s was a crucial part of the strategy to control African mobility (termed “influx control” by the apartheid state). These homeland areas, established largely on pre-existing Native Reserve areas, provided a space to locate excess labor and non-labor population resources (women, children and the aged) far from urban centers, in rural parts of the country where land quality was generally poor. Several million Africans had been resettled in these homelands by the 1960s (Simkins, 1983). My empirical analysis focuses on how residents of these different homelands were differentially able to respond to local drought.

The four earliest homelands (Transkei, Boputhatswana, Venda and Ciskei, or TBVC areas) were formalized in prior Native Reserve areas between 1959 and 1962. QwaQwa, KwaZulu, Gazankulu, Lebowa, Kangwane and KwaNdebele (the remaining non-TBVC homelands) were

13 Wolpe (1988), Simkins (1988), Lemon (1984), Savage (1986) and Maylam (1990) discuss the main policies of population control under apartheid. Reed (2012) provides empirical evidence on migration trends over different historical periods before and after apartheid. Lemon (1984) writes “Probably no avowedly capitalist country controls its labor market to the same degree as South Africa….State restrictions on freedom of movement continue to hinder Africans in particular from selling their labor freely.” Describing twentieth century population distributions, Simkins (1983) concludes that South Africa was under-urbanized relative to other countries at the same level of economic development in the early 1980s, largely due to the policies of spatial segregation and labor mobility restrictions.

14 Native Reserve Areas were demarcated as early as 1913 and expanded in 1936. Evans (1997) describes how fear of massive demographic shifts in white urban areas provided a primary motivation for the establishment of these reserves to house the majority black population.
established much later in the period, between 1969 and 1977. Reasons for this staggered formalization were primarily political: the apartheid government required local chiefs to be sufficiently compliant and had to establish bureaucratic structures in these rural areas to ensure continued control of homeland administrations (Evans 1997).

Once a homeland had been established, Africans assigned to and living in that homeland area had extremely limited rights to live and look for work outside of this area. They were prohibited from migrating freely between homelands as well as between homelands and urban areas. Instead, official permission for labor migration had to be sought from labor bureaux located within the assigned district (Greenberg and Giliomee 1983). These offices registered jobseekers, posted job requisitions received from South African companies, and certified job contracts, enabling workers work legally in South Africa. Signing up at the local labor office was a necessary step for all migrant workers, making these labor offices the official gatekeepers for legal African labor migration out of the homelands.

Between 1916 and 1984, nearly 18 million “Pass Law” arrests were made for illegal movements of Africans (Maylam 1990). This indicates both that legal restrictions on mobility were not completely effective at preventing movement (Reed 2012) and that such restrictions raised the costs of free internal migration for Africans. These costs were higher for residents of the homelands, the focus of this study. And among all of the homelands, the costs of migration were highest for residents of the TBVC areas.

These four homelands were the first to be established, and hence the first areas to be subject to all of the apartheid bureaucracy established to enforce “influx control”. They were also the only homelands to eventually be granted political independence from South Africa. In the latter part

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16 In Secretary for Bantu Administration and Development General Circular No. 25 (1967), “1. It is accepted Government policy that the Bantu are only temporarily resident in the European areas of the Republic, for as long as they offer their labour there. As soon as they become, for some reason or other, no longer fit for work or superfluous in the labour market, they are expected to return to their country of origin or the territory of the national unit where they fit in ethnically if they were not born and bred in the homeland.”
of the sample period, approximately 8 million TBVC residents lost their South African citizenship, and were required to use passports to enter South Africa (Savage 1986). For both of these reasons, Africans faced tougher restrictions on migration from TBVC areas throughout the period of our analysis. In the absence of more exact data on the dates of establishment and operation of local labor bureau within each district, using the information on early versus late homelands provides a fuzzy but still salient way to measure spatial differences in labor mobility restrictions. Moreover, using the set of districts designated as homelands later in the period as “controls” for the earlier homelands is a cleaner comparison than using non-homeland rural areas as comparison groups, since homeland areas tended to be established on the lowest productivity land, making living conditions particularly poor in these areas. 17

Figure 1 shows the ten homelands scattered across the country. I use maps like Figure 1 and ArcGIS software to spatially identify which modern district boundaries cover a “majority homeland or TBVC area” and assign each district a value of $TBVC_d=1$ or 0 based on this spatial match. The Data Appendix describes this assignment in more detail. The TBVC indicator is the broad measure I use through much of the analysis to proxy for external restrictions on spatial mobility. Since this indicator does not vary over time, I also experiment with a narrow definition of TBVC status that takes a value of one in TBVC districts only during their years of de jure independence (when mobility restrictions were arguably the highest), and is otherwise zero. My outmigration results are robust to this narrow definition.

Despite the historical discussion above, there is still a concern that homelands may differ in dimensions other than restricted labor mobility, and that these other dimensions condition the ability of families to cope with drought. Key suspects for these other differences include access to credit to help families cope with income shocks, access to public health services, and differences in agricultural productivity of land across homeland areas. While district fixed effects

17 As late as the 1990s over half of rural African adults still consumed under 2,100 calories per person per day (Wilson 1996). Using data from the South African National Income Dynamics Panel, Mariotti (2011) shows that an acute income shock (related to labor demand) in some of the homelands in the mid-1970s led to height improvements among African men. 18 If anything, TBVC states had marginally more per capita resources for health care than the non-TBVC states, since the government used spending promises to promote the adoption of political independence (De Beer 1984).
can account for level differences in these variables, any such differences may make it difficult to attribute differential responses to drought to differences in spatial mobility restrictions.

In addressing this concern, it is important to keep in mind that all manner of social services (like hospitals) and private services (like banks) were fairly uniformly underprovided in the rural homeland areas. During the period I study, access to formal and informal credit and savings products was low in both TBVC and non-TBVC areas, since banking facilities for Africans were largely non-existent during apartheid. As late as 1995 less than 20% of African households in former homeland areas reported having any formal or informal savings products (South African Income Expenditure Survey, 1995, own calculations).

The situation for health services was similar: all of the homelands suffered from a dire under-provision of public health facilities (Brown 1987). South Africa spent only 0.23% of GDP on health services in the homelands by the mid-1970s – an area where 32% of the population resided (Price 1986). This chronic underfunding contributed to high baseline levels of malnutrition in all homelands and a high prevalence of kwashiororkor and marasmus, respiratory infractions, gastroenteritis, and measles among children (De Beer 1984). While poor baseline levels of health probably made the homeland populations more susceptible to drought shocks overall, there is no reason to expect that baseline health was worse in TBVC states than in non-TBVC states.

These two examples of how TBVC and non-TBVC homelands were systematically underserviced are representative of the situation during apartheid. However, the skeptical reader may not be convinced by this historical context, since TBVC areas are somewhat more remote and even more rural than non-TBVC areas. To provide empirical evidence that differences in access to services across the homelands do not account for differential responses to drought, I control for interactions of drought variables with district level population density in 1946 and with a measure of geographic remoteness of the district. Both of these variables are plausibly correlated with differences in access to social services (as well as access to markets and jobs)

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18 If anything, TBVC states had marginally more per capita resources for health care than the non-TBVC states, since the government used spending promises to promote the adoption of political independence (De Beer 1984).
that could affect how families respond to drought. Finally, since agricultural productivity differs across the homeland areas, I also control for the interaction of an index of suitability for maize production (the local staple) and drought measures. As long as the health results do not dissipate once these baseline district-level variables interacted with drought are included, we can be more confident that differences in health outcomes do indeed arise from differential mobility restrictions in place in the TBVC and non-TBVC areas.

2. Empirical strategy

The key empirical challenges in this paper are (1) how to identify the long-term health impacts of drought, and (2) how to isolate the role that spatial mobility plays in mitigating these effects. To address these challenges, I estimate the impact of local droughts on health outcomes and on outmigration across differentially restricted labor markets in a difference-in-difference-in-differences research design.

For the health analysis, I use Census data on each person’s birth district to estimate the effect of drought exposure during early childhood in the place of birth on disability later in life. Using individual-level observations on disability and on drought exposure, and controlling for birth district $\mu_d$ and birth year $\phi_t$ fixed effects, I estimate

$$Y_{jdt}=\delta_0+\delta_1DROUGHT_{dt}+\delta_2DROUGHT_{dt}*TBVC_d+\mu_d+\phi_t+\omega_{jdt}$$ (1)

where $Y_{jdt}$ indicates disability status for a range of disability outcomes (or the total number of disabilities) reported for person $j$ born in district $d$ in year $t$. $DROUGHT_{dt}$ is one of two measures of drought exposure in early life and $TBVC_d$ indicates whether the birth district falls within the boundaries of a TBVC state or not. I estimate this specification for an “Any Disability” indicator using the full sample as well as separately for men and women, since prior research documents the particular sensitivity of males to early-life nutritional insults (Almond and Mazumdar 2011, Almond 2006; Almond and Currie 2011a).

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19 Birth district is captured by a person’s prior district of residence. This means that birth district is measured with error for some individuals. I discuss this measurement error in the Measurement Error Appendix.
The economics literature has recently provided much evidence for the fetal origins of disease, or, the long-term health effects of nutritional deprivation and exposure to environmental toxins in utero. However, infants and children continue to be vulnerable to malnutrition and infection after birth, also with potential long-run consequences. Martorell (1999) indicates that until 3 years of age, the physical and mental development of a child remains in the critical period because growth rates are higher during this time than in any other period of life and because infant immunological systems are still developing. Even mild malnutrition and associated micronutrient deficiencies (vitamin A, folic acid, zinc, iodine, and iron) during this period can contribute to a syndrome of “developmental impairment” that includes growth failure (stunting and wasting), delays in cognitive, motor and behavioural development, lower levels of resistance to disease, and increased morbidity and mortality (Martorell 1999).

I measure drought exposure ($DROUGHT_{dt}$) in two ways, both of which vary at the birth district and birth year level. The simplest measure is an indicator for whether district $d$ experienced a drought in year $t$ (the year of birth of the individual) or not. The second measure captures the fraction of years from in utero to four years of age that a person was exposed to drought in their birth district. By exploiting the fact that some individuals were exposed to multiple drought events in early childhood, depending on their year of birth and place of birth, this second measure captures the intensity of drought exposure at a range of critical ages when nutritional deficiencies can affect later health outcomes.

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20 Almond and Mazumdar (2011) present a detailed discussion of the biological mechanisms accounting for the link between in utero deprivation or disease and negative later life health outcomes.
21 For example, the critical period for the development of binocular vision is between 3 and 8 months, while neuronal development of the vestibular system (which affects motor skill development) has a critical period in the first three weeks of life (Rice 2000).
22 In the well-known INCAP study in Guatemala, a randomized nutritional intervention for pregnant women and infants led to taller, heavier adults with greater strength and work capacity, and higher wages (Martorell 1999, Hoddinott et al 2008). Schroeder et al (1995) show that nutritional intervention before three years of age had the largest impacts on health outcomes for children. Case and Paxson (2010) use data from numerous surveys to show that height deficiencies in childhood (an outcome of nutritional deprivation) are strongly correlated with significantly higher rates of disabilities and poorer health in later life.
23 Although the Census questionnaire asked for month of birth information, these variables are not reported in the public data, restricting our analysis of drought exposure to annual periods. However, the annual measure of drought is a reasonable measure of exposure in early life, since precipitation deficits measured over a longer period of time more accurately reflect conditions of drought than acute rainfall shocks over shorter periods. To interpret the
Birth year fixed effects in equation (1) account for age effects in health outcomes and for any contemporaneous national shocks affecting these outcomes. District fixed effects control for constant unobservable differences between districts that may affect health. For example, some districts may be drought-prone, have different access to public health facilities, or different approaches to child health investments.

The parameters of interest are $\delta_1$, the effect of drought in early childhood in non-TBVC areas and $\delta_2$, the differential effect of drought exposure in early childhood in TBVC relative to non-TBVC areas. Within-district variation in birth timing relative to drought events identifies $\delta_1$ and $\delta_2$. If drought negatively affects health in both areas, disability rates will be higher for cohorts with greater early-life drought exposure ($\delta_1 > 0$ and $\delta_2 \geq 0$). If families in TBVC areas are less able to respond to drought, then $\delta_2$ will be strictly greater than zero.

Interpreting what accounts for estimates of $\delta_2 > 0$ is the second key challenge of the paper. Because drought may be severe enough to affect total population and total fertility (either directly or in response to increased child mortality Almond and Mazumdar 2011, Martorell 1994) an important concern is whether drought effects on population differ across TBVC and non-TBVC areas in ways that would confound estimates of $\delta_2$. For example, if fertility rates in drought versus non-drought periods are lower in TBVC areas relative to non-TBVC areas, a negative survivor bias may explain $\delta_2 > 0$. I explore this possibility in the final section of the paper and find that differences in (log) cohort size across TBVC and non-TBVC areas and differential changes in completed fertility instead support a positive survivor bias story: that is, estimates of $\delta_2$ likely underestimate the true impacts of drought in TBVC relative to non-TBVC areas, since cohorts are significantly smaller in these areas after drought exposure.

fractional measure of drought exposure, it is important to note that permits were never granted for children to move outside of homelands with any parent. Hence, district at birth is likely to be the same district throughout early childhood for this sample. An alternative way of capturing drought exposure in early childhood would be to include multiple indicators for drought in each year of life (rather than the fractional measure). Estimates of health results using this specification are broadly similar, but point estimates on each year of exposure variable are noisy. Since this paper focuses on how families cope with drought rather than on identifying the exact timing of when drought exposure in infancy matters the most, I do not present estimates from these alternate specifications.
To provide evidence that \( \delta_2 > 0 \) is driven by differences in migration barriers during *apartheid*, I analyze how outmigration rates change across TBVC and non-TBVC areas in response to drought events. I specify a regression similar to (1) at the district-year level and use Census data on each person’s district of residence between 1948 and 1986 (data are described in detail in the next section) to construct district-year (\( dt \)) level measures of outmigration. I estimate the following equation for the percent of adults in district \( d \) in year \( t \) that migrate away from this district in year \( t \) (\( \text{PERCENTMOVE}_{dt} \)), controlling for district \( d \) and year \( t \) fixed effects:

\[
\text{PERCENTMOVE}_{dt} = \beta_0 + \beta_1 \text{DROUGHT}_{dt} + \beta_2 \text{DROUGHT}_{dt} \times \text{TBVC}_d + \delta_d + \delta_t + \epsilon_{dt} \quad (2)
\]

where \( \text{TBVC}_d \) is defined as in equation (1) and \( \text{DROUGHT}_{dt} \) is an indicator for whether district \( d \) experienced a drought in year \( t \). Since migration may respond in the year of a drought, or in the year following, I estimate an extended specification adding indicators for drought in the year before \( t \) and the interaction of this with \( \text{TBVC}_d \). If people respond in anticipation of a coming drought, we might expect to see migration increase in the year before a drought too. To capture this possibility, I include an indicator for drought in the year after \( t \) and its interaction with \( \text{TBVC}_d \).

For both disability and migration outcomes, the main levels of variation in my data are at the district-year level. Since I am most concerned that health, migration and drought measures could be spatially correlated across districts in the same year (Bertrand, Duflo and Mullainathan 2004), I adjust for within-year error correlations across districts by clustering on year of birth in the health outcome regressions, and on year of migration in the outmigration regressions.\(^{25}\)

Year fixed effects in equation (2) control for year-specific common shocks to outmigration, for example, a national drought, or the nationwide intensification of Pass Law enforcement (Lemon 1984). District fixed effects control for persistent level differences in unobservable

\(^{24}\) Rather than collapse data to the magisterial district level (an administrative unit demarcated in the 1996 Census), I use a higher level of aggregation (the district council) to ensure there are enough observations in each geographic unit. Details on all datasets, data aggregation and variable construction appear in Data Appendix 1.

\(^{25}\) Drought conditions *within* a district are not correlated over time. After controlling for year and district fixed effects there is no first order serial correlation in errors for the drought indicator, or for rainfall shocks. Coefficients from lagged residual regressions for drought, SPI and rainfall shocks are: .025 (\( p \) value = .36), .038 (\( p \) value=.186) and .038 (\( p \) value=.192)).
characteristics affecting outmigration across districts. For example, pre-existing migrant networks may be stronger in some districts than others. The key parameters of interest are $\beta_1$, the effect of drought on outmigration from non-TBVC areas and $\beta_2$, the differential effect of drought on outmigration from TBVC relative to non-TBVC areas. Given the discussion in Section 1, I expect $\beta_1 > 0$ and $\beta_2 < 0$ for the apartheid period. Such a pattern of signs would confirm that TBVC residents faced real differences in spatial mobility restrictions.

A key identification assumption in equations (1) and (2) is that there are no contemporaneous shocks to outmigration or health during drought years. This assumption rules out (for example) labor demand shocks emanating from the predominantly white economy that fall disproportionately on either TBVC or non-TBVC districts in a drought year.\textsuperscript{26} Having many separate natural experiments (multiple drought events in different districts across many different years) to identify the effects of drought makes the assumption of no contemporaneous shocks reasonably defensible.

Causal estimates of the differential effects of drought on disability and outmigration across TBVC and non-TBVC areas provide reduced form evidence for the impact of spatial mobility on long-term health. For outmigration to be the primary channel through which families living in TBVC homelands mitigate the impact of drought on infant health, I additionally assume that (after controlling for district fixed effects) TBVC and non-TBVC residents differ primarily in the limits they face on free labor mobility. This assumption is motivated by the historical context discussed in Section 1 and supported by the robustness checks that control for interactions of baseline district variables (population density in 1946, remoteness and a maize suitability index) with drought measures.

Table 1 shows that TBVC and non-TBVC areas are similar on a range of observable characteristics: the adult sample is balanced on age (23 years), education, and individual-level

\textsuperscript{26} Mariotti (2011) analyzes one such shock: she examines an acute shock to labor demand from the Transkei generated by increased demand for domestic labor on the gold mines of the Witwatersrand in the mid-1970s. However, this positive labor demand shock affected the TBVC areas rather than non-TBVC areas.
exposure to drought in birth year and throughout early childhood. Differences in the fraction of women in TBVC and non-TBVC areas are significant at the 5% level, but small.

TBVC areas are also more remote than non-TBVC areas, and are less suited to maize production (the staple grain) than non-TBVC areas, although these differences are not statistically significant. They are characterized by lower population density than non-TBVC areas; since lower population densities are likely correlated with lower provision of social services, we might worry that households in TBVC areas are simply unable to cope with drought as well as households in non-TBVC areas. Hence, as an important robustness check on the health results, I controls for these district level variables interacted with the relevant drought measure. If the triple difference estimates fall towards zero after introducing these controls, we would worry that population density, remoteness or agricultural potential of the land account for differential disability responses to drought. Alternatively, if the results survive these controls, we can be much more confident that such fundamental differences between TBVC and non-TBVC districts do not explain the long-term health effects of drought exposure.

The differences in mean disability rates across TBVC and non-TBVC areas in Table 1 foreshadow the main health results. For all outcomes, disability rates are higher (and sometimes substantially higher) for the TBVC sample than for the non-TBVC sample. Of course, constant differences in disability rates across areas are controlled for by district fixed effects. The regression specification in (1) seeks to estimate the differential differences in disability rates in drought versus non-drought periods.

Population totals and composition are also potentially sensitive to drought exposure among women of childbearing age. Table 1 indicates higher fertility rates for female cohorts who have completed childbearing by 1996 and who live in TBVC areas, relative to non-TBVC areas (4.9 children per woman versus 4.7). This suggests that endogenous fertility responses may contribute to some of the health effects.

At the individual level, the percent of adults moving out of a TBVC area in any given year is significantly lower (0.21%) compared with non-TBVC area outmigration (0.25%), reflecting the higher restrictions on mobility faced by TBVC residents. Overall though, outmigration rates from all areas are low, since all of the districts are located in former homeland areas. Finally,
drought prevalence is somewhat higher in TBVC than non-TBVC areas at the district-year level: drought occurs in 6.8% of years for TBVC areas and 4.1% of years in non-TBVC areas, although this difference is not statistically significant ($p$-value = 0.19), and is in any case accounted for by including district fixed effects in equation (2).

The final piece of my empirical strategy addresses the question of whether remittances link adult outmigration from homelands with improved household resources for those left behind during drought. Unfortunately, there are no spatially disaggregated data on remittance flows during *apartheid*. However, long-standing migrant networks in the former homelands allow me to estimate the remittance responses to drought using data from recent years. Looking within households situated in former rural homelands in 1996, I ask whether remittances are more likely to flow into households with a migrant worker than into households without a migrant worker after a drought in 1995. I control for differences in remittance receipts between households in districts that did not experience drought in 1995. Since this specification uses only cross-sectional variation to identify drought effects, I also control for historical drought prevalence to soak up district-level unobservables affecting remittances. Under the assumption that historical drought prevalence adequately controls for unobservable differences between districts that affect remittances, I can treat drought in 1995 as uncorrelated with remaining unobservable factors. I estimate this difference-in-differences regression for all households in the former homeland areas, and separately within TBVC and non-TBVC areas, to provide evidence that remittances do respond to drought – but only in households with migrant workers.

3. Data and key variables

i. Measuring drought

I use rainfall data from over 1,000 weather station locations to construct a district-year specific drought measure using the Standardized Precipitation Index (SPI) (McKee, Doesken and Kleist 1993). The SPI measures the probability of observing a recent rainfall event based on the

27 There is no consensus on how the onset, duration or completion of a drought should be marked (Wilhite, 2001; World Meteorological Organization 2006), however, the climatological literature has shown the robustness of the SPI in capturing precipitation deficiencies that extend over time (Roualt and Richard 2003).
distribution of all rainfall events for a given time scale and place. It characterizes South African
droughts well (Roualt and Richard 2003). Following the climatological literature, I define
\( DROUGHT_{dt} \) in each district \( d \) and year \( t \) to be 1 for values of the SPI below -1.5 and 0 otherwise
(McKee et al 1993).

Many economics papers use rainfall shocks to proxy for short-run income shocks. I focus on
drought rather than rainfall shocks because it is Africa’s most prevalent natural disaster (Bensen
and Clay 1993). Furthermore, South Africa’s staple crop (maize) is rain-fed. Limited water
availability reduces maize output by interrupting growth at several points in the growing season
(Le Roux 2009). Insufficient rainfall over an extended period has particularly negative
consequences for yields. As I show in Data Appendix 1, maize yields appear more sensitive to
rainfall deficiencies than to rainfall excesses. Drought is therefore the relevant measure of an
important local economic shock in South Africa.

Figure 2 shows the distribution of these droughts during apartheid. This is the main source of
variation used to identify the immediate impacts of local shocks on outmigration and the long-
term effects on disability rates across TBVC and non-TBVC areas. Each bar represents the
fraction of TBVC and non-TBVC districts experiencing a local drought in a given year. The
figure shows substantial variation over time: some years are entirely drought-free (e.g. 1975)
while in other years (the early 1980s) over 30% of districts experience drought. In most years, a
smaller, positive fraction of districts experience drought.

\( ii. \quad \text{Measuring disability and population composition using Census data} \)

I use the 10% individual record data from the 1996 South African Census to construct the sample
for the health and population composition analysis. The main sample consists of African
individuals born between 1948 and 1986 (age 10-48) whose current district (for never movers) or
prior district (for movers) is in a rural TBVC or non-TBVC homeland.\(^{28}\) Individuals report

\(^{28}\) Sample selection is discussed in detail in the Data Appendix. Briefly, I restrict the sample to African adults with
non-missing age information who report their current (for never movers) or former district (for movers) is a rural
homeland. Never movers living in non-homeland areas (42% of the African adult rural sample), and movers whose
former district is a non-homeland area are excluded (1% of the African adult rural sample). I exclude movers who
started out in non-homeland areas and moved to a homeland because they would have had different rights to live and
whether they have any serious disability and the type of disability: vision, hearing or speech, mental or physical disability (e.g. paralysis). I construct an indicator for “Any serious disability?” and a “Number of serious disabilities” variable for the main analysis.

Table 1 Panel A shows disability prevalence in the sample. 5.2% of individuals report a serious disability, with vision being the most prevalent disability (2.3%). 1.1% of the sample has a hearing/speech disability and 1.4% have a physical disability; mental disability is reported at much lower rates (0.7%). These disability rates do not merely reflect diseases of old age since the sample includes people aged 10 to 48.

The table also presents information on total fertility rates for women in TBVC and non-TBVC areas. Among cohorts of women who have completed childbearing by 1996 (aged 40 to 60), total fertility rates are about 4.7 children per woman. Women from TBVC areas have significantly higher total fertility rates than women in non-TBVC areas. For each woman in the fertility sample, I compute the fraction of her childbearing years (1951-1996) exposed to drought. Table 1 Panel A shows that drought exposure in this sample of women is slightly higher in TBVC than non-TBVC areas.

### iii. Measuring outmigration using Census data

Migration is difficult to measure well using household survey data. Instead, demographers often turn to Census data to characterize migration. The benefit of Census data is that it provides comprehensive coverage of migrant groups across the country, unlike general household surveys that draw from a subset of districts. The coverage of the Census also allows aggregation of migration data to broader geographic units (for our purposes, the district level). But, can cross-sectional Census data be leveraged to understand the dynamic process of historical migration?

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29 These rates are comparable with disability rates for Uganda reported in Almond and Mazumdar (2011).
30 I use the prior district designation as the relevant location for each woman’s drought exposure.
31 No household surveys or Census data capture comprehensive migration histories in South Africa (Casale and Posel 2006).
I model how outmigration responds to local shocks using a novel way of combining migration information in South Africa’s 1996 Census with assumptions about the extent of repeated migration during *apartheid*. I construct a pseudo-panel dataset of individual-year observations capturing where each person lived in each year using the Census questions: “Where do you live now? Where did you live before this? What year did you move here?” This dataset indicates whether a person moved out of a given district in any given year, based on the “last move” data. Within this pseudo-panel dataset, I observe the total number of adults (ages 18 and over) in each district in each year between 1948 and 1986 and the number who move away from each district in each year. I use this to describe historical outmigration from each district in each year.\(^{32}\) That is, I collapse the data to district-year level and generate the percent of adults living in each district that migrated away in each year (details in the Data Appendix). This \(\text{PERCENTMOVE}_{dt}\) variable is the main outcome in equation (2).

While the “last move” data has rich information on duration of residence information, it possesses some unusual properties. Earlier migrations for the same individual and earlier populations at risk of migrating are rendered invisible by later migrations (Schmertmann 1999). Because of the design of these Census questions, the data do not allow us to observe multiple moves. I address this important data limitation in three ways.

First, to make progress with the empirical analysis I assume that no individual ever moves more than once. This strong assumption implies that Census outmigration histories are complete (Schmertmann 1999). As long as the identification assumptions discussed in Section 2 are satisfied, OLS regressions of equations (1) and (2) provide consistent estimates of the parameters of interest. All of the main results are presented under this assumption.

\(^{32}\) These questions are routinely asked in about 58% of Censuses that collect data on migration (Bell 2005) and in many Demographic Health Surveys (Schmertmann 1999), but are not often used for migration analyses. The demographic literature has tended to use questions on “Where were you five years ago?” and “Where do you live currently” to describe migration transitions. These data are known as N-year ago moves. Although these data are simpler to work with, they contain much less information than the last move data and can miss more recent moves (Schmertmann 1999). Schmertmann (1999), Amaral (2008) and Xu-Doeve (2008) have argued that last move data (rather than N-year ago move data) can consistently estimate migration transitions, albeit with some additional structure and assumptions.
Second, I consider what happens when this assumption of “no multiple moves” fails. I characterize the problem as one of measurement error in binary variables. Misclassification of migration status occurs when a person moves more than once. As long as this error is uncorrelated with measured drought exposure and as long as drought at the district-year level is measured without error, estimates from the migration analysis in (2) will be biased towards zero (Bound, Brown and Mathiowetz 2001) while estimates from the health analysis in (1) are unaffected.

However, in some years, drought misclassifications will be correlated with measurement error in migration status. With measurement error in drought, equation (1) underestimates the impact of drought on disabilities. Bias in the outmigration regressions is more complex because of the correlation between measurement error in the dependent and independent variables in equation (2).

I develop intuition for the form of this measurement error bias in a difference-in-differences regression of outmigration on drought (i.e. ignoring the TBVC interaction term) in the Measurement Error Appendix. In this setting, the fraction of people who move more than once (multiple movers) and the observed (potentially mismeasured) fraction of drought exposures determine the form of measurement error bias in the drought coefficient. As long as there are few multiple movers and a small fraction of individuals exposed to drought, the difference-in-differences comparison always underestimates the impact of drought on outmigration. Recent data from a large nationally representative household survey (the 2007 South African Community Survey) indicates that multiple movements across South African districts are quite rare even in 2007 (only 1% of African adults report moving more than once in the 2002-2007

33 Schmertmann (1999) shows that a “Naïve Estimator” for migration rates built on the assumption that people move at most once performs well as long as there is only a small fraction of the population making multiple moves. The bias in this estimator is always downwards. To my knowledge, none of the demographic literature approaches the problem of how to use these “last move” data through a standard measurement error lens.

34 If drought exposure itself is mismeasured, the bias caused by misclassification of a binary independent variable is still downwards. This could occur if, for example, birth district is mismeasured for multiple movers.

35 The Measurement Error Appendix derives this bias for the regression equation using an individual migration indicator as the outcome. I estimate migration equations using the data at the individual level (rather than at the aggregate district level, as in equation (2)), and find the same pattern of coefficient signs and similar magnitudes in this alternative approach.
period), 22 years after formal mobility restrictions were lifted. Multiple permanent movements during *apartheid* were likely even more rare, largely because of the regime of mobility restrictions in place.

Unfortunately, the analytical approach to deriving the measurement error bias does not extend easily to the triple difference specification in equation (2). As a third and final strategy, I implement a specific robustness check for the outmigration results motivated by the intuition that last move data always underestimates actual migration (Schmertmann 1999) and that last move data is more accurate for recent moves. I restrict the sample to a later period so that last move outmigration data should be more accurate. Although statistical power is lost by reducing the sample size, the sign and size of the main migration results stand up to this robustness check. This suggests that measurement error in migration is unlikely to be large enough to overestimate the effect of drought on migration.

Figure 3 uses the constructed district-year level panel data to show the percent of adults permanently leaving TBVC and non-TBVC areas between 1948 and 1986. The maximum value for outmigration is small in level terms. Only 0.6% of the district-level adult population migrates away in later years.36 There is also a strong upward trend in outmigration rates over time.37 Importantly, Figure 3 shows that outmigration from non-TBVC areas is higher than outmigration from TBVC areas in almost all years; consistent with the idea that spatial mobility was more restricted in TBVC districts under *apartheid*.

4. Main Results

i. Effects of early childhood drought exposure on disability

Table 2 presents the main results from estimating the triple difference regression in equation (1). For each of the outcomes “Any serious disability” and “Number of serious disabilities”, I present  

36 Using just the cross sectional component of the data, only 3% of African adults report migrating in the year of the Census (1996) which is ten years after the end of our sample period. Only 2% of Africans from former homeland areas report moving in 1996. These rates of migration are consistent with 30% of the African adult population in rural areas reporting ever moving in their lifetimes.

37 The upward trend in internal migration is consistent with Reed (2012), who uses life histories from the South African Migration and Health Survey to study migration from the 1950s to the 1990s.
the full sample results for the two measures of drought exposure in columns (1) through (4) and results for male and female samples in columns (5) to (12).

Greater exposure to drought in early childhood significantly raises the chance of having a serious disability. For a mean level of drought exposure, disability rates are 1.9 percentage points higher, and the average number of disabilities is about 40% higher for non-TBVC cohorts. These effects of drought exposure are significantly larger for individuals born into TBVC areas. At the mean drought exposure rate, TBVC individuals are 1 percentage point more likely to have a serious disability, and have 28% more disabilities relative to the mean number of disabilities.

Results using the measure of drought exposure in birth year (columns (2) and (4)) are similar. Drought exposure at birth raises disability rates in both TBVC and non-TBVC areas, with the negative health impacts being three times larger in the TBVC areas. Although neither the main nor interaction effects are precisely estimated in these two columns, drought parameters are jointly significant at the 5 and 10% level respectively. Moreover, in this pooled sample, the effect of drought on disability rates in TBVC areas (the sum of the $\delta_1$ and $\delta_2$) is significantly different from zero at the 5% level. CHECK p VALS

Columns (5) through (12) show that the male sample drives these disability results. Mean drought exposure in early childhood raises the rate of male disability by about 2.3 percentage points and the number of serious male disabilities by 0.026. In TBVC areas, drought exposure almost doubles these effect sizes. Using only the drought in birth year indicator, males born into TBVC areas report 0.9 percentage points higher disability rates. All of the male results are precisely estimated and represent economically meaningful magnitudes. Relative to the average level of disability in the male sample, drought in the birth year in a TBVC area raises male disability rates by almost 20%, while mean drought exposure over the first four years of life raises disability rates by about 40%. I can reject the hypothesis that mean drought exposure has no effect on the chances of disability and the number of disabilities for males later in life at the 1% level ($p$-values of the joint tests are 0.009 and 0.008 respectively).

In contrast, mean drought exposure in childhood and drought in the birth year has no differential impacts on disability rates among females from TBVC relative to non-TBVC areas areas. I
cannot reject the joint hypotheses that drought in the birth year has no differential impact on female disability rates in TBVC areas (p-value of the joint null is 0.16 and 0.22 for any disability and number of disabilities respectively). And, although all of the mean drought exposure variables are jointly significantly different in the female regressions of columns (9) and (11), the point estimates are far smaller than in the male regressions, and almost zero on the TBVC interaction terms. These sex differences in the disability effects of drought are consistent with theories that male infants are more fragile than female infants (Barker, 1995; Kraemer 2000). Moreover, the magnitudes of these effects match up with what other researchers have found for different sources of nutritional deficiencies in early childhood (Almond and Mazumdar 2011). For the rest of the disability analysis, I focus on understanding results for the male sample.

Table 3 provides some robustness checks on male health results. Columns (1), (2), (5) and (6) of the table reproduce the male results from Table 2. The remaining columns present results for the same outcomes from regressions that control for interactions of baseline district-level variables (population density in 1946, remoteness and agricultural potential) with drought measures. Almost all of the coefficients on the drought variables increase in size after controlling for these district-level variables, and in some cases are more precisely estimated. Rather than accounting for the differential effects of drought in TBVC areas, adding in these district variables amplifies the impact of drought in TBVC areas. The coefficients on these interaction terms also make some sense: disability rates are higher among drought cohorts in districts with higher population density (more competition for limited resources), and lower among drought cohorts in districts with initially poor maize suitability (since families were not growing much maize in these areas to begin with). Table 3 is consistent with the argument that estimates of $\delta_1$ and $\delta_2$ are not driven by district-level differences in remoteness, population density or agricultural suitability across TBVC and non-TBVC areas.

Table 4 explores which components of male disability are most sensitive to drought exposure around birth. I present results for each of the component disabilities (vision, physical, hearing/speech and mental disability) and both drought measures in columns (1) to (8) of the table. First, note that all of the TBVC interaction terms are positive indicating that drought exposure around birth and mean drought exposure in early childhood raises disability rates even
more in TBVC areas. The (statistically) strongest results come from vision and physical
disability outcomes. For each of these disabilities, the effect of drought on TBVC cohorts is at
least 25% higher than the effect on non-TBVC cohorts. For physical disability, this difference is
almost double (column (7)).

Importantly, coefficients on the interaction terms in the sight and physical disability regressions
are robust to including district level controls interacted with drought measures (columns 3, 4, 7
and 8). As in Table 3, many of the triple difference coefficients increase in size after controlling
for these baseline district controls, suggesting that differences in population density, agricultural
potential and remoteness across TBVC and non-TBVC areas mitigate the impact of drought in
TBVC areas rather than explain the larger negative impact of drought in these homelands.

Consistent with prior evidence on the impact of childhood exposure to shocks on later life health,
the results of Tables 2, 3 and 4 show that early-life drought exposure has significant negative
impacts on the prevalence of disabilities among African males from former homeland areas. The
negative effects of drought on physical and vision disabilities are concentrated in cohorts
exposed to drought in their birth year and are larger for cohorts that have more cumulative
exposure to drought by age 4. In addition, drought exposure accounts for an even higher fraction
of total disability for African males born into TBVC areas. Nutritional deficiencies associated
with drought during the first few critical years of life appear to be an important source of
developmental impairment in these areas. These differences in health responses to drought across
TBVC and non-TBVC areas imply far-reaching consequences of limits to spatial mobility. The
next section provides direct evidence on the migration mechanism at work in the homelands.

\textit{ii. Evidence on mechanisms: Effect of local drought on adult outmigration}

In Section 1, I argued that the key difference between TBVC and non-TBVC rural homelands
was the intensity of external mobility restrictions facing each group. Table 5 provides empirical
support for this argument. I show results from estimating outmigration equation (2) for the full
sample period in columns (1) to (3), where $TBVC_d$ is fixed over time. Columns (4)-(6) present
results for the outmigration regression where the TBVC indicator only turns on in the years in
which the TBVC homelands are \textit{de jure} independent states (that is, it is time and district
varying). In all regressions, the dependent variable is the percent of adults moving out of a district in a given year.

Outmigration is higher across all homelands in drought years and in years following a drought. The effects are large relative to average outmigration rates and precisely estimated even after controlling for a full set of year and district fixed effects. In column (1), the estimate of $\beta_1$ indicates that drought induces 0.088 percentage points more outmigration. Relative to mean outmigration at the district-year level (0.24%), this is a 36% increase in adults moving out. Outmigration in the year following a drought (in column (2)) is 0.099 percentage points higher. I include indicators for drought one year in the future, to check whether people move in anticipation of a drought event. While the coefficients on these indicators are positive (columns 2 and 4), they are only about half the size of the coefficients on drought this year and drought last year, and not statistically significant at conventional levels.

Differences in outmigration rates in drought years across TBVC and non-TBVC areas are also striking. Drought induces two to four times more adult outmigration from non-TBVC areas, where spatial mobility restrictions were weaker. The estimates of $\beta_2$ in columns (1) and (2) are large and negative although standard errors are also large because of the noisiness of the outmigration variable. The total effect of drought on outmigration from TBVC areas is the sum of the main effect and interaction term. Using estimates from column (1), outmigration is only 0.021% (0.088 - 0.067) higher in TBVC areas in a drought year, or 10% higher relative to the mean. This muted migration response to drought in TBVC areas is also evident in the year following a drought (column (2)).

In columns (4) and (5) of Table 5, I estimate the same outmigration regressions using a more narrow definition of TBVC areas. Here, TBVC takes a value of one in the years during which the relevant homeland is legally independent from South Africa, otherwise zero. This narrow version of TBVC status captures an even starker difference in the external constraints on mobility. The basic patterns of outmigration response to drought persist when switching to this alternative definition and become stronger in the year following a drought. Outmigration from non-TBVC areas is higher by between 0.061% and 0.082% in drought years and in years following a drought. In contrast, outmigration from TBVC areas exposed to drought is significantly lower.
I check whether these outmigration results are robust to including district controls interacted with drought variables. Columns (3) and (6) show that they are. The main effect of drought on outmigration rates is higher (and estimated more precisely) after controlling for historical population density, remoteness and maize suitability interacted with drought variables. And, the triple difference interaction terms are all negative and statistically significant at the 10% level. Outmigration rates in response to drought are between 33% and 50% lower in TBVC areas relative to non-TBVC areas.

Finally, I test the sensitivity of the results to measurement error concerns. Columns (7) and (8) present results for the main migration regressions using the broad TBVC measure and restricting the sample to the later period 1960-1986. Although statistical power is lost due to the smaller sample, the message regarding migration responses to the drought is largely the same. The percent of adults out-migrating from a homeland district is significantly higher during a drought and this effect is almost entirely confined to non-TBVC areas. For example, in column (7), drought raises outmigration from a non-TBVC area by 0.066%, while in TBVC areas the effect is significantly smaller, at only 0.026%. The robustness of the outmigration response to drought in these final two columns of Table 5 reassures us that measurement error bias arising from misclassified migration status (for multiple movers) is unlikely to explain all of our results.

We can combine the results of Table 2 and Table 5 to quantify how much outmigration could help mitigate the disability effects of drought. Since the mean male disability rate in the sample is 5.1%, the triple difference estimate of the impact of drought exposure in the year of birth for TBVC cohorts (0.09 percentage points) takes this disability prevalence up to 6% of the male sample. This translates into 162 additional disabilities for the average district (18,000 males aged 10 to 48 per district). (If we scale the effect to be for one birth cohort only, using the number of children age 0 reported in the Census, then drought exposure at birth in TBVC areas increases male disability prevalence by 118). The triple difference estimates of the impact of drought in a given year on adult outmigration from TBVC districts ranges from -0.05 percent to -0.09 percent. This corresponds with between 21 and 37 fewer outmigrants for the average TBVC district (41,210 individuals over age 15 per district). So, for each potential adult migrant who was unable to migrate away from a TBVC district during a drought, there are between 4.3 and 7.8 additional disabilities among male children. If we add in the constrained migration associated
with drought in the previous year, the number of disabilities per potential migrant falls to between 1.8 and 2.6 per potential migrant.\footnote{If, instead, we use the results reflecting the impact of mean drought exposure up to age 4 on male disability rates (1.2 percentage points), we calculate that drought exposure in a TBVC district generates 198 more disabilities. This translates into between 5.3 and 9.6 more disabilities per potential adult migrant.}

This back-of-the-envelope calculation suggests that restricted spatial mobility had large, but plausible impacts on male disability prevalence. First, the disability results themselves are similar in magnitude to the impact of Ramadan-related nutritional deficiencies estimated for male children from Uganda, Iraq and Michigan (Almond and Mazumdar 2011). Second, migrants from homeland areas often supported large extended families in these areas. Third, outmigration of adults directly frees up household resources for other family members. And, finally, and most importantly, remittance flows represent very large fractions of total household income in these former homeland areas.\footnote{De Beer (1984: 57) describes migrants as “the most privileged people in the reserves”. He estimated that an average worker from the Ciskei, working illegally in a large city for three quarters of the year and spending the remaining months in jail for pass law violations would still increase their standard of living by 700%. Survey data from 1993 indicates that in rural homeland areas of KwaZulu-Natal, migrant remittances represented between 25 and 35% of total monthly household income.} The next section provides evidence from more recent times that remittances from homeland labor migrants do respond to drought, thereby linking outmigration and the welfare of households left behind during drought.

\textit{iii. Evidence on mechanisms: Effect of drought on remittances}

Table 6 shows that drought induces remittance flows towards households in rural ex-homeland areas and that migrant workers facilitate these flows. The table presents results from household-level regressions of remittance receipts (“Any remittances”) in 1995 on district-level drought in 1995, an indicator for whether a migrant worker is attached to the household, and the interaction of these two variables. Because households with and without migrant workers could differ on a number of unobservables, it is important to control for the presence of a migrant worker attached to the household. I also control directly for the fraction of drought years experienced by each district between 1948 and 1995 to account for differences in remittance behavior induced by the “drought-proneness” of an area. Columns (1) and (2) present results for the entire sample, while columns (3) to (6) break this out for non-TBVC and TBVC districts.
Even though this evidence on remittances comes from the post-*apartheid* period when legal barriers to labor migration are no longer in place, the legacy of historical mobility restrictions persists. In 1996, just over 30% of all households in rural TBVC and non-TBVC districts reported at least one migrant worker (this fraction is somewhat higher in non-TBVC households) and one in five received some type of remittance the year before the Census. Conditional on having any migrant worker attached to the household, the chances of a household receiving any remittance were one in two. About 7% of households without migrant workers also received some remittances (results not shown). The estimates in Table 6 show that drought exposure in 1995 has no significant impact on remittance receipts in households without a migrant worker. In contrast, migrant worker households in drought-exposed districts were a significant 2.7 percentage points more likely to receive any remittances in 1995 (column (1)), and the result is robust to including baseline district controls interacted with historical drought prevalence. Drought raises the probability of receiving any remittances by 13% relative to the mean rate of remittances.

Splitting the sample into TBVC and non-TBVC areas, it is clear that having a migrant worker in the household facilitates remittances after drought in both areas. This relationship is more precisely estimate and somewhat larger in non-TBVC areas, perhaps reflecting the stronger historical migrant links between these areas and the rest of the economy (although the sample size is also larger for the non-TBVC areas). For non-TBVC areas, drought exposure raises the chances of receiving remittances by over 16%.

Combined with the prior results on outmigration, Table 6 supports the notion that remittances link outmigrants with their rural families in the former homelands and shows that these remittance flows respond positively to local economic shocks. Having a migrant worker attached to a household can help families to protect incomes against the negative effects of local drought.

*iv. Addressing population composition effects*

As a final concern about whether the disability results are driven by differences in spatial mobility across TBVC and non-TBVC homelands, I examine whether any of the differential effects of drought on male disability in TBVC areas are driven by drought-induced changes in
cohort composition. Changes in total fertility, child mortality or sex selection in response to drought may contribute to or even account for all of these disability results.

If fewer children are born during drought due to higher fetal death rates, lower conception rates or planned pregnancy timing, survivors may be stronger (positive “selection” effects dominate “scarring” effects as in Bozzoli, Deaton and Quintana-Domeque 2009). Positive survivor bias would mean that results in Tables 2, 3 and 4 underestimate the impact of drought on disability in all of the homelands. Going further: if positive selection among males was stronger in TBVC areas, our main regression would underestimate the impact of drought in TBVC areas even more. On the other hand, positive selection that is stronger in non-TBVC areas could pose a problem for our interpretation of mechanisms, since differential survivor bias could feasibly explain the male disability differentials across TBVC and non-TBVC areas.

To check for differential drought impacts on total cohort size and cohort sex composition, I estimate regressions similar to (1) for log total population, log total males and log total females born in a given district in a given year between 1948 and 1986. I also estimate regressions for total completed fertility among women ages 40 to 60 in TBVC and non-TBVC areas.

Table 7 shows that the effects of drought on disability rates are unlikely to be driven by cohort composition effects. Cohorts exposed to the mean rate of drought up to age 4 are smaller in all areas (not significant), and in TBVC areas, these cohorts are significantly smaller – on the order of 2 percent. Results are similar, although much more noisily estimated, using drought exposure in the cohort’s birth year. Note that the TBVC interaction terms are larger for men than for women. Taken together these results suggest that positive survivor bias probably characterizes the main disability results in both TBVC and non-TBVC areas, with larger effects in TBVC areas. With significantly fewer male survivors in TBVC areas, disability results are likely underestimated in the triple difference regressions.

40 There is also a direct effect of drought on population composition through outmigration. However, since I use year of birth and district of birth to assign drought exposure measures to individuals, and since children were not able to access permits for relocation, disability results cannot be driven by migration-related composition changes.
As a final check for composition effects, I estimate fertility regressions to test whether TBVC “survivors” are negatively selected as a result of more educated women postponing fertility at a higher rate in TBVC areas. The sample is restricted to women whose first district is a rural homeland area and I assume each woman bears her children in the same district reported as her first district.\textsuperscript{41} I regress total fertility on the fraction of a woman’s childbearing years (ages 15-40) during which she experienced drought and the interaction with of this exposure measure with the TBVC status of the district. I control for first district fixed effects and birth year to capture mother age effects. Although total fertility is not the best outcome variable to get at the immediate impacts of drought, it still tells us something about whether overall fertility is higher or lower among women with greater total drought exposure, and differentially so in TBVC relative to non-TBVC areas.

Table 8 presents results for the full sample of women (column (1)) and for subsamples of women with different levels of education. Total fertility rates are significantly lower (0.22 fewer births, or about 4.7% relative to the mean) when women are exposed to the mean years of drought during their childbearing years – however, this effect is only present for women in TBVC areas. This negative effect for total fertility is consistent with negative cohort size effects in drought years in TBVC areas (Table 7). Breaking up the sample of mothers by level of education, we see that mothers without education drive these negative fertility effects. Cohort composition changes driven by the most educated mothers reducing fertility cannot account for our disability results. This lends further support to the idea that TBVC babies born during drought events are on average positively selected and that any disability effects that show up for surviving cohorts are likely underestimates of the total health effects of drought.

5. Conclusions

This paper presents new evidence on the long-term consequences of spatial barriers to mobility in a developing country where incomes are tied to variations in local weather conditions. Using externally imposed differences in migration barriers stemming from South Africa’s \textit{apartheid}

\textsuperscript{41} This assumption generates a noisy assignment of first district for the (likely small) fraction of women who moved multiple times during \textit{apartheid}.
policies, I estimate that early childhood drought exposure raises disability rates by about 17% for African males born into areas facing the greatest barriers to migration. This is over and above the average impact of drought on disability for exposed cohorts across all areas, and is not driven by cohort composition effects. I also use a new method to construct migration histories using a cross-section of Census data. Using these constructed migration histories I estimate that adult outmigration from less mobility-restricted areas is over 30% higher during a drought year but only 10% higher in areas facing tougher restrictions. I document the link between outflows of adults and inflows of money by showing recent evidence that remittance receipts respond to drought in households with migrant workers.

While the policies of apartheid are unique to South Africa, different forms of internal mobility restrictions occur throughout the world. Some barriers are institutional, as in China and the former Soviet Union. Others, particularly in Africa, are geographic in nature or related to inadequate transportation infrastructure. The development literature has noted the direct and immediate effects of limited spatial mobility on income volatility and has viewed spatial mobility as a form of insurance against economic shocks. My results identify a specific implication of migration barriers for health human capital accumulation over the long run. In environments prone to frequent local environmental shocks like drought, enhancing spatial mobility could generate large welfare gains through the health-protective effects of labor migration and remittance behaviors.

Highlighting how spatial mobility can act as insurance also provides new insights into the long-term consequences of economic shocks experienced in early life. There is a wealth of credible empirical evidence that negative shocks to the nutritional and disease environment in early life have severe short-run effects on child health and significant long-term effects on health in adulthood. Much less is known about how families can mitigate the impact of these negative shocks on the health of their children (Almond and Currie 2011a, Almond and Currie 2011b). These South African results suggest that where they are able to, families use labor mobility over space to weather the effects of drought, with corresponding long-term gains in health human capital accumulation.
References


Bryan, Gharad, Shyamal Chowdhury and A. Mushfiq Mobarak, 2012 “Risk aversion and seasonal migration”, Mimeo, Yale University


Casale, Daniela and Dorrit Posel. 2006 “Migration and remittances in South Africa”, Mimeo, University of KwaZulu-Natal


Currie, Janet and Tom Vogl. 2013 “Early-Life Health and Adult Circumstance in Developing Countries”, Annual Review of Economics, Volume 5


Simkins, Charles. 1983. Four essays on the past, present & possible future of the distribution of the black population of South Africa, South African Labour and Development Research Unit, University of Cape Town


Source: Political map of South Africa 1986, Perry-Castañeda Library Map Collection, http://www.lib.utexas.edu/maps/south_africa.html, accessed July 2011. Homelands are (in order of establishment dates): TBVC areas: Transkei (1), Boputhatswana (2), Venda (3), Ciskei (4) and non-TBVC areas: Lebowa (5), KwaZulu (6), Qwaqwa (7), Gazankulu (8), Kangwane (9), KwaNdebele (10)
Figure 2 shows the fraction of South African homeland districts experiencing a drought annually between 1948 and 1986. The left hand panel shows drought in non-TBVC (less restricted) areas, the right hand panel for TBVC (more restricted) areas. The drought indicator is based on the Standardized Precipitation Index (SPI) as described in Data Appendix 1.

Figure 3 presents lowess-smoothed outmigration rates from TBVC and non-TBVC areas over time using Census data from 1948-1986. The y axis shows the average percent of adults who outmigrate from a district in a given year, across all districts. Kernel is Epanechnikov, bandwidth is 0.3.
<table>
<thead>
<tr>
<th></th>
<th>Full sample means</th>
<th>TBVC sample means</th>
<th>Non-TBVC sample means</th>
<th>p-value of difference</th>
<th>N</th>
<th>Min.</th>
<th>Max.</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Individual-level data</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>1. Cohorts born 1948-1986</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Drought in year of birth</td>
<td>0.067</td>
<td>0.073</td>
<td>0.062</td>
<td>0.720</td>
<td>655,532</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Fraction of infancy (in utero to age 4) in drought</td>
<td>0.074</td>
<td>0.081</td>
<td>0.069</td>
<td>0.214</td>
<td>655,532</td>
<td>0</td>
<td>0.4</td>
</tr>
<tr>
<td>Age</td>
<td>23.742</td>
<td>23.775</td>
<td>23.714</td>
<td>0.285</td>
<td>655,532</td>
<td>10</td>
<td>48</td>
</tr>
<tr>
<td>Female</td>
<td>0.545</td>
<td>0.543</td>
<td>0.546</td>
<td>0.006</td>
<td>655,532</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Years of education</td>
<td>6.356</td>
<td>6.426</td>
<td>6.295</td>
<td>0.202</td>
<td>641,575</td>
<td>0</td>
<td>15</td>
</tr>
<tr>
<td>Any serious disability</td>
<td>0.052</td>
<td>0.054</td>
<td>0.051</td>
<td>0.012</td>
<td>655,532</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Number of serious disabilities</td>
<td>0.057</td>
<td>0.059</td>
<td>0.054</td>
<td>0.000</td>
<td>655,532</td>
<td>0</td>
<td>4</td>
</tr>
<tr>
<td>Sight disability</td>
<td>0.023</td>
<td>0.024</td>
<td>0.022</td>
<td>0.003</td>
<td>655,532</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Speech/hearing disability</td>
<td>0.011</td>
<td>0.012</td>
<td>0.011</td>
<td>0.006</td>
<td>655,532</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Physical disability</td>
<td>0.014</td>
<td>0.015</td>
<td>0.014</td>
<td>0.148</td>
<td>655,532</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Mental disability</td>
<td>0.007</td>
<td>0.008</td>
<td>0.007</td>
<td>0.000</td>
<td>655,532</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td><strong>2. Female cohorts age 40-60 in 1996</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of children ever born</td>
<td>4.793</td>
<td>4.909</td>
<td>4.688</td>
<td>0.000</td>
<td>79,532</td>
<td>0</td>
<td>23</td>
</tr>
<tr>
<td>Fraction of child-bearing years in drought</td>
<td>0.071</td>
<td>0.076</td>
<td>0.067</td>
<td>0.023</td>
<td>79,532</td>
<td>0</td>
<td>0.4</td>
</tr>
<tr>
<td><strong>Panel B: District-year level data 1948-1986</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Percent of adults who migrate out</td>
<td>0.240</td>
<td>0.215</td>
<td>0.255</td>
<td>0.00</td>
<td>624</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Fraction of districts experiencing drought</td>
<td>0.051</td>
<td>0.068</td>
<td>0.041</td>
<td>0.19</td>
<td>624</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td><strong>Panel C: District level data</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Population density in 1946 (people per km(^2))</td>
<td>48.237</td>
<td>38.207</td>
<td>54.255</td>
<td>0.43</td>
<td>16</td>
<td>15</td>
<td>137</td>
</tr>
<tr>
<td>Distance to nearest large city (kms)</td>
<td>33.247</td>
<td>36.466</td>
<td>31.316</td>
<td>0.62</td>
<td>16</td>
<td>7</td>
<td>77</td>
</tr>
<tr>
<td>Median value of maize suitability index</td>
<td>6.563</td>
<td>7.000</td>
<td>6.300</td>
<td>0.17</td>
<td>16</td>
<td>5</td>
<td>9</td>
</tr>
<tr>
<td>Number of districts</td>
<td>16</td>
<td>6</td>
<td>10</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of birth years</td>
<td>39</td>
<td>39</td>
<td>39</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Individual and district-level means for African respondents in the 1996 Census, 10% sample. TBVC stands for Transkei, Boputhatswana, Venda and Ciskei, the four earliest homeland states. Non-TBVC indicates observations from districts in the remaining six homeland areas. Individual-level data includes people born 1948-1986 (age 10-48 in 1996) who are currently or previously living in any districts formerly part of a homeland. Fertility data is reported for females who have completed childbearing in 1996, i.e. cohorts born 1936-1956. District-year level data is restricted to individuals who are adults during 1948-1986. District controls include: population density measured in the 1946 national Census, distance to the nearest large city calculated using 1996 Census maps, and the median value of the maize suitability index calculated from FAO data. p-values are from regressions of each variable on a TBVC indicator, clustered on year of birth (Panel A), on year (Panel B), or not at all (Panel C).
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Any disability</td>
<td>Num. disabilities</td>
<td>Any disability</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Fraction early childhood in drought</td>
<td>0.0192**</td>
<td>0.0207**</td>
<td>0.0235**</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.008)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>Fraction early childhood in drought*TBVC</td>
<td>0.0106*</td>
<td>0.0141**</td>
<td>0.0212***</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.006)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Drought in birth year</td>
<td>0.001</td>
<td>0.002</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Drought in birth year*TBVC</td>
<td>0.004</td>
<td>0.005</td>
<td>0.0099**</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.003)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Birth year, district FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
</tbody>
</table>

**p-values for F-tests**

<table>
<thead>
<tr>
<th></th>
<th>Male sample [N=298,475]</th>
<th>Female sample [N=357,057]</th>
</tr>
</thead>
<tbody>
<tr>
<td>All drought parameters jointly =0</td>
<td>0.00</td>
<td>0.02</td>
</tr>
<tr>
<td>Sum of Drought and Drought*TBVC=0</td>
<td>0.00</td>
<td>0.01</td>
</tr>
<tr>
<td>Mean of outcome</td>
<td>0.052</td>
<td>0.052</td>
</tr>
</tbody>
</table>

Robust standard errors clustered on the year of birth. Levels of significance: p<0.01***, p<0.05**, p<0.1*. Drought exposure is a binary variable constructed using values of the Spatial Precipitation Index; fraction of infancy in drought is the fraction of years from in utero period to age 4 that the drought indicator equals one; TBVC indicates whether an individual reports a prior district is TBVC or not. Sample restricted to 1996 Census data on individuals born between 1948 and 1986. Coefficient on fraction early childhood in drought evaluated at the mean of this variable.
Table 3: Effects of drought exposure in early childhood on prevalence and number of male disabilities later in life: Robustness checks

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Any disability</th>
<th>Num. Disabilities</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Fraction early childhood in drought</td>
<td>0.0235**</td>
<td>0.052**</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.023)</td>
</tr>
<tr>
<td>Fraction early childhood in drought*TBVC</td>
<td>0.0212***</td>
<td>0.039***</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>Drought in birth year</td>
<td>-0.001</td>
<td>-0.008</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>Drought in birth year*TBVC</td>
<td>0.0099**</td>
<td>0.009**</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>District controls interacted with drought measures</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fraction early childhood in drought*District Pop. Density 1946</td>
<td>0.001***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td></td>
</tr>
<tr>
<td>Fraction early childhood in drought*Kms to nearest City</td>
<td>0.001</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Fraction early childhood in drought*Maize suitability index of district</td>
<td>-0.012***</td>
<td>-0.016***</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Drought in birth year*District Pop. Density 1946</td>
<td>0.000**</td>
<td>0.000**</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Drought in birth year*Kms to nearest City</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Drought in birth year*Maize suitability index of district</td>
<td>0.001</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.002)</td>
</tr>
</tbody>
</table>

**Birth district, year fixed effects**

<table>
<thead>
<tr>
<th></th>
<th>Y</th>
<th>Y</th>
<th>Y</th>
<th>Y</th>
<th>Y</th>
<th>Y</th>
<th>Y</th>
<th>Y</th>
<th>Y</th>
</tr>
</thead>
</table>

<table>
<thead>
<tr>
<th>p-values for F-tests</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>All drought parameters jointly =0</td>
<td>0.02</td>
</tr>
<tr>
<td>Sum of Drought and Drought*TBVC=0</td>
<td>0.01</td>
</tr>
<tr>
<td>All district interactions with Drought=0</td>
<td>0.00</td>
</tr>
</tbody>
</table>

Mean of outcome | 0.051 | 0.051 | 0.051 | 0.051 | 0.055 | 0.055 | 0.055 | 0.055 | 0.055 |

Robust standard errors clustered on the year of birth. Levels of significance: p<0.01***, p<0.05**, p<0.1*. Drought exposure is a binary variable constructed using values of the Spatial Precipitation Index; fraction of infancy in drought is the fraction of years from in utero period to age 4 that the drought indicator equals one; TBVC indicates whether an individual reports their prior district is TBVC or not. District variables (population density in 1946, distance to the nearest city in kilometers, and the maize suitability index where higher numbers indicate lower suitability) are each interacted with the relevant drought measure. Sample restricted to 1996 Census data on males born between 1948 and 1986 (N=298,475). Coefficient on fraction early childhood in drought evaluated at the mean of this variable.
<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Sight disability [Mean=0.02]</th>
<th>Physical disability [Mean=0.01]</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Fraction early childhood in drought</td>
<td>0.0083*</td>
<td>0.021</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>Fraction early childhood in drought*TBVC</td>
<td>0.002</td>
<td>0.007</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Drought in birth year</td>
<td>0.001</td>
<td>0.006</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Drought in birth year*TBVC</td>
<td>0.0042*</td>
<td>0.005**</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td><strong>p-values for F-tests</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>All drought parameters jointly =0</td>
<td>0.05</td>
<td>0.04</td>
</tr>
<tr>
<td>Sum of Drought and Drought*TBVC=0</td>
<td>0.02</td>
<td>0.01</td>
</tr>
</tbody>
</table>

**Birth year, district FE**

Y Y Y Y Y Y Y Y

**District variables*Drought controls**

N N Y Y N N Y Y

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Hearing/speech disability [Mean=0.01]</th>
<th>Mental disability [Mean=0.01]</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Fraction early childhood in drought</td>
<td>0.006</td>
<td>0.006</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>Fraction early childhood in drought*TBVC</td>
<td>0.003</td>
<td>0.006</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Drought in birth year</td>
<td>0.000</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Drought in birth year*TBVC</td>
<td>0.001</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.002)</td>
</tr>
<tr>
<td><strong>p-values for F-tests</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>All drought parameters jointly =0</td>
<td>0.02</td>
<td>0.56</td>
</tr>
<tr>
<td>Sum of Drought and Drought*TBVC=0</td>
<td>0.01</td>
<td>0.29</td>
</tr>
</tbody>
</table>

**Birth year, district FE**

Y Y Y Y Y Y Y Y

**District variables*Drought controls**

N N N Y Y N N Y Y

Robust standard errors clustered on the year of birth. Levels of significance: **p<0.01***, **p<0.05**, *p<0.1*. Drought exposure is a binary variable constructed using values of the Spatial Precipitation Index; fraction of infancy in drought is the fraction of years from in utero period to age 4 that the drought indicator equals one; TBVC indicates whether an individual reports a prior district is TBVC or not. District*Drought controls are; drought measure*population density in 1946, drought measure*distance to nearest city, and drought measure*median value on maize suitability index. Sample restricted to 1996 Census data on males born between 1948 and 1986 (N=298,475). Coefficient on fraction early childhood in drought evaluated at the mean of this variable.
### Table 5: Mechanisms: Effect of drought on percent of adults who outmigrate from TBVC and non-TBVC homelands

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Drought year</td>
<td>0.0881*</td>
<td>0.0775*</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.045)</td>
</tr>
<tr>
<td>Drought year*TBVC</td>
<td>-0.067</td>
<td>-0.061</td>
</tr>
<tr>
<td></td>
<td>(0.048)</td>
<td>(0.049)</td>
</tr>
<tr>
<td>Drought last year</td>
<td>0.0992***</td>
<td>0.3848**</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
<td>(0.150)</td>
</tr>
<tr>
<td>Drought last year*TBVC</td>
<td>-0.067</td>
<td>-0.023</td>
</tr>
<tr>
<td></td>
<td>(0.055)</td>
<td>(0.057)</td>
</tr>
<tr>
<td>Drought next year</td>
<td>0.047</td>
<td>0.234</td>
</tr>
<tr>
<td></td>
<td>(0.040)</td>
<td>(0.233)</td>
</tr>
<tr>
<td>Drought next year*TBVC</td>
<td>-0.007</td>
<td>-0.040</td>
</tr>
<tr>
<td></td>
<td>(0.054)</td>
<td>(0.063)</td>
</tr>
<tr>
<td>Birth year, district FE</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>District variables*drought controls</td>
<td>N</td>
<td>N</td>
</tr>
</tbody>
</table>

**P-values for F-tests**

| All drought parameters=0   | 0.11                    | 0.05                    | 0.04                    | 0.14                    | 0.03                    | 0.40                    | 0.24                    | 0.27                    |

N observations: 624 624 624 624 624 624 432 432

Robust standard errors clustered on year; p<0.01***, p<0.05**, p<0.1*. Sample restricted to 1996 Census data on African men and women who report their prior district was in a former homeland area and who are 18 or older in 1996. Unit of observation is the district-year. Outcome is the percent of adults who move away from a prior district in a given year. Drought is an indicator for whether there was a drought in the district in a given (or prior or following) year.

1 Broad TBVC measure is an indicator for whether the prior district falls into one of the independent homeland areas or not and is constant through 1948-1986.

2 Narrow TBVC measure is the broad TBVC measure refined to turn on only during the years in which TBVC states were independent.
Table 6: Effect of drought in 1995 on remittances to former homelands in 1995

<table>
<thead>
<tr>
<th></th>
<th>All households [N=222,355]</th>
<th>Households in non-TBVC areas [N=115,126]</th>
<th>Households in TBVC areas [N=107,229]</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td></td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>Drought last year</td>
<td>0.002</td>
<td>0.026</td>
<td>0.009</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.016)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Any migrant worker in HH?</td>
<td>0.413***</td>
<td>0.406***</td>
<td>0.391***</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.023)</td>
<td>(0.024)</td>
</tr>
<tr>
<td>Drought last year*Any migrant worker in HH?</td>
<td>0.027**</td>
<td>0.028***</td>
<td>0.033**</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>Fraction of 1948-1996 period in drought</td>
<td>0.036**</td>
<td>0.048***</td>
<td>0.029</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.013)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>District variables*Drought last year controls</td>
<td>N</td>
<td>Y</td>
<td>N</td>
</tr>
<tr>
<td>p-value for F-test</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sum of Drought and Drought*Migrant=0</td>
<td>0.00</td>
<td>0.01</td>
<td>0.00</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>0.41</td>
</tr>
</tbody>
</table>

Robust standard errors clustered at the district level. p<0.01***, p<0.05**, p<0.1*. Sample restricted to 1996 Census data on households residing in one of the TBVC or non-TBVC districts in 1996. Unit of observation is the household. Fraction of sample with a migrant worker attached to household in 1996 is 0.318 in non-TBVC areas and 0.314 in TBVC areas. Coefficient on fraction of 1948-1986 period in drought is multiplied by sample mean time in drought (0.067). District control variables described in Table 3 notes.
<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>Ln population</th>
<th>Ln males</th>
<th>Ln females</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Fraction early childhood in drought</td>
<td>-0.024</td>
<td>-0.063</td>
<td>-0.036</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.053)</td>
<td>(0.042)</td>
</tr>
<tr>
<td>Fraction early childhood in drought*TBVC</td>
<td>-0.02***</td>
<td>-0.046***</td>
<td>-0.036***</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.017)</td>
<td>(0.013)</td>
</tr>
<tr>
<td>Drought in birth year</td>
<td>-0.009</td>
<td>-0.070</td>
<td>0.027</td>
</tr>
<tr>
<td></td>
<td>(0.159)</td>
<td>(0.360)</td>
<td>(0.290)</td>
</tr>
<tr>
<td>Drought in birth year*TBVC</td>
<td>-0.048</td>
<td>-0.129</td>
<td>-0.075</td>
</tr>
<tr>
<td></td>
<td>(0.062)</td>
<td>(0.142)</td>
<td>(0.113)</td>
</tr>
<tr>
<td>Birth year, district FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Distritv variables*Drought controls</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
</tbody>
</table>

**p-values for F-tests**

| All Drought, Drought*TBVC parameters jointly=0         | 0.02          | 0.45     | 0.02       | 0.37         | 0.02         | 0.53         |

| N                                                      | 624           | 624      | 624        | 624          | 624          | 624          |

Robust standard errors clustered on the year of birth. p<0.01***, p<0.05**, p<0.1*. District controls described in Table 3 notes. Regressions are estimated using observations aggregated to the year of birth-first district level. Sample is restricted to cohorts born 1948-1986 (ages 10-48) in 1996. Coefficients in 1, 3 and 5 are weighted by average fraction of early childhood years in drought (0.06).
### Table 8: Effects of drought exposure during childbearing years on fertility outcomes among African women who have completed childbearing by 1996

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>Number of children ever born</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All education levels</td>
</tr>
<tr>
<td>Fraction years in drought (ages 15-40)</td>
<td>0.001 (0.061)</td>
</tr>
<tr>
<td>Fraction years in drought (ages 15-40)*TBVC</td>
<td>-0.228*** (0.084)</td>
</tr>
<tr>
<td>Birth year, district FE</td>
<td>Y</td>
</tr>
</tbody>
</table>

| N                  | 70,836 | 33,312 | 15,852 | 19,952 |
| Mean of outcome    | 4.79   | 4.96   | 5.11   | 4.35   |

Robust standard errors clustered on the year of mother's birth, p<0.01***, p<0.05**, p<0.1*. Sample restricted to women who have completed childbearing in 1996 (ages 40-60). The fraction of years exposed to drought during childbearing years is constructed using drought prevalence in the first district during the years in which the woman is between age 15 and 40 inclusive (1951-1996). Sample is restricted to women who have completed childbearing in the 1996 Census and who have at least one birth. Coefficients are evaluated for the mean fraction of years in drought: 0.071.
Appendix 1: Data

1. Rainfall Data and Drought

The South African Weather Service [http://www.weathersa.co.za/web/](http://www.weathersa.co.za/web/) provided the raw historical rainfall data. These data contain monthly rainfall measures at the weather station level for over 1,600 weather stations across South Africa from 1920 to 2009. I spatially match the GIS locations of rainfall stations to corresponding district boundaries and aggregate rainfall totals to the district-year level.

To create a measure of drought, I construct the Standardized Precipitation Index (SPI) at the district and year level (McKee, Doesken and Kleist 1993). The SPI measures the probability of observing a recent rainfall event based on the distribution of all rainfall events for a given time scale and place. Since rainfall is not normally distributed, the SPI procedure calls for a gamma distribution to be fit to the empirical data distributions. I fit a gamma distribution to the annual total rainfall of each district and generate estimates of the scale and location parameters for district-specific rainfall patterns. For each year in the data, and the district-specific gamma distribution, I compute the probability of observing the total rainfall that was measured in each year and translate this into a normally distributed random variable using the normal CDF. This number is the district-year-specific SPI, where positive numbers reflect above-average rainfall and negative values reflect below-average rainfall. The positive relationship between log rainfall and the SPI measure across all districts is shown in Appendix Figure 1.

Following the climatological literature (e.g. McKee et al 1993) I define an indicator $DROUGHT_{dt}$ for each district ($d$) and year ($t$) that takes a value of 1 when the SPI is less than -1.5, and 0 otherwise. The spatial specificity of this measure is helpful because the same quantitative rainfall deficit may indicate inadequate precipitation in historically wetter districts but not in historically drier districts. Appendix Figure 2 shows lowess-smoothed graphs of the district level mean SPI values across TBVC and non-TBVC areas for the years 1948 to 1986.
There is a tight link between the SPI measure and South African maize production. Using province-level data from the South African Maize Board for the period 1964 to 1984 and for the commercial maize-growing provinces (Transvaal and the Orange Free State), I estimate the relationship between the SPI measure and maize yields. Appendix Figure 3 shows the lowess-smoothed relationship between the log of South Africa’s annual maize output (in tons) against the Spatial Precipitation Index using an Epanechnikov kernel with a 0.5 bandwidth. This positive relationship is asymmetric. Output appears more sensitive to low values of the SPI than it is to
higher, positive values of the SPI. Figure 3 suggests that drought in particular captures an important negative productivity shock in agriculture.

Appendix Figure 3

2. Homeland Boundary Data and the TBVC assignment

GIS data on sub-national boundaries for the 1996 and 2001 Census were obtained from Statistics South Africa (www.statssa.gov.za). I use the 2001 district council Census boundaries as the main geographic unit of observation since these areas are large enough to treat as distinct local labor markets and contain sufficient population in each year to make aggregation feasible.¹

To define which of these districts belong to former homeland areas, I obtained online maps of the ten homelands with the predominant map dated 1986 (see Figure 1 in the main text). I overlaid these homeland maps onto Census boundaries and, where there was overlap, assigned districts to homelands. I created an indicator TBVCₜ that takes a value of 1 if a district overlapped with any of the TBVC homelands, and is 0 for those districts overlapping the remaining six homelands. ² Of the 53 district councils in South Africa, 16 of them (30%) fall

¹ Magisterial districts are too small to contain sufficient population and rainfall measurements for my analysis.
² TBVC stands for Transkei, Bophuthatswana, Venda and the Ciskei.
into prior rural homeland areas. Of these 16 areas, six fall in the former TBVC areas and the remaining 10 fall in the non-TBVC areas.

3. 1996 Census Data

The 10% sample of individual records from the 1996 South African Census was obtained from Statistics South Africa (www.statssa.gov.za).

i. Migration variables

This Census asked all individuals about their current district of residence, their former district of residence and the year in which they moved to their current district. I use this information to define several variables relevant to migration:

- The district of current residence
- The district of former residence: this is the same as district of current residence for individuals who report never having moved, for individuals who have moved since the end of apartheid (1994-1996), for individuals who reported moving during childhood to their current residence, and for those who report moving within a district
- An indicator for whether a person moved before 1994 (the end of apartheid) and during their adult lives. This indicator is 1 if a person’s former residence differs from their current residence and if they report the year they moved to their current residence.

For the analysis in the paper, I eliminate individuals who report a current residence (for never movers) or a prior residence (for movers) in a district outside of any of the homelands. I also eliminate those who report living in (for never movers) or formerly living in (for movers) districts outside of South Africa. Less than 1% of the sample has a usual residence outside South Africa and less than 5% have a prior residence outside of South Africa. Of the remaining sample of adults who report a former residence (for movers) or current residence (for never movers) located in rural South Africa, 97% have complete information on current and former district of residence and the year of moving to current residence. For the 3% who report a current residence and no information about year of moving, I assign them to be non-movers.
For the migration analysis, I further restrict the sample to African adults aged 18 and older in 1996 who report a current (for never movers) or prior residence (for movers) in South Africa that is predominantly rural and located in one of the former homeland areas.

For each year in which a respondent is 18 or older, I identify what district they lived in under the strong assumption that each person moves only once. That is, I create a pseudo-panel dataset describing the place of residence by year of adulthood. I match this panel to drought at the district-year level. Finally, I collapse the resulting individual-year-districts dataset to district-year level for the migration analysis.

**ii. Health, fertility and population outcomes**

For the disability analysis, and for the analysis of cohort size and sex composition, I use the sample of African adults who lived in any of the former homeland areas between 1948 and 1986. I match the cross-sectional data on outcomes at the individual level to the drought data on year of birth and prior district.

Note that the Census does not capture place of birth information, so I assume that a person’s prior residence is their birth district. This means that birth district is potentially misclassified for people who move multiple times. Appendix 2 discusses the implications of this measurement error.

For the analysis of fertility and child mortality outcomes, I restrict the sample to African women aged 40 to 60 in 1996 and create a variable that represents the fraction of their childbearing years (ages 15-40) that they experienced drought. I assign drought exposure at the district level using the prior district reported by these women.

**4. District-level control variables**

To control for key district level variables that could contribute to differential responses of disability rates to drought events, I create:

- District-level population density recorded in the 1946 South African Census. These data were digitized from hardcopies of Census aggregate reports and matched to later district boundaries.
The straight-line distance (in kilometers) from the midpoint of each district polygon to the nearest large city. Distances were calculated using 1996 Census maps in ArcGIS version 10.

A district’s median score on the FAO’s maize suitability index. This index captures how suitability land is for maize production at a fine grid level. I use these values to create the median value of the suitability index across all points in given district in ArcGIS. Low values of this number represent greater suitability of the soil for maize production.

References

Appendix 2: Measurement error in Census migration data

The nature of “last move” Census data induces measurement error in migration and, potentially, in drought exposure. This note characterizes the resulting measurement error bias that arises when modeling how outmigration responds to drought exposure. Intuitively, the bias is related to the size of the population that actually moves more than once, to the fraction of observed drought events, and to the fraction of multiple movers who have misclassified drought exposure due to invisible prior migrations.

To fix ideas, note the Census contains three types of individuals: people who have never moved from their district of residence (“never movers”), people who have only ever moved districts once (“single movers”) and those who have moved multiple times (“multiple movers”). Complete migration histories are known for the never movers (who are identifiable in the data) and single movers. Misclassification only occurs for multiple movers, in moves before their last. For these multiple movers, earlier moves are made invisible by later moves (Schmertmann 1999). In addition, since a drought exposure variable for each year of life is assigned to an individual based on their district of residence in each year, misclassified migration could induce misclassification in drought exposure for that person. This has implications for regressions of migration on drought.

To illustrate, consider the following difference-in-differences model:

\[(1) \ y_{jdt}^* = \alpha + \beta D_{jdt}^* + \lambda_d + \delta_t + \epsilon_{jdt} \]

where \(y_{jdt}^*\) indicates whether a person \(j\) moves away from district \(d\) in year \(t\) \((y_{jdt}^* = 1)\) or not \((y_{jdt}^* = 0)\), \(D_{jdt}^*\) indicates whether a person was exposed to drought in district \(d\) in year \(t\) \((D_{jdt}^* = 1)\) or not \((D_{jdt}^* = 0)\), \(\epsilon_{jdt}\) is an error term and \(\lambda_d\) and \(\delta_t\) are district and year fixed effects respectively. Throughout this appendix, starred outcome and independent variables denote true values of these variables; unstarred variables denote observed outcomes.

A person’s observed migration status in each year \((y_{jdt})\) can be related to their true migration status \((y_{jdt}^*)\) as follows:
(2) \( y_{jdt} = y_{jdt}^* + v_{jdt} \)
   i) \( v_{jdt} = 0 \) if \( y_{jdt} = y_{jdt}^* = 0 \) or \( 1 \)
   ii) \( v_{jdt} = -1 \) if \( y_{jdt} = 0, y_{jdt}^* = 1 \)

Condition (2i) describes never movers, single movers and multiple movers who are on their last move. For these cases, there is no measurement error in migration status, so \( v_{jdt} \) is always zero.
Condition (2ii) describes the case of a misclassified non-move for multiple movers, when a real move is unseen because it was prior to the last move. Since every reported move is a true move, \( v_{jdt} = 1 \) (\( y_{jdt} = 1, y_{jdt}^* = 0 \)) is ruled out.

Observed drought exposure for each person in each district of each year (\( D_{jdt} \)) is related to true drought exposure (\( D_{jdt}^* \)) in a similar way:

(3) \( D_{jdt} = D_{jdt}^* + w_{jdt} \)
   i) \( w_{jdt} = 0 \) if \( D_{jdt} = D_{jdt}^* = 0 \) or \( 1 \)
   ii) \( w_{jdt} = 1 \) if \( D_{jdt} = 1, D_{jdt}^* = 0 \)
   iii) \( w_{jdt} = -1 \) if \( D_{jdt} = 0, D_{jdt}^* = 1 \)

Condition (3i) describes never movers, single movers and multiple movers on their last move who have no measurement error in drought exposure. Condition (3ii) describes the misclassification of non-drought exposure for a multiple mover whose prior move is unobserved (i.e. if \( v_{jdt} = -1 \)). Condition (3iii) describes misclassification of drought exposure for a multiple mover whose prior move is unobserved (i.e. if \( v_{jdt} = -1 \)).

Using (2) and (3) to substitute out true unobserved values of drought and migration in (1), we can estimate the following using the Census pseudo-panel data on last moves:

(4) \( y_{jdt} = \alpha + \beta D_{jdt} + \lambda_d + \delta_t - \beta w_{jdt} + v_{jdt} + \epsilon_{jdt} \)

If we assume \( \text{cov}(D_{jdt}, \lambda_d + \delta_t + \epsilon_{jdt})=0 \) (essentially, observed drought is randomly assigned to districts and years) we can make progress describing the measurement error bias in \( \beta_{OLS} \):

\[
\text{plim } \beta_{OLS} = \frac{\text{cov}(D_{jdt}, y_{jdt})}{\text{var}(D_{jdt})} \\
= \frac{\text{cov}(D_{jdt}, \alpha + \beta D_{jdt} - \beta w_{jdt} + v_{jdt} + \lambda_d + \delta_t + \epsilon_{jdt})}{\text{var}(D_{jdt})} \\
= \beta + \frac{\text{cov}(D_{jdt}, -\beta w_{jdt} + v_{jdt})}{\text{var}(D_{jdt})} \\
= \beta(1-\frac{\text{cov}(D_{jdt}, w_{jdt})}{\text{var}(D_{jdt})}) + \frac{\text{cov}(D_{jdt}, v_{jdt})}{\text{var}(D_{jdt})}
\]  (5)
Hence, \( \text{plim}(\beta_{\text{OLS}}) \neq \beta \). The first term in this expression represents measurement error bias coming from misclassified drought exposure. The second term represents additional bias generated by the relationship between misclassified migration and misclassified drought exposure. If measurement errors in drought and migration are uncorrelated (\( \text{cov}(D_{jdt},v_{jdt})=0 \)), we would be left with the standard downwards bias from measurement error generated by misclassification of a binary independent variable (Aigner 1973, Bound, Brown and Mathiowetz 2001 p. 3725-3726).\(^1\)

Because of how drought exposure is assigned to an individual (based on district of residence), it is likely that migration and drought errors are correlated, so we must evaluate \( \text{cov}(D_{jdt}, w_{jdt}) \) and \( \text{cov}(D_{jdt},v_{jdt}) \) to understand the net effects of the two biases in (5). The joint probability distribution of migration and drought variables is useful for this exercise:

**Appendix 2 Table 1: Joint PDF for migration and drought variables**

<table>
<thead>
<tr>
<th>Observed migration status ((y_{jt}))</th>
<th>True migration status ((y_{jt}^*))</th>
<th>Measurement error in migration ((v_{jdt}))</th>
<th>Observed drought exposure ((D_{jdt}))</th>
<th>True drought exposure ((D_{jdt}^*))</th>
<th>Measurement error in drought ((w_{jdt}))</th>
<th>Drought<em>error in drought ((D_{jdt}^</em> w_{jdt}))</th>
<th>Drought<em>error in migration ((D_{jdt}^</em> v_{jdt}))</th>
<th>Probabilities</th>
</tr>
</thead>
<tbody>
<tr>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>P1</td>
</tr>
<tr>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>0 0 0 0 0 0 0 0</td>
<td>P2</td>
</tr>
<tr>
<td>1 1 0 1 1 0 0 0</td>
<td>1 1 0 1 1 0 0 0</td>
<td>1 1 0 1 1 0 0 0</td>
<td>1 1 0 1 1 0 0 0</td>
<td>1 1 0 1 1 0 0 0</td>
<td>1 1 0 1 1 0 0 0</td>
<td>1 1 0 1 1 0 0 0</td>
<td>1 1 0 1 1 0 0 0</td>
<td>P3</td>
</tr>
<tr>
<td>1 1 0 0 0 0 0 0</td>
<td>1 1 0 0 0 0 0 0</td>
<td>1 1 0 0 0 0 0 0</td>
<td>1 1 0 0 0 0 0 0</td>
<td>1 1 0 0 0 0 0 0</td>
<td>1 1 0 0 0 0 0 0</td>
<td>1 1 0 0 0 0 0 0</td>
<td>1 1 0 0 0 0 0 0</td>
<td>P4</td>
</tr>
</tbody>
</table>

*P8=1-P1-P2-P3-P4-P5-P6-P7

From this, the following useful moments are available:

\[
E[D_{jdt}] = P1 + P3 + P5 + P7 \text{ which we will estimate by } \bar{D}, \text{ the fraction of observed drought exposures}
\]

\[
E[w_{jdt}] = P7 - P8
\]

\[
E[D_{jdt}w_{jdt}] = P7
\]

\(^1\) As discussed in Bound et al (2001), this result requires that the misclassification rate of drought exposure (sum of false negatives and false positives) does not exceed 1. This does not seem to be an unreasonably strong assumption to make in our context.
\[E[D_{jdt}v_{jdt}] = - P5 - P7\]
\[E[v_{jdt}] = -P5-P6-P7-P8\]

= - (1-P1-P2-P3-P4) which we will estimate by -(\(\bar{M}\)) and where \(\bar{M}\) is the fraction of incorrectly classified multiple movers.

\[V[D_{jdt}] = E[D_{jdt}^2] - E[D_{jdt}]^2\]
\[(E[D_{jdt}]) (1 - (E[D_{jdt}]))) which we estimate by \(\bar{D}(1 - \bar{D})\)\]

\[V[v_{jdt}] = E[v_{jdt}^2] - E[v_{jdt}]^2\]
\[(P5+P6+P7+P8) - (P5+P6+P7+P8)^2\]
\[(1-P1-P2-P3-P4)(1 -(1-P1-P2-P3-P4)) which we estimate using \((\bar{M})(1 - \bar{M})\)\]

With these, we can compute expressions for each bias term in (5). For the first term:

\[\text{cov}(D_{jdt}, w_{jdt})/\text{var}(D_{jdt}) = (E[D_{jdt}w_{jdt}] - E[D_{jdt}]E[w_{jdt}])/[(E[D_{jdt}]) (1 - (E[D_{jdt}])))\]
\[= (P7 - (P7-P8)*E[D_{jdt}])/((E[D_{jdt}]) (1 - (E[D_{jdt}])))\]
\[= (P7*(1- E[D_{jdt}]) + P8*(E[D_{jdt}]))/((E[D_{jdt}]) (1 - (E[D_{jdt}])))\]
\[= P7/(E[D_{jdt}]) + P8/(1- E[D_{jdt}]) > 0\]

We can estimate this expression as:

\[P7/\bar{D} + P8/(1 - \bar{D}) \quad (6)\]

To address the second term in (5):

\[\text{cov}(D_{jdt}, v_{jdt})/\text{var}(v_{jdt}) = (E[D_{jdt}v_{jdt}] - E[D_{jdt}]E[v_{jdt}])/(\text{var}(v_{jdt}))\]
\[= (- (P5+P7) - E[D_{jdt}]E[v_{jdt}])/((\text{var}(v_{jdt}))(1 - (E[v_{jdt}])))\]

We can estimate this as:

\[\bar{D}/(1 - \bar{M}) - (P5+P7)/(\bar{M})(1 - \bar{M}) \quad (7)\]

Putting (6) and (7) together, we can rewrite (5) as:

\[\text{plim } \beta_{\text{OLS}} = \beta(1 - \text{cov}(D_{jdt}, w_{jdt})/\text{var}(D_{jdt})) + \text{cov}(D_{jdt}, v_{jdt})/\text{var}(D_{jdt})\]
\[= \beta*[1 - (P7/\bar{D}) - P8/(1 - \bar{D})] + \bar{D}/(1 - \bar{M}) - (P5+P7)/(\bar{M})(1 - \bar{M})\]

The sign of this bias is ambiguous. If the true \(\beta\) is positive, we can consider what values of actual drought exposure \((\bar{D})\) and fraction of mismeasured movers \((\bar{M})\) would create a net downwards
bias. Then, we can use information from external datasets to learn whether these values are plausible in this South African case.

The net bias in (5) will be downwards when

\[ 1 > \frac{P7}{D} + \frac{P8}{1 - D} \]  
\[ \frac{D}{1 - \bar{M}} - \frac{(P5+P7)}{(1-\bar{M})} < 0 \] and small

If we further make the reasonable assumption that \( P7 = P8 \) (the misclassification of drought exposure for misclassified multiple movers is symmetric), and rearrange a) and b), these conditions become:

\[ a') \quad P7 < D^{*}(1 - D) \]
\[ b') \quad \bar{M} D - P5 - P7 < 0 \] and small

In words, these conditions imply that the bias in (5) is downwards whenever there are smaller fractions of multiple movers (small \( \bar{M} \)) and smaller observed fractions of drought-exposed cohorts (small \( D \)).

- In my data, \( D = 0.051 \) implying \( D^{*}(1 - D) = 0.048 \)
- I use an upper bound of 0.13 for \( M \); hence, \( D M = 0.006 \) or smaller.\(^2\)

Using the values in the South African data, as long as \( P7 < 0.048 \) and \( 0.006 - P5 - P7 < 0 \) and small, then \( \beta_{OLS} \) is likely downwards biased. Put another way, the fraction of misclassified multiple movers mistakenly assigned to drought exposure (\( P7 \)) would have to be larger than 0.048; and the fraction of misclassified multiple movers with any drought exposure (\( P5+P7 \)) would have to be smaller than 0.006 in order for (5) to overestimate the impact of drought on outmigration. This seems unlikely in the South African setting.

\(^2\)Data from the 2007 South African Community Survey and the 2007 Cape Area Panel Study indicate that the fraction of Africans who move more than once in the past five years is between 0.01 and 0.13 respectively. The 2007 Community Survey collects data on more than 300,000 African adults including their province of current and prior residences. The 2007 Cape Area Panel Survey is a sample of young adults (ages 24-33) drawn from a province with a highly mobile population, hence the higher rates of misclassification. Older data from the 1997 October Household Survey corroborates these numbers: among African adults aged 15 and older in 1986, only about 4% of them report moving at all across magisterial districts in any year prior to 1986. And, under 1% report multiple cross-district moves during the apartheid period.
Implications for estimating the triple difference model for migration

The focus of the paper is on results from triple-difference specifications of the relationship between drought and migration outcomes:

\[ y_{jdt}^* = \beta_0 + \beta_1 D_{jdt}^* + \beta_2 D_{jdt}^* TBVC_{jdt} + \lambda_d + \delta_t + \epsilon_{jdt} \]  (8)

where true migration is mismeasured for multiple movers and true drought exposure and TBVC status of the prior district could both be mismeasured. While the analytical framework for measurement error bias described above is not helpful in signing the bias in this specification, it inspires a specific robustness check. Restricting the sample over which (8) is estimated to later periods should provide a less biased measures of \( \beta_1 \) and \( \beta_2 \), since misclassification of migration (and hence drought) only occurs for moves prior to the last one. Hence, I test the robustness of the migration results estimating (8) over more recent subsamples of the data.

Note that in the paper, I conduct the outmigration analysis using district-year level data and the percent of adults outmigrating from each district in each year as the outcome variable. I obtain very similar results when estimating (8) on the (much larger) individual-level data.

Implications for disability regressions

In the analysis of health outcomes (disability rates) using (8), the only way that measurement error can creep in is through misclassification of drought exposure and TBVC status of birth district. Neither of these errors is likely to be related to measurement error in disability, so we expect estimates of \( \beta_1 \) and \( \beta_2 \) to be biased downwards (Bound, Brown and Mathiowetz 2001).

References

