National Road Upgrading and Structural Transformation: Evidence from Ugandan Households*

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Abstract

Sub-Saharan Africa is urbanizing, but a significant portion of the urban population still works in agriculture. We argue that this is because smaller cities are isolated from national markets and trade. We test this claim using individual panel data and Uganda's doubling of paved roads, which improved remote areas' market access. We find that market access causes workers to quit family farms for specialized paid employment outside of agriculture. Effects concentrate in peripheral areas, households with comparative advantage in off-farm work, and reflect off-farm opportunities rather than a reduced demand for farm output. We also find that market access leads remote households to simplify farming techniques and scale back farming. Findings are consistent with reliable transport enabling trade with major markets, creating opportunities to specialize according to comparative advantage.

Keywords: Market Access, Off-farm Employment, Economic Diversification, Job Specialization, Comparative Advantage, Road Infrastructure

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1 Introduction

Economic growth is often seen as inseparable from structural transformation, with labor moving out of agriculture into a more diverse industrial economy (Lewis, 1954; Desmet and Henderson, 2015). Structural transformation has been a key component of growth across Europe, the Americas, and Asia, but shifts out of agriculture have been slow for much of Sub-Saharan Africa. This is not because the region is stagnant; Sub-Saharan Africa is rapidly urbanizing, but urban households remain surprisingly reliant on farming. In fact, 20% of the region's urban workers earn their living from agriculture, a figure that rises to 40% in smaller cities and towns (Henderson and Turner, 2020). And while the drivers of Sub-Saharan Africa's urbanization are well studied (Gollin et al., 2016; Jedwab et al., 2017, e.g.), we do not know why this urbanization has failed to create manufacturing and service sector jobs.

We argue that barriers to trade, particularly poor transportation infrastructure, can explain why agriculture remains so prominent outside of the largest cities. Our central claim is that for smaller cities and towns to support manufacturing and service sectors, they need access to national markets. Policymakers have argued that inadequate inter-city transportation holds back Sub-Saharan Africa's secondary cities (Roberts and Anyumba, 2022), but empirical evidence is limited. Existing literature has shown that market access causes population to flock to the largest cities (Jedwab and Storeygard, 2022), but others have shown that this urbanization has failed to yield structural transformation (Gollin et al., 2016). What's more, this literature often overlooks the smaller cities and towns where most Africans live.[1]

To address this gap, we identify market access as a cause of structural transformation by studying Uganda's big push to double its stock of paved roads. This investment in infrastructure substantially improves long-distance travel and expands market access for urban centers and remote towns alike. We combine market access growth with individual panel data to study outcomes for

[1]This is a gap caused by data limitations that affect much, if not all, of the related literature. For example, Jedwab and Storeygard (2022) study cities with populations above 10,000. This restriction omits 85% of the population in the case of Uganda. Alder et al. (2022) use remote sensing data and lights at night, which are known to be sparse outside of major population centers (Goldbeck and Lindlacher, 2021).

households in both big cities and small towns. By tracking employment over time, we draw a direct link from market access to shifts out of agriculture and into paid work. Using detailed occupation and industry information, we find that new employment is in tradeable sectors and relatively specialized occupations. We also track farming activity to confirm that our results reflect actual structural transformation rather than competition. Indeed, market access increases farmgate prices, suggesting that shifts out of agriculture reflect a pull from better off-farm opportunities.

Uganda is an ideal setting for our study. Roads move virtually all of Uganda's freight cargo and policymakers have identified the road network's disrepair as a key barrier to regional trade and economic development Uganda Ministry of Work and Transport (2020). To address this concern, the federal government invested heavily in paving and rebuilding major roads during the 2010s. This dramatically reduced the cost of shipping goods across the country by improving travel times and reliability. To measure reductions in trade costs, we create a census of road upgrades completed from 2010 to 2020, geocode their locations, and compute associated travel time improvements.

We summarise local exposure to new roads with market access following Donaldson and Hornbeck (2016), Jedwab and Storeygard (2022), among others. Market access grows when road upgrades reduce travel times to big cities, and growth varies across the country depending on where people live and which roads are upgraded. Our identification strategy applies the inconsequential unit approach used by Chandra and Thompson (2000), Redding and Turner (2015), and Donaldson (2018). In our setting, road investments aimed to connect ten strategic locations. So, we focus on households outside of strategic locations to isolate variation in market access that road improvements incidentally caused. Beyond the inconsequential unit approach, we show that our results are robust to solely using the variation in market access induced by roads built far away as in Jedwab and Storeygard (2022) and controlling for non-random exposure to random road upgrades as in Borusyak and Hull (2023). We also find that results are not confounded by weather shocks or changes in electricity access, nor do they reflect pre-existing trends.

We first show that improved market access causes individuals to move from working on family farms to paid non-agricultural employment and that this effect concentrates among workers in peripheral areas. For someone living in a region that started with below-median market access, experiencing the average market access improvement raises the probability of taking paid non-agricultural work by 4.9 percentage points. This effect is a substantial effect in our setting, where just 13% of our sample works a paid job. In contrast, we find statistically and economically insignificant effects on job switches in less remote places.

In terms of specific job characteristics, we find that market access causes individuals in initially remote areas to move into occupations that are more unique and more often found in the capital city. Market access also shifts the non-agricultural sector towards tradable products (e.g. apparel manufacturing), creates jobs that are downstream of agriculture (e.g. beverage manufacturing), and does not create mining sector jobs. Overall, these findings are consistent with the idea that reliable transport connects people with urban areas, expands market extent, and increases demand for specialized off-farm work. This means that market access ultimately diversifies small-town economies and gives peripheral workers access to returns to specialization that economists typically associate with big cities. This interpretation matches theories of agglomeration economies in which market extent determines individuals' degree of specialization (Duranton and Puga, 2004).

We take employment results as evidence that improving access to urban areas creates opportunities for people to find specialized work that aligns with their comparative advantage. This view also implies that job shifts should be strongest for people who prefer working outside of agriculture, but work on family farms for lack of other opportunities. To examine this claim, we document that market access' effect on employment is strongest among households with unproductive family farms, weak land security, and poor weather conditions. Consistent with non-agricultural development, we show that improving market access increases built-up land and micro-enterprise profit. In addition, we show that the effect of market access on employment is not due to migration, local commuting, the fiscal-multiplier mechanism, or changes in access to agricultural inputs or technology.

We then study effects on farmgate prices to parse mechanisms and support claims about household welfare. While our main results suggest that market access increases off-farm labor demand, pulling people into off-farm jobs, market access can also increase farms' exposure to competition, pushing people into off-farm employment (Sotelo, 2020). From a farmer's perspective, these stories imply opposite changes in farmgate prices, with only the pull mechanism unambiguously making them better off. Empirically, the push mechanism is an inward shift in demand for a household's agricultural output, which should decrease price. In contrast, the pull mechanism can accompany increasing prices received on farm products that households continue selling. We find that market access reduces quantities sold and increases farmgate prices—evidence that the pull mechanism dominates and occupation shifts accompany improved household well-being. We also find that households use fewer high-quality inputs, suggesting that increasing farmgate prices do not reflect higher-quality output being produced as households get access to higher-quality inputs.

This paper contributes to ongoing literature on structural transformation and urbanization in developing countries. Although earlier literature suggests a close connection between urbanization and industrialization, many African countries experienced tremendous urbanization with limited structural transformation (Henderson and Kriticos, 2018; Henderson and Turner, 2020). A growing literature explores alternative explanations for Africa's urbanization, including resource booms (Ismail, 2010; Gollin et al., 2016), population growth (Jedwab et al., 2017), and climate change (Barrios et al., 2006; Henderson et al., 2017). Instead of explaining urbanization, we highlight remoteness and limited transportation as an obstacle to developing manufacturing and service industries. Documenting such obstacles can help explain why much of Africa's experience differs so dramatically from other recent growth stories, such as the rapid industrialization of many East Asian countries.

Many African countries still have substantial deficits in regional transportation infrastructure (Jedwab and Storeygard, 2022), and we provide new evidence that this limits non-agricultural development and specialization. This finding is related to Fafchamps and Shilpi (2005), who show that proximity to cities coincides with greater individual specialization and local diversification. In a similar vein, Cali and Menon (2013) find that urbanizing regions create positive spillovers that reduce poverty in nearby rural areas. We show that continuing to improve transportation networks in poor economies can expand access to large cities and spread structural transformation and specialization in smaller towns. We also contribute a finer view of individual job transitions and provide a new test to rule out the influence of agricultural-market competition.

Our findings also shed new light onto the interdependence of small and large cities, which has implications for how systems of cities develop. In many African countries, urbanization is heavily concentrated in a single city, while smaller cities and towns often benefit less from economic growth (Henderson and Kriticos, 2018). This excessive urban primacy can prevent labor mobility and job creation (Christiaensen and Todo, 2014), lead to sub-optimal sectoral specialization across cities (Duranton and Puga, 2001), and ultimately hinder productivity growth (Henderson, 2003). Our results highlight that smaller cities rely on access to major urban centers, and that inter-city roads can mediate this access.

Finally, we add new results to the empirical literature on the economic effects of market access. Work in this vein documents how market access affects regional income (Jaworski and Kitchens, 2016), land value (Donaldson and Hornbeck, 2016), productivity (Hornbeck and Rotemberg, 2021), environment (Asher et al., 2020; Abman and Lundberg, 2023), comparative advantage and trade (Adamopoulos, 2025; Fajgelbaum and Redding, 2022; Sotelo, 2020; Duranton et al., 2014; Baldomero-Quintana, 2020).[2] Our study is unique in using individual and household panel data rather than city- or region-level aggregates.[3] This allows us to conclude that effects on employment reflect incumbent residents switching jobs; an effect that previous literature cannot separate from changing worker composition. Our data also allow us to test specific mechanisms by examining the characteristics of individual job-switchers, the jobs that they leave behind, and the jobs that they move towards.

The remainder of the paper proceeds as follows. Section 2 describes the relevant features of the context and Uganda's National Road Network upgrades, as well as a theoretical framework

[2]Studies in developing countries find that by improving market access, inter-city roads typically cause growth in primary cities and that remote areas benefit in some contexts (Jedwab and Storeygard, 2022; Peng et al., 2024; Fenske et al., 2023) but are adversely impacted by competition in others (Baum-Snow et al., 2020; Faber, 2014).

[3]Using household panel data is more common in studies of feeder roads and rural households (Kebede, 2024; Gebresilasse, 2023; Shamdasani, 2021; Asher and Novosad, 2020; Nakamura et al., 2020; Dumas and Játiva, 2020). We diverge from that literature by focusing on the role of access to markets and travel times to major hubs.

to guide the rest of the analysis. We introduce the data in Section 3 and identification strategy in Section 4. We then report empirical findings in Section 5 and Section 6 concludes.

2 Background and Theoretical Framework

2.1 Uganda's Economic Geography

More than eighty percent of Uganda's population lives in towns of less than 10,000 people and two-thirds of the country's jobs are in subsistence agriculture (Uganda Bureau of Statistics, 2016). The only urban center exceeding one million people is Kampala, the country's capital, which sits in central Uganda on Lake Victoria's coast. Kampala is the National Road Network's largest hub and our market access calculations show that the central and eastern regions (which contain Kampala and a major trade route to Kenya) were Uganda's least remote areas at the outset of our study period, as highlighted in Figure B.1(a).

Uganda is subdivided into regions, which are further divided into districts, then counties, and finally subcounties. Subcounties are our primary unit of geography and identify household locations in our main dataset (see Section 3.1). The mean subcounty land area is 210 square kilometers, which is equivalent to a circle with a radius of 8 kilometers. To create time-consistent geographical boundaries, we use 2018 subcounty boundaries and harmonize boundaries across years in our household panel data.[4]

2.2 National Road Upgrades

Uganda's National Road Network (NRN) carries nearly all of the country's freight cargo but was largely unpaved and often in poor condition in the late 2000s (Uganda Ministry of Work and Transport, 2020). In 2008, the federal government launched a fifteen-year plan for NRN upgrades and repairs, and by the end of 2019, it had nearly doubled the stock of paved roads (Uganda Ministry of Work and Transport, 2020). These road upgrades were part of a broader long-

[4]The 2018 administrative boundary files are obtained from https://www.arcgis.com/home/item.html?id=2897e7de50c84c189f47906c1db57c76 in September 2022.

term development initiative called Vision 2040, which highlights ten strategic cities and towns for targeted infrastructure investment, including “a multi-lane paved national road network linking major towns, cities, and other strategic locations” (Uganda National Planning Authority, 2013). The Vision 2040 cities include six regional hubs—Kampala, Gulu, Mbale, Mbarara, Arua, and Jinja—and four special economic zones: Nakasongola (industrial potential), Kabarole (tourism potential), Hoima (oil production), and Moroto (mining). Our empirical analysis leverages this investment design by examining the incidental connections to infrastructure across the rest of the country.

Figure 1 presents an example of a road that was upgraded from its initial unpaved state to a sealed all-weather road in 2019. This road is 150 KM west of Kampala and is typical of upgrades in remote areas.[5] Figure 2(a) maps NRN upgrades during our sample period; road upgrades are highlighted by completion year and districts housing the ten strategic cities are in yellow. This map shows that strategic cities all contain upgraded roads and some sit at intersections of major NRN routes, confirming that upgrades do indeed target these strategic locations. And while Figure 2(a) presents 44 upgrading projects completed by the end of our sample period, 25 were still in progress. In Section 5.4, we use these ongoing projects for robustness tests that build the recentered market access measure developed by Borusyak and Hull (2023).

There is anecdotal evidence of these road upgrading projects enabling industrial activity across Uganda. For example, Kampala-Jinja highway’s construction was followed by a tenfold increase in the number of factories in the Namanve Industrial Park, which is along the highway in the Mukono District (Wandati, 2024). The Vurra-Arua-Koboko-Oraba road encouraged development in fruit and tobacco processing, juice production, and mineral and honey processing facilities (Monitor, 2017). With these cases in mind, our econometric analysis examines systematic effects on industrial development and structural transformation through the lens of workers’ occupational choices

[5]Some projects, particularly in central areas, upgrade roads that were already paved but in disrepair, creating unmeasured heterogeneity in travel improvements. Looking forward, our focus on remote areas minimizes concerns about this sort of measurement error, since these roads were mostly unpaved in the 2000s. Due to the unavailability of satellite images for the entire country over the study period, we are unable to use remote sensing methods to measure changes in road quality.

and households' production decisions.

2.3 Conceptual Framework

We organize our empirical analysis around a simple model of occupation choice and structural transformation. This partial equilibrium model focuses on how improvements in market access change the returns to working in different sectors, which, in turn, affect workers' job choices. The model highlights that, for a particular location, improved access to major markets causes a shift away from agriculture, either due to intensified agricultural competition or enhanced non-agricultural opportunities. Consequently, this shift does not necessarily improve every worker's welfare, and uncovering the mechanism (or ruling out alternatives) is crucial. The model focuses on roads' effect on the costs of transporting goods to and from major cities. However, employment outcomes can also reflect migration and commuting decisions that might also respond to road improvements. So, the end of this section considers alternative channels that Section 5 examines empirically.

The model considers many workers in a location choosing to work in agriculture or manufacturing, depending on which gives higher utility. Each worker is defined by an idiosyncratic preference ϵ; this captures individual comparative advantage in farming or barriers to entering manufacturing. Working in agriculture gives utility $u_a(p_a)$ and manufacturing gives $u_m(p_m) - \epsilon$; p_a and p_m are local output prices in agriculture and manufacturing, and utility increases in each sector's output prices. This simple reduced-form model is isomorphic to workers valuing profits, wages, or even farming to secure their basic food needs as in Gollin and Rogerson (2010). Assuming ϵ is continuously distributed with cumulative density $F(\epsilon)$, the probability of working in manufacturing is $\Pi_m(p_m, p_a) = F\left(u_m(p_m) - u_a(p_a)\right).$

Each sector's output prices depend on location-specific supply and demand shifters, including market access. In autarky (low/no market access), prices reflect unmodelled local markets. With free trade (high market access), prices converge to global levels. Both local and global prices are exogenous to workers, and whether market access causes prices to shrink or grow in a location is an empirical matter.

As new roads increase market access by reducing the cost of shipping goods to and from big cities, market access shifts both p_a and p_m. Accordingly, market access' marginal effect on occupation choice is

$$\frac{d\Pi_m}{d(\text{MA})} = F'\left(u_m(p_m) - u_a(p_a)\right)\left(u'_m(p_m)\frac{\partial p_m}{\partial(\text{MA})} - u'_a(p_a)\frac{\partial p_a}{\partial(\text{MA})}\right).$$

Therefore, market access causes switching from agriculture to manufacturing if

$$u'_m(p_m)\frac{\partial p_m}{\partial(\text{MA})} > u'_a(p_a)\frac{\partial p_a}{\partial(\text{MA})}. \tag{1}$$

Equation 1 shows that market access can shift marginal farmers towards manufacturing under two conditions: (1) if the demand for manufacturing grows and exceeds the growth in agricultural demand, or (2) if competition from national sellers reduces all prices, with the impact on manufacturing being less pronounced.[6] Scenario 2 reflects competition effects that hurt manufacturing less than agriculture, causing job switching and decreasing utility.[7] By contrast, Scenario 1 represents the creation of new off-farm opportunities and structural transformation within the framework of Lewis (1954).

It is not immediately clear whether market access will cause competition or structural transformation in a location. For example, Krugman (1991) shows that opening peripheral regions to trade with big cities may adversely affect the manufacturing sector in those peripheral areas. Faber (2014) and Baum-Snow et al. (2020) find that Krugman's prediction played out in China. Sotelo (2020) provides a quantitative framework illustrating that better roads can either boost demand or intensify competition in the agricultural sector. Our empirical results show that $\frac{\partial p_a}{\partial \text{MA}} > 0$ (see Section 5.5.3), which is consistent with structural transformation outweighing competition.

[6] The first scenario corresponds to $\frac{\partial p_m}{\partial(\text{MA})} > \max\left\{0, \frac{u'_a(p_a)}{u'_m(p_m)}\frac{\partial p_a}{\partial(\text{MA})}\right\}$, and the second scenario is defined by $u'_a(p_a)\frac{\partial p_a}{\partial(\text{MA})} < u'_m(p_m)\frac{\partial p_m}{\partial(\text{MA})} < 0$.

[7] Note that utility must fall if both prices fall, but whether any given individual moves jobs depends on relative marginal utilities, which, in practice, can vary across individuals and locations.

To us, road upgrades reduce the cost of shipping outputs; but input costs can also be affected. For manufacturing, this increases the return to labor, and so is isomorphic to increasing p_m. For agriculture, reducing the costs of fertilizers or pesticides could increase their use, which we speculate would increase returns to agriculture captured by p_a. However, this mechanism is less relevant in our context, where farms are small and rarely use fertilizers, pesticides, and improved seeds.[8] To the extent that these inputs matter in our context, empirical results find that improving market access reduces the use of fertilizers and improved seeds, and does not affect pesticide use (see Section 5.5.3).

In principle, improved roads could also facilitate migration by making it easier to relocate to markets with better opportunities. However, data from the 2014 Uganda Population Census (Ruggles et al., 2024) indicate a low migration rate, with only 8% of households having moved to their current locality within the past five years. Moreover, as shown in Section 5.5.4, changes in market access do not appear to influence individual migration. Motivated by these empirical findings, the model does not incorporate migration as a response to road improvements.

Similarly, upgraded roads can be used for local commuting, which can also affect occupation choices. While the model does not incorporate this channel, we explicitly test and reject it in Section 5.5.4. We find that road upgrades within a location or close to this location alone do not explain market access' effect on employment. Instead, our results reflect improvements to long-distance trips that are more relevant for moving goods.[9]

[8] As shown in Table A.1, the share of households using pesticides, fertilizers, and improved seed are 10%, 12%, and 14%, respectively.

[9] This is expected, since our main results focus on relatively remote areas, where vehicle access is limited and major centres are too far away to justify commuting, even under ideal conditions.

3 Data

3.1 Household Panel Data

Outcome variables come from seven waves of Uganda National Panel Survey (UNPS) microdata covering 2009 to 2019.[10] These data include unique identifiers for individuals and households that we use to build panels of individual employment outcomes, demographics, and household agricultural activities. Panels are unbalanced because of regular turnover in the survey, we restrict the sample to individuals 14 years or older to avoid capturing primary-school-aged people, and we define individual locations as the centroid of their subcounty. Almost all UNPS households remain in the same subcounty over the entire time period, making this a sample of incumbent households.[11] Our data cover 5,201 households falling in 564 subcounties, and Figure B.1(b) illustrates the subcounties covered by the UNPS.

Our broadest occupation categories indicate whether each individual works on their household farm or with family-owned livestock (agriculture), for someone else for pay (employee), as an own-account worker (self-employed), or is not employed.[12] We also observe employees' reports of whether their employer contributes to a pension fund, deducts income taxes, and offers paid leave, which we take as indicators of higher-paying formal employment. Finally, we identify specialized and urban-oriented jobs using ISCO occupation classifications that identify 104 unique occupations in our sample.[13] In addition, we use ISIC industry classifications to identify tradable

[10]The UNPS is a multi-topic household survey initiated in 2009, and we use data from 2009, 2010, 2011, 2013, 2015, 2018, and 2019 (Uganda Bureau of Statistics, 2014). Each survey round includes two visits to assess agricultural outcomes in both of Uganda's traditional cropping seasons. For the rest of household and individual outcomes, data is collected once per survey round. In practice, surveyors work year-round, and survey responses are roughly evenly distributed across months.

[11]Only 5% of households switched subcounties during the sample period. In the main analysis, we assign these households to the subcounty where they spent the majority of their time during the survey period. If we instead drop households who switch subcounties, our results do not change (shown in Appendix Table A.7, Column 6); this confirms that our findings do not reflect selection via migration.

[12]We categorize individuals as own-account workers when they are employers, own-account workers, apprentices, or unpaid helpers in a household business. Apprentices and unpaid helpers in a household business together account for 2.5% of the sample. Excluding them from this category produces nearly identical results, and the estimation is available upon request.

[13]The occupation classification system changed in 2013, and we manually harmonize the occupation systems over time. Appendix C provides details.

outputs, outputs downstream of agriculture, and jobs in the mining sector.[14]

For supplementary analysis, we aggregate plot-level indicators of seed type, pesticide use, fertilizer use, and crops grown to track household agricultural activities. We also use the procedure described in Aragón et al. (2022) and Tian et al. (2022) to construct total factor productivity (TFP) for each household in every cropping season. The UNPS also documents whether household farms were affected by the following five natural disasters in the past 12 months: drought or irregular rains, floods, landslides or erosion, unusual pests and crop disease, and unusual livestock disease. Finally, households that own non-agricultural enterprises report their profits, the number of workers hired, and their total wage bill. Appendix C defines the UNPS variables used in our analysis.

Table A.1 summarizes the UNPS sample. Panel A shows that the majority of individuals work on family farms, while the remaining workers are equally likely to hold paid positions or engage in self-employment.[15] Additionally, most jobs do not offer pensions, income tax deductions, or paid leave.[16] Panel B shows that paid employment is approximately evenly spread across major occupations. Panel C summarises farming households' agricultural practices and shows that very few households use pesticides, fertilizer, or improved seeds. Finally, Panel D summarises demographic information in the UNPS sample.[17] Importantly, our central analysis focuses on a subsample excluding households from Vision 2040 districts and Table A.1 shows that this restricted sample is observably similar to the nation as a whole.

3.2 Geographic Data

We use 2002 census microdata, which represents the most recent census year prior to the road upgrading investments, to compute baseline local population, age, gender, education, migration,

[14] Job details, including occupation and industry information, are based on a one-week recall period.

[15] Panel A's first column shows that 49.1% of individual-years in the sample work on a family farm, which grows to 53.5% in the subsample without Vision 2040 districts. Columns 2 and 3 show that paid employment accounts for 13.4% of the sample and is roughly as common as self-employment.

[16] The final three columns of Panel A report low rates of employer-provided pensions, income tax deductions, and paid leave. They are all mechanically zero for family-farm and self-employed workers. Conditional on paid employment, 14% of employees report pension access, 20% report income tax deductions, and 17% report paid leave.

[17] For the full sample, the sample is 48% male, with a 78% literacy rate and an average age of 33.4 years. The average household contains 5.4 people, and half of households possess non-customary land. Households rarely rely on commercial farming as their main source of income, with only 3% of households reporting commercial farming as their primary income source in a given year.

and sectoral composition (Minnesota Population Center, 2020). We interact these baseline characteristics with time fixed effects in the econometric analysis to account for any potential convergence or divergence between areas with varying population size, working population, education levels, etc., which might confound the effects of market access.[18]

We also measure subcounties' built-up area using the Copernicus Global Land Cover dataset (Buchhorn et al., 2020). Built-up area is a commonly used proxy of urban development and non-agricultural activity, as it directly reflects the physical footprint of human-made structures and infrastructure rather than farmland or natural areas (Angel et al., 2011). Copernicus classifies land cover at a 100-meter resolution, and we extract each subcounty's built-up land area in 2015 (the first year land cover data are available) and 2019 (the last year of our study period).

3.3 Travel Times and Road Upgrades

We compute origin-destination travel time reductions due to road upgrading by combining the timeline of NRN upgrades with routes and travel times from the Open Source Routing Machine (OSRM) (Luxen and Vetter, 2011). First, we digitize annual reports from Uganda's Ministry of Works and Transport to record the location and completion date of each NRN construction project completed between 2010 and 2020 (Uganda Ministry of Work and Transport, 2020). Next, we use the OSRM to identify endline year driving routes and travel times for each origin-destination pair.[19]

We then overlay the road upgrading projects onto the driving routes to calculate the share of each route's length upgraded annually. With upgrade shares and endline year travel times in hand, we compute historical travel times by simulating the removal of NRN upgrades. To this end, we

[18] In addition, baseline-year local population is used to construct market access, our main independent variable, as detailed in Section 3.4. These variables are measured at the county level (rather than subcounty) because the 2002 census is not available at the subcounty level. On average, a county includes 7 subcounties.

[19] This calculation uses a snapshot of the 2021 road network from the Geofabrik OpenStreetMap repository to deliver travel times and route lines through the fully upgraded road network. Origins are always the centroid of subcounties because we observe them in household panel data, and destinations are the centroid of counties because census population data used to compute market access are only available at the county level.

aggregate upgrade shares to the endline year, 2021, and estimate the following pairwise regression:

$$ln(time_{od2021}) = \zeta UpgrShr_{od2021} + \gamma_1 dist_{od} + \gamma_2 dist_{od}^2 + \gamma_3 dist_{od}^3 + \alpha_o + \alpha_d + u_{od}, \quad (2)$$

where $time_{od2021}$ represents the travel time (in hours) from origin o to destination d with all road upgrades intact (from OSRM), $UpgrShr_{od2021}$ is the cumulative share of road length upgraded, α_o and α_d are origin and destination fixed effects, and we include a cubic polynomial in driving distance $dist_{od}$. The coefficient ζ becomes our conversion factor from upgrade shares to travel times and is identified by comparing differences in current travel times across routes of similar lengths but varying levels of road upgrade coverage.[20]

An ordinary least squares estimate of Equation 2 gives a coefficient of $\zeta = -0.306$, suggesting that upgrading 10% of a route's length will decrease travel time by 3.06%.[21] With this estimate in hand, we compute travel time in prior year t as

$$time_{odt} = time_{od2021} \times \exp\Bigg(-0.306 \times \big(UpgrShr_{odt} - UpgrShr_{od2021}\big)\Bigg), \quad (3)$$

which only changes over time to reflect NRN upgrades. Travel takes longer in year t if the fully upgraded travel time is long ($time_{od2021}$ is large) and the route has not yet been upgraded to its current state ($UpgrShr_{odt}$ is less than $UpgrShr_{od2021}$).[22] Using changes in travel time to Kampala—the primary city—to illustrate the magnitude of these improvements, Rows 1 and 2 of Table A.2's first column show that, on average, subcounties were 5.4 hours away from Kampala at baseline, and

[20] To backcast travel times using Equation 2, we assume that present-day differences in travel times between upgraded and non-upgraded routes (which we observe) are the same as differences between present-day and pre-upgrade travel times on a given upgraded route (which we compute). Since OSRM avoids roads that are slow or impassible in 2021, travel time differences across routes can reflect both slower speeds along unpaved roads and re-routing to avoid unpaved roads. Therefore, backcasted travel times can also reflect both slower speeds and re-routing.

[21] ζ has a two-way o- and d-clustered standard error of 0.018. The regression has $R^2 = 0.97$, suggesting that upgrade shares, distance, and high-dimensional fixed effects account for most first-order determinants of travel time. If upgraded roads tend to have the worst counterfactual travel conditions, perhaps due to a lack of re-routing options or particularly poor initial road quality, this procedure might understate travel time improvements. In addition, controlling for a cubic polynomial in the mean terrain ruggedness index along each route (Nunn and Puga, 2012) barely changes the estimation of ζ, and results are available upon request.

[22] Imputed travel times are never slower than current travel times since cumulative and permanent upgrades imply that $UpgrShr_{odt} \leq UpgrShr_{od2021} \; \forall \; o, d, t$.

this travel time decreased by 21% by the end of our study period.[23]

3.4 Market Access

Following Jedwab and Storeygard (2022), we define market access as a household's travel-time-discounted sum of the population in all potential destinations.[24] Specifically, the market access of a household in subcounty k in year t is given by:

$$\text{MA}_{kt} = \sum_d \frac{pop_d}{time_{kdt}^{\theta}}, \tag{4}$$

where pop_d is the population of destination d in the baseline year, $time_{kdt}$ is the travel time between k and d, and θ is the elasticity of trade flows with respect to travel time.[25] This specification reflects a location's ease of accessing people and markets, and it grows when roads to population centers are upgraded. Equation 4 is a reduced-form approximation of market access terms derived from general equilibrium trade models (Donaldson and Hornbeck, 2016), and we demonstrate that our results are robust to using the traditional model-based measure.[26]

The elasticity parameter θ is the product of the elasticity of trade flows to trade costs and the elasticity of trade costs to travel time. We calibrate the elasticity of trade costs with respect to travel time to 0.169 following Donaldson (2018). The elasticity of trade flows with respect to trade costs ranges from 3.8 to 8.2 in modern-day developing countries and in historical settings in developed countries (Peng et al., 2024); we use the midpoint as our main specification and explore alternative values as robustness checks.[27] [28]

[23]Column 2 adjusts sample weights for the household panel's geographic distribution, finding a shorter baseline time of 4.9 hours to Kampala, as remote areas are sparsely populated. Column 3 shows that excluding the Vision 2040 plan's strategic cities raises the baseline time to 5.1 hours but has little effect on average travel time reductions.

[24]As in Jedwab and Storeygard (2022), we use travel times instead of iceberg trade costs due to the lack of suitable shipment value.

[25]We fix populations at their baseline levels so that changes in market access are solely driven by reductions in travel time.

[26]The structural market access measure divides destination d's population by its own market access, which allows a destination influences market access more if it is an exclusive trade partner. We iteratively compute structural market access as $\text{MA}_{kt} = \sum_d \frac{pop_d}{time_{kdt}^{\theta}} \frac{1}{\text{MA}_{dt}}$.

[27]Together, our main specification of θ is 1.014, which is close to values used in Harris (1954), Berger and Enflo (2017), Peng et al. (2024), Marein (2022), among others.

[28]These values are estimated using data from other developing countries or historical settings in developed countries

As reported in Row 4 of Table A.2, the average subcounty's market access grew by about 15% during the sample period, with substantial variation across subcounties.[29] Figure 2(b) shades subcounties according to deciles of market access growth between 2009 and 2019. Market access growth is most pronounced in subcounties with upgraded roads, but there is also substantial growth in those without direct upgrades. Among subcounties that do not contain upgraded roads, market access grows if an upgraded road is between these subcounties and a major market. So, market access captures indirect effects that a binary treatment variable (indicating whether a subcounty contains an upgraded road) cannot. Figure 2(b) also shows that market access growth disproportionately clusters around major cities like Kampala. Concentration around Kampala motivates our empirical strategy which focuses on peripheral areas that were not targeted by national plans to identify effects on household outcomes.

4 Empirical Strategy

To estimate the effects of road improvements on individuals' job outcomes and households' economic activities, we start with the following two-way fixed effect regression:

$$y_{ikrt} = \beta \ln(\mathrm{MA}_{kt}) + \alpha_i + \tau_{rt} + X_k\gamma_t + Z_i\varphi_t + \mu_{it}, \tag{5}$$

where y_{ikrt} is the outcome of individual or household i located in subcounty k, region r in time t. Road improvements enter by increasing market access MA_{kt} and the goal is to identify β, which captures the effects of road upgrades. Unit fixed effects, α_i, force identification based on changes over time, controlling for both individual heterogeneity and fixed location characteristics. Region-time fixed effects, τ_{rt}, control for differential trends in Uganda's four economic regions.[30] Controls

(rather than Africa). For robustness we also consider calibrating θ to 2.10, following Buys et al. (2010), who estimate a trade-distance elasticity of -2.10 based on trade flows across Sub-Saharan African countries.

[29] Column 2 reveals that both the mean and standard deviation of growth increase when weighted to reflect household data. Column 3 confirms that excluding Vision 2040's strategic cities does not alter the substantial average growth or its variation across subcounties.

[30] Specifically, region-time varying effects control for factors such as the development of the growing oil sector, which can disproportionately affect northern regions and confound the effects of market access growth. Similarly, they help mitigate concerns about trade routes impacting the southeast region during the study period. Furthermore, as detailed in Section 5.4, the main results remain unchanged when subcounties near the southeast trade routes or mining

$X_k\gamma_t$ include a battery of baseline census year local characteristics interacted with time dummies to capture convergence or divergence across areas with different demographic and economic characteristics.[31] [32] Additionally, we include individual age and its square as well as individual literacy and gender interacted with time dummies ($Z_i\varphi_t$). The error term is clustered at the subcounty level since our treatment variable varies at this level. We also demonstrate the robustness of the standard errors when clustering at a higher geographical level or using spatial clustering methods.

With unit and region time-varying effects, along with the time-varying effects of local characteristics, identifying causal effects requires that among subcounties within the same region and similar baseline characteristics, changes in market access are unrelated to unobservable shocks that would affect household outcomes. However, NRN upgrades is part of the Vision 2040 development plan, which aims to develop ten strategic cities that policy and infrastructure investments should support. Therefore, a major threat to identification is that market access might grow most near strategic cities and resource endowments, which independently affect employment opportunities and are not fully captured by controls.

To address this identification challenge, we apply the inconsequential unit approach (Chandra and Thompson, 2000; Redding and Turner, 2015; Donaldson, 2018): we omit households sharing a district with any Vision 2040 city. This approach ensures that our estimation sample focuses on households in peripheral areas, as is relevant for our analysis, while leveraging market access growth driven by incidental connections intended to link ten strategic cities.[33] While the targeted districts experienced the largest growth in market access, omitting households from these districts

sites are excluded.

[31]We include log population and shares of households who moved within five years, are aged 18 to 64, have completed primary school, identify as female, work as subsistence farmers, for wages, or in self-employment. Time-interacted census controls further restrict comparisons to subcounties that begin with similar economic and demographic features.

[32]Table A.3 presents a balance test, comparing baseline characteristics of subcounties that see low versus high market access growth during our study period. The two groups display similar demographic profiles; we find negligible differences in female share, prime working age share, and subcounty population. Notably, we also find that low- and high-growth groups have comparable baseline market access on average. However, we do find that high-growth subcounties begin with lower rates of subsistence farming and higher rates of primary school completion. So, we emphasize that our main specification controls for time-varying effects of all these demographic and economic characteristics. By doing so, any location-specific trends linked to local demographics will not confound the results.

[33]Redding and Turner (2015) summarises a literature that uses incidental connections identification strategies in similar settings.

barely affects market access growth's variance.[34]

Focusing on peripheral regions also alleviates concerns about market access mechanically growing in the country's economic center, as noted by Borusyak and Hull (2023). Figure 2(b) shows substantial market access growth near the capital city of Kampala; this is both because it is a large market and because it is at the center of the pre-existing network of major roads. So, even if upgrades were randomly distributed across pre-existing roads, they would disproportionately improve market access near Kampala. This would be a problem because there are many reasons why Kampala would disproportionately create new jobs. Our peripheral sample addresses this concern by excluding households near Kampala and other major centers. Additionally, in Section 5.4, we apply the recentered-IV approach proposed by Borusyak and Hull (2023), which confirms that our main results remain robust.

Another potential threat to identification is the possibility that positively selected households move toward areas with growing market access. This is unlikely to be significant in our context due to Uganda's low migration rate. According to the 2014 census, the average household had lived in its current locality for 36 years, and only 8% of households have moved within the past five years (Ruggles et al., 2024). For a more direct test, Section 5.5.4 uses census data to estimate the effect of market access growth on the likelihood of individuals moving into a locality. We find no relationship between in-migration and market access growth. Additionally, we find that market access growth does not lead to migration from less populated areas to densely populated city centers. This suggests the presence of significant migration frictions in our setting, which aligns with findings from Frohnweiler et al. (2024).

[34]Comparison of Columns 1 and 3 in Table A.2, Row 4 shows that omitting Vision 2040 districts reduces the mean and standard deviation of market access growth among UNPS households to levels that resemble those of the nation as a whole (with equal weights across subcounties).

5 Main Results

5.1 Paid Employment

We first estimate market access' effect on the individual probability of working in paid employment and report the results in Table 1. The dependent variable across Columns 1 to 4 is one for paid employees and zero for all other outcomes, which include family farm work, self-employment, and unemployment. All regressions include individual fixed effects, individual controls, and region-year effects, and subcounty-clustered standard errors are in parentheses. Column 1 shows that improved market access is associated with shifts into paid employment. Column 2 adds baseline census controls to find that experiencing average market access growth increases one's probability of taking paid employment by 2.8 percentage points.[35]

Splitting the sample according to baseline market access reveals that effects are driven by the country's most remote areas.[36] Specifically, Column 3 of Table 1 shows that in areas with below-median baseline market access, experiencing average market access growth increases the probability of paid employment by 4.9 percentage points.[37] In contrast, the effect is close to zero for areas with above-median baseline market access (shown in Column 4).[38] Furthermore, Columns 5 and 6 confirm that shifts into paid employment reflect new non-agricultural jobs rather than people finding similar work on nearby farms.[39]

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[35]The sample average of market access growth comes from the Table A.2's bottom right cell: $0.187 \times 0.15 = 0.028$.

[36]We split households in half based on their market access before 2009 and run separate regressions in each group. Figure B.1(a) illustrates the subcounties classified as remote areas. Since most subcounties within Vision 2040 regions—which are excluded from our analysis—have above-median baseline market access, Column 3 has more observations than Column 4. A pooled regression yields nearly identical results, which are available upon request. While not reported in the paper, estimating effects by market access quartile reveals a consistent pattern: the most negative effects are concentrated among workers in the bottom quartile of market access, followed by those in the second-lowest quartile, with effects in both the third and fourth quartiles close to zero.

[37]We show in Table A.4 that male workers, those in their prime age, and individuals from families with a higher number of working-age members are most likely to take the paid positions. We find similar responses between literate and illiterate workers.

[38]Since most subcounties within Vision 2040 regions have higher baseline market access, Column 3 contains more individuals than Column 4.

[39]One important consideration is whether improving remote areas' market access creates new jobs nationally or reallocates urban jobs to smaller cities and towns. If increased paid work in remote areas comes at the expense of other places, then market access would decrease paid work in places that were initially less remote. Instead, Table 1 Column 4 shows that improving market access in initially less remote areas has an economically and statistically insignificant

Table 2 goes on to confirm that increasing paid employment reflects a decreasing probability of working on a family farm.[40] Panel A finds a large and significant drop in the probability of family farm work, a relatively small and statistically insignificant increase in the probability of being self-employed, and no notable effect on non-employment. Panels B and C split the sample on initial market access to find that, once again, workers in initially remote areas drive these effects.

Together, these results indicate that by improving market access, road upgrades cause workers in remote areas to switch from working on family farms to supplying labor for non-agricultural markets.[41] And since workers in subcounties starting with below-median market access drive these effects, the remainder of our investigation focuses on this subsample. Sections 5.2 and 5.3 proceed to examine the occupations and industries of new jobs more closely, and Section 5.4 presents robustness tests.

5.2 Job Specialization

Having established that market access drives shifts into paid employment, we now explore whether these jobs are relatively unique or resemble those typically found in big cities. To this end, we define individual outcomes measuring their occupation's uniqueness—how rare it is on a national scale—and urbanness—how concentrated their occupation is towards the capital city. Specifically, let N_j be national employment in occupation j and $j(i)$ the occupation of individual i. Then, uniqueness of i's occupation is

$$\text{Uniqueness}_i = 1 - \frac{N_{j(i)}}{N}$$

effect on market labor supply. And while we cannot rule out competition between remote areas or reallocation away from Vision 2040 districts, this provides suggestive evidence that new jobs in previously remote areas do not come at the expense of growth in initially better-connected cities.

[40] Each cell is the effect of market access on one of three mutually exclusive employment states, and outcomes are zero for individuals working in any other category, including paid employment.

[41] Table A.5 documents additional features of the paid positions that workers take when market access grows. Column 1 shows no effect of market access on the probability of employer pension contributions. However, Columns 2 and 3 show positive effects on the probability of employers deducting income tax and offering paid leave. These patterns suggest movement towards well-compensated and formal jobs but warrant cautious interpretation because employer pensions, income tax deductions, and paid leave are only indirectly related to compensation and job formality and are all relatively rare in our sample.

where $\frac{N_{j(i)}}{N}$ is the nationwide share of jobs in i's occupation, j. This measure is zero when everyone takes the same job and approaches one as an individual's job gets less common nationally. We define a related measure of a job's urbanness using Kampala's share of each occupation's national employment. As with uniqueness, urbanness varies from zero to one, with zero meaning that i's job never appears in the capital city and one meaning the job only appears in the capital.

We use 2002 census microdata to compute each occupation's uniqueness and urbanness and Table A.6 presents values for select occupations in the bottom, middle, and top of the uniqueness distribution. For example, uniqueness and urbanness are both small for subsistence crop farming, which accounts for 67% jobs nationally and just 0.2% of jobs in Kampala. In contrast, the 2002 census reports fewer than 500 software developers nationally, and two-thirds of them were in Kampala, making software development very unique and very urban.

Columns 1 and 2 of Table 3 present effects of market access on the uniqueness and urbanness of jobs. Column 1 estimates the effect on uniqueness to find that, indeed, market access leads individuals to specialize in less common jobs. Column 2 estimates the effect on urbanness to find a rise in the propensity to take jobs that typically concentrate in the capital city.

One concern is that subsistence agriculture takes such a large share of employment that effects on uniqueness and urbanness mechanically reflects shifts from family farms to paid work. Numerically, Table 3 implies that receiving average market access growth increases their jobs' uniqueness by 0.063. This effect is more than double the 0.026 difference that mechanically follows from the coarse occupation shift in Section 5.1.[42] This comparison suggests that households take particularly specialized and unique jobs—even relative to paid employees across the country—when their market access improves.

5.3 Industry Composition

We now examine which industries create jobs when market access grows. We first document that market access creates jobs in the non-farm traded-sectors rather than local services. To this

[42]Multiplying the mean difference in family farm and paid employees' uniqueness by the 4.9 p.p. change in paid employment associated with an average increase in market access growth gives an increase of 0.026 in uniqueness.

end, we match the Gervais and Jensen (2019) tradability measure to industries of employment in the UNPS and use the tradability of each individual's job as an outcome. Gervais and Jensen (2019) measure tradability of 969 manufacturing and service industries based on the idea that more tradable products will exhibit greater disparity between local supply and local demand. Table 3, Column 3 shows that average increase in market access leads to a 12% increase in product tradability relative to the baseline average.[43]

We also present evidence of development of industries that are directly downstream of agriculture. We measure agriculture downstreamness using the share of agricultural inputs used in a worker's industry and estimate its response to market access. This outcome reflects individuals moving to jobs in industries that are downstream of agriculture rather than changing industries' input mix, since industry input shares are fixed to match the United State's 2002 Input-Output Table.[44] As shown in Table 3, Column 4, improved market access particularly benefits the development of sectors downstream of agriculture.

Finally, there are concerns that structural transformation is driven by the mining industry, which may not benefit local residents or could even lead to a resource curse in the long run (Berman et al., 2017). We find that this is not the case in Uganda. In Table 3, Column 5, we construct a binary variable indicating whether one's job is in the mining and extraction sector and find that the propensity to switch from other jobs to mining barely changes as market access grows.

5.4 Robustness

This subsection probes identifying assumptions by applying a battery of robustness tests to our main occupation effect estimates, focusing on observations in subcounties with below-median baseline market access. First, Figure B.2 shows that our main result is robust to accounting for spatial correlation using relatively conservative district-clustered standard errors or by computing Conley (1999) standard errors. Additionally, Table A.8 shows that results are robust to using a

[43] Multiplying 0.248 by the average market access growth across subcounties (0.15) yields 0.037, which is then compared to the sample average product tradability of 0.309. We replace individual fixed effects with subcounty fixed effects as the sample is restricted to a subset of workers in non-agricultural sectors. The same applies to Column 4 of Table 3.

[44] Input-output data is provided by the World Bank (Eberhard-Ruiz et al., 2020).

model-based MA measure and alternative values for the trade elasticity (θ) rather than the literature's middle value.

Next, to address the concern that effects reflect differential trends between more and less remote areas, we re-estimate market access' effect on occupation conditional on year-interacted controls for log baseline market access and its square. This regression identifies effects by comparing individuals from subcounties that initially had similar market access, but see different market access growth because of road upgrades. Effects on paid employment in Table A.7, Column 1 are similar to baseline estimates, suggesting that results are not confounded by differential trends between more and less remote regions. A related concern is that our results could be confounded by diverging trends between urban and rural areas if urban centers and towns experience greater market access growth. We address this by controlling for an urban-area-specific time trend in Table A.7, Column 2; the result is consistent with our main results.[45]

Third, Column 3 of Table A.7 controls for local road upgrades with an indicator of an individual's subcounty containing a road that was upgraded in or before each year. Conditional on local road upgrades, market access has a slightly larger effect on employment. This suggests that our results are not driven by last-mile connections, employment found paving or maintaining national roads themselves, or unobserved trends that are correlated with precise routes of road upgrades. Notably, Column 3 also finds that local upgrades have no direct effect on occupations. This is our first piece of evidence that the road upgrades we study are primarily a technology for trade rather than commuting, and we revisit this result in Section 5.5.4. This is further supported by Column 4 of Table A.7, which shows that the estimates are robust to excluding subcounties crossed by road upgrade projects.

Fourth, Column 5 of Table A.7 demonstrates that results remain similar when market access is instrumented with a leave-out instrument that excludes nearby destinations from market access

[45]Each UNPS household is classified as urban or rural based on the population density of its enumeration area. An enumeration area is the census' smallest geographic unit; each subcounty includes multiple enumeration areas. We then calculate the subcounty-level urban population share by averaging across sampled individuals and classify subcounties with more than 50% urban households as urban regions. The results remain robust when using alternative cutoffs to define urban regions or when using the urban population share as a continuous measure. Additional results are available upon request.

(Jedwab and Storeygard, 2022).[46] This instrument isolates variation from road upgrades connecting subcounties to population centers that are far away. Accordingly, this test supports our claim that our main results do not reflect commuting or local multiplier effects.[47] This also alleviates concerns that population weights, combined with the hub-and-spoke national road network, make market access mechanically grow near urban centers where occupations might change anyway.

We also consider three confounding factors that may correlate with market access growth and affect households' job choices: other local investments, natural disasters, and electricity grid expansion. First, subcounties along the Kampala-Hoima Route may experience accelerated growth due to the discovery of oil during the study period. Similarly, subcounties along the Northern Trade Corridor, a key trade route connecting Uganda to neighboring countries, may receive additional investment support. So, we exclude subcounties along both routes in Table A.7 Column 7.[48] Second, natural disasters, such as droughts or floods, can be seen as shocks to agriculture that push households into paid work. Disasters would bias our results if they are correlated with changing market access, and Table A.7 Column 8 controls for such events.[49] Third, if road upgrades are natural places to run new power lines, market access growth may correlate with grid electricity expansion, potentially biasing our results. Since electricity is crucial for non-agricultural production and structural transformation, we control for grid access in Table A.7 Column 9.[50] All these alternative specifications yield results similar to the baseline.

To address the possibility of pre-existing trends, we test whether changes in market access from 2009 to 2019 are predicted by changes in the uptake of paid positions from 2005 to 2009.[51] To this

[46] We exclude subcounties within a 100-kilometer radius, which accounts for 13% of the average subcounty's possible destinations. Results for alternative radii are similar and available upon request.

[47] Specifically, the commuting channel and local multiplier effects would be captured by travel time or access to nearby subcounties. However, excluding this source of variation does not affect our estimates, suggesting that the key factor is connectivity to major markets farther away rather than proximity to nearby areas.

[48] To further rule out mining activities' effects, we exclude the 3% of subcounties with mineral production, processing facilities, exploration sites, or known deposits. Results remain largely unchanged and are available upon request.

[49] In particular, we construct a dummy variable that turns to one if at least one of the five natural disasters surveyed by the UNPS happens to the household.

[50] UNPS surveys household access to grid electricity, enabling us to construct a subcounty-level indicator.

[51] The occupation data from 2005 are sourced from the Uganda National Household Survey (UNHS) 2005-2006. The UNPS is an extension of the UNHS, with a subset of households initially surveyed by the UNHS continuously monitored since 2009. See Appendix C for more details.

end, we run the regression:

$$\Delta \text{Employee}_{ikr}^{2005-2009} = \beta \Delta \ln \text{MA}_k^{2009-2019} + \tau_r + X_i \gamma + \mu_{ikr}, \tag{6}$$

where $\Delta \text{Employee}_{ikr}^{2005-2009}$ is the change in the employment outcome of individual i who lived in subcounty k, region r between 2005 and 2009 and $\Delta \ln \text{MA}_k^{2009-2019}$ is the change in log market access from 2009 to 2019.[52] As this test focuses on the subset of subcounties with pre-2009 data, we first re-estimate the baseline regression using the subset of locations surveyed in the 2005-2006 Uganda National Household Survey (UNHS).[53] Table A.7 Column 10 shows that the effect for this subsample is larger than the baseline (Table 1, Column 3). In contrast, Table A.7 Column 11 finds no statistically significant relationship between pre-policy changes in paid employment and policy-induced changes in market access.

Finally, Table A.7, Column 12 shows that our results are robust to using a recentered-IV approach following Borusyak and Hull (2023). Relative to our main identification strategy, the recentered-IV accounts for the possibility that the locations of road upgrades, and how these locations interact with market access' population weights, might be correlated with unobserved shocks to occupation choice. To build recentered instruments, we randomize road project completion years among all road projects that began during our study period (regardless of their completion) 999 times, creating 999 counterfactual market access growth scenarios. We then subtract the average counterfactual value from the actual value, teasing out variation driven by transportation network structure.

5.5 Mechanisms

We now present evidence that shifts into paid work reflect better non-agricultural opportunities rather than reduced demand for farm output. First, we document growth of built-up land and

[52] We restrict the sample to remote subcounties outside Vision 2040 regions and control for a quadratic function of age, gender, education, region fixed effects, and census characteristics. The error terms are clustered at the subcounty level.

[53] See Appendix C for more details about UNHS.

household enterprises, indicating that better market access improves non-agricultural opportunities in remote areas. Next, heterogeneity tests show that workers with a comparative advantage outside agriculture are the most likely to transition into specialized off-farm employment. We then use farmgate prices and agricultural input data to rule out mechanisms such as increased competition in agricultural output markets and better access to agricultural inputs, which can push workers out of family farms without expanding alternative opportunities. Finally, we confirm that migration, commuting, and fiscal-multiplier effects are not the primary drivers of our findings.

5.5.1 Built Up Area and Household Enterprises

We estimate market access' effect on built-up area as an aggregate measure of off-farm economic activity. Table 4, Columns 1 and 2 present the results. Each column is a regression at the subcounty-year level, includes a full set of fixed effects and controls, and restricts the sample to initially remote subcounties outside Vision 2040 districts.[54] Column 1 further restricts the sample to subcounties surveyed by the UNPS, and Column 2 includes all initially remote and non-Vision 2040 subcounties. Both columns show positive effects of market access on built-up area, consistent with market access causing economic development and growth in initially remote places.

Next, we use households' non-agricultural business outcomes to support the claim that better market access indeed creates off-farm opportunities.[55] To this end, Columns 3 to 5 of Table 4 examine effects on existing household enterprises. Column 3 finds that market access increases reported profits. Columns 4 and 5 find some evidence that market access increases labor demand and wages paid, but effects are imprecisely estimated and warrant cautious interpretation.[56]

5.5.2 Heterogeneous Effects and Comparative Advantage

As improved roads integrate remote towns with big cities and expand non-agricultural opportunities, individuals with a comparative advantage outside agriculture should be most likely to change

[54] Built-up area regressions use a balanced panel of subcounties, include subcounty fixed effects, region time-varying effects, and baseline census characteristics interacted with year dummies.

[55] We lack the firm-level data necessary to test how bigger firms and formal sector firms respond.

[56] The sample is restricted to households and years in which an active non-agricultural enterprise is reported.

jobs. We examine this claim by testing for heterogeneous effects of market access using three proxies for comparative advantage in agriculture. First, we split the sample by households' baseline agricultural TFP (in Table 5, Column 1) and find that paid employment uptake is strongest among households with less productive family farms.

Next, we distinguish households that own non-customary land, a property right that provides greater land security than the customary land-holding schemes that are common in Uganda. Non-customary property rights secure land ownership and can encourage investment in advanced agricultural techniques, leading to more productive farms (Aragón et al., 2022). Table 5, Column 2 shows that owners of customary land are more likely to switch to paid positions after an improvement in market access, consistent with stronger effects on less productive farms. Finally, we show in Column 3 that households in subcounties more frequently affected by natural disasters—another proxy for comparative disadvantage in agriculture—are more responsive to market access.[57]

Together, these results suggest that individuals driving occupation shifts have a comparative advantage outside of agriculture. Effects are strongest on farms with lower productivity, poorer weather conditions, and weaker land security. We argue that this finding highlights market access' role in creating opportunities to sort into occupations that better match individual comparative advantage. However, these results do not rule out the possibility of falling agricultural prices, which would push less productive farmers into paid employment. The next section proceeds by using price data to address this critical question.

5.5.3 Competition Push vs Opportunity Pull

As emphasized in Section 2.3, two distinct factors can cause the shifts out of agriculture we associate with market access. First, market access might increase competition, reducing demand for remote households' farm output and pushing workers out of agriculture as shipping competing goods to their local markets becomes easier. This push mechanism would make anyone who

[57] Using household-level weather shocks, we calculate the probability that each subcounty experiences any of the natural disasters surveyed by the UNPS within a given year. This probability is then used to differentiate between areas exposed to high and low risks of natural disasters.

would otherwise sell agricultural products worse off. Alternatively, market access might create non-agricultural opportunities that pull workers out of agriculture and make them better off.

We use farmgate price and quantity data to distinguish these two mechanisms. If the push mechanism dominates, improving a household's market access shifts demand for their product inwards and causes movement along a supply curve; the result would be reductions in both quantity sold and price received. If the pull mechanism dominates, market access causes households to supply less agricultural output which, at a local level, can increase prices received on any agricultural products that are still sold.

Table 6 estimates market access' effects on prices and quantities of farm output sold. The unit of observation is a household-crop-time triplet; regressions include household-crop, crop-time, and region-time fixed effects; quantities are measured in kilograms sold; and price is value divided by quantity. This regression examines changes in the farmgate price of the same crop, produced by the same household, in different years. Conditional on fixed effects, price variation cannot reflect crop-level price trends or fixed differences in household farming practices that affect output quality.

Results suggest that a 1% increase in market access raises price by 0.5% and reduces quantity by 1.6%. This increase in farmgate prices is inconsistent with a scenario in which import competition makes households worse off. Instead, it is consistent with the pull mechanism, through which market access improves relative returns to non-agricultural work. Although we do not take a stand on the underlying cause of the price increase, which can reflect both growing demand for agricultural output and reduced local supply, our findings are consistent with market access improving the welfare of remote households. Columns 1 and 2 of Table A.9 supports this claim by showing that market access also reduces reliance on subsistence farming for income and reduces injury and illness rates, which proxy for household well-being.[58]

Consistent with labor shifting off farms, Table A.10 shows that market access improvements cause remotely located farming households to scale back agricultural production; they farm fewer

[58]Table A.9's Column 1 shows that market access reduces the probability of a household reporting subsistence farming as their main income source. Column 2 shows that market access also reduces the probability of an individual reporting illness or injury in the last month.

plots, fewer crops, and become less likely to spend on improved seeds and fertilizer. For example, Columns 1 and 2 of Table A.10 show that market access decreases the probability of using fertilizer or improved seed on any crops, consistent with complementarities between these inputs and farm labor. Accordingly, Columns 4 and 5 show reductions in the number of plots and crops farmed. Column 6 supports this by showing no effect on a household's probability of growing maize, which is typically commercially traded rather than consumed at home (Haggblade and Dewina, 2010).[59] Finally, as shown in Column 7, there is a noisy decline in households' agricultural TFP.

5.5.4 Other Mechanisms

This subsection considers non-trade mechanisms through which road upgrades can affect employment outcomes. First, if improved inter-city roads facilitate farmers' access to high-quality inputs or agricultural knowledge, this could enhance farm productivity, freeing up labor for non-agricultural activities. However, this is unlikely: we find that market access makes households less likely to use fertilizers and improved seeds, and that there is a noisy decline in households' agricultural TFP.

We also rule out the effects of local fiscal multipliers, whereby road construction stimulates local economies (as would any government spending) and creates new jobs. If our results mostly reflect local multipliers, then nearby road upgrades should be more important than improved national market access, and controlling for the road upgrades within a subcounty should reduce the effect of market access. Yet, as shown in Table A.7 Column 3, controlling for local road upgrades modestly increases market access's effect. In a similar vein, Column 5 of Table A.7 explicitly leverages far-away road upgrades by excluding market access growth caused by connections to nearby places. This regression should yield a smaller effect if local multipliers explain our results. However, we find results that are close to the main estimate, suggesting that local fiscal-multipliers cannot be the primary mechanism at play.

[59] Additionally, Column 3 of Table A.9 shows that there is limited change in a household's likelihood of relying on commercial agriculture as its main income source. Meanwhile, Column 4 shows only a noisy decline in agricultural participation, suggesting that households continue to engage in agricultural activities rather than fully abandoning farming.

Columns 3 and 5 of Table A.7 also contradict the view that market access captures regional roads that facilitate daily commuting to nearby towns for non-agricultural jobs. If regional commuting were important, then living near an upgraded road should matter more than the long-distance connections captured by market access. However, Column 3 shows that local road upgrades have no significant effect on employment. Moreover, the leave-out instrument used in Column 5 excludes the local connections that would be critical for commuting, but this does not change market access' effect on employment.

Finally, we test whether improved market access affects individual migration. To this end, we use the 2014 Population Census, which provides comprehensive coverage of both movers and incumbent residents.[60] The census documents individuals' current district and how long they have lived there. We use this information to assess market access' effect on in-migration. In particular, we estimate the following regression:

$$\text{In-migration}_{id} = \phi \Delta \ln \text{MA}_{d,2009-2014} + Z_i + X_d + \gamma_r + \epsilon_{id}, \tag{7}$$

where In-migration_{id} is a binary variable that equals 1 if individual i moved into district d between 2009 and 2014. $\Delta \ln \text{MA}_{d,2009-2014}$ measures the change in market access for district d from 2009 to 2014.[61] As in individual regressions, we drop Vision 2040 districts and control for individual characteristics Z_i, including a quadratic function of age, sex, and literacy. We also control for region fixed effects (γ_r) and baseline district characteristics X_d from the 2002 census. Finally, the error terms are clustered at the district level, since the treatment variable varies at this level in the regression.

As shown in Column 1 of Table A.11, districts experiencing greater market access growth do not attract additional in-migrants. In Column 2, we interact the main independent variable with the local population in 2002 to test whether market access growth causes individuals to move from less populated areas to more populated city centers. However, we find no differential effect. These

[60] We restrict the sample to those who reached 14 by 2009 and not moving to or from abroad.

[61] To compute district-level market access for each year, we take a population-weighted average of market access across all subcounties.

findings confirm that changes in market access do not affect individual migration.

6 Conclusion

Urbanization in Sub-Saharan Africa is notable for its lack of structural transformation, especially in secondary cities and more remote towns. At the same time, the region's limited transportation infrastructure is seen as a significant impediment to growth and has become its largest use of international aid (Ali et al., 2015; Graff, 2019). With these facts in mind, we provide new evidence that road upgrades, which make moving goods to big cities faster and more reliable, create opportunities for non-agricultural work that is only feasible with regular access to big-city markets.

Leveraging Uganda's substantial investment in inter-city roads and detailed individual panel data, we show that improving remote households' market access causes shifts from family farming to more specialized off-farm work. We attribute this shift to improved non-agricultural opportunities, and find that effects are strongest for those with comparative advantage outside of agriculture. At the same time, we find that households that continue farming see output prices grow, consistent with shifts to paid work being driven by improved off-farm opportunities rather than competition in agricultural markets.

These results suggest that remoteness can, at least in part, explain the surprisingly large number of farmers living in Sub-Saharan Africa's smaller cities and towns. In particular, we argue that urban job opportunities come from access to big cities. Reliable inter-city transport gives the hinterlands access to big-city markets, where they can buy and sell a diverse range of goods and services. This, in turn, creates demand for specialized off-farm work that is only profitable if output can reliably be shipped and sold in big-city markets.

These findings present several avenues for future work. First, transportation's effects may depend critically on wholesale market structure and dynamics in the trucking industry; future work could unpack the role of intermediaries and supply-chain structure using firm-to-firm transaction data. It is also possible that big cities act as entrepôts that mainly offer exposure to international markets; examining export markets' role in our findings is another interesting area for future research. And while we argue that people in peripheral areas benefit from other cities' growth through trade, rather than just migration, future work can determine whether trade and migration are substitutes or complements in addressing regional inequities. Answering these questions can shed more light on the mechanisms at play, broaden policy recommendations, and speak to how our results might generalize to other settings.

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Tables

Table 1: Workers Switch to Paid Positions

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variables	Employee				Agriculture	Non-agriculture
lnMA	0.220**	0.187*	0.325**	0.025	0.049	0.276**
	(0.094)	(0.099)	(0.135)	(0.151)	(0.082)	(0.114)
Observations	43,468	43,468	23,117	20,351	23,117	23,117
N of Clusters	462	462	263	199	263	263
Census Cntl	N	Y	Y	Y	Y	Y
Baseline MA	Full	Full	$<$ Median	$\geq$ Median	$<$ Median	$<$ Median

Each column presents results of an OLS regression including individual fixed-effects, a quadratic function of age, time-varying effects of gender and literacy, and region time-varying effects. Columns 2 to 6 add time-varying effects of census characteristics. Columns 3, 5 and 6 restrict the sample to observations with below-median baseline market access and Column 4 restricts to above-median baseline market access. Observations in Vision 2040 districts are omitted throughout and standard errors clustered by subcounty are in parentheses. *** $p<0.01$, ** $p<0.05$, and * $p<0.1$.

Table 2: Workers Quit Family Farms

	(1)	(2)	(3)
Dep. variables	Family farm	Self-employed	Unemployed
	Panel A. Full sample		
lnMA	-0.314**	0.094	0.033
	(0.124)	(0.095)	(0.101)
Observations	43,468	43,468	43,468
N of Clusters	462	462	462
	Panel B. Below-median MA in the baseline		
lnMA	-0.487**	0.143	0.019
	(0.213)	(0.156)	(0.149)
Observations	23,117	23,117	23,117
N of Clusters	263	263	263
	Panel C. Above-median MA in the baseline		
lnMA	-0.095	0.088	-0.017
	(0.167)	(0.138)	(0.156)
Observations	20,351	20,351	20,351
N of Clusters	199	199	199

Each cell presents results of an OLS regression including individual fixed-effects, a quadratic function of age, time-varying effects of gender and literacy, region time-varying effects, and time-varying effects of census characteristics. Observations in Vision 2040 districts are omitted throughout and standard errors clustered by subcounty are in parentheses. *** p<0.01, ** p<0.05, and * p<0.1.

Table 3: Job Specialization and Characteristics of Non-Agricultural Jobs

Dep. variables	(1) Uniqueness	(2) Urbanness	(3) Tradability	(4) Downstream of agriculture	(5) Mining
lnMA	0.418**	0.088**	0.248*	0.167**	-0.004
	(0.173)	(0.040)	(0.132)	(0.075)	(0.016)
Observations	17,402	17,402	2,615	3,569	23,117
N of Clusters	262	262	199	211	263

The sample is restricted to below-median baseline market access and omits Vision 2040 districts. Unemployed individuals are excluded from Columns 1 - 4 due to the absence of occupation and industry information. Columns 3 and 4 further exclude agricultural workers, as measures of tradability and downstreamness are only available for non-agricultural industries. Each column presents results of an OLS regression including a quadratic function of age, time-varying effects of gender and literacy, region time-varying effects, and time-varying effects of census characteristics. Columns 1, 2 and 5 includes individual fixed-effects, aligning with the central regression specification. Due to limited sample sizes, Columns 3 and 4 replace individual fixed effects with subcounty fixed effects. Consequently, the estimates can be interpreted as the effect of market access on the characteristics of non-agricultural jobs within a subcounty. Standard errors clustered by subcounty are in parentheses. *** p<0.01, ** p<0.05, and * p<0.1.

Table 4: Change in Built-up Land Area and Households' Enterprises

Dep. variables	(1) ln(Built-up land)	(2)	(3) ln(Profit)	(4) Num. workers	(5) ln(Wage)
lnMA	1.556***	1.200***	3.411**	1.761	8.298
	(0.588)	(0.434)	(1.642)	(2.156)	(6.880)
Observations	530	972	2,730	2,730	273
N of Clusters	265	486	197	197	57
Sample	UNPS	All	UNPS	UNPS	UNPS

The sample is restricted to below-median baseline market access and omits Vision 2040 districts. Region time-varying effects and time-varying effects of census characteristics are always controlled. Columns 1 and 2 further include subcounty fixed effects while Columns 3 to 5 include household fixed effects. Column 1 focuses on subcounties surveyed by UNPS, while Column 2 includes all subcounties. Standard errors clustered by subcounty are in parentheses. *** p<0.01, ** p<0.05, and * p<0.1.

Table 5: Heterogeneity in Transition to Paid Positions

	(1)	(2)	(3)
Dep. variable		Employee	
Agricultural TFP below median	0.467***		
	(0.177)		
Agricultural TFP above median	0.163		
	(0.186)		
Customary land owner		0.461***	
		(0.174)	
Non-customary land owner		0.233	
		(0.186)	
High risk of natural disasters			0.413**
			(0.204)
Low risk of natural disasters			0.139
			(0.163)
Observations	19,032	20,420	23,117
N of Clusters	224	224	263
p-value of test $\beta_1 = \beta_2$	0.189	0.357	0.222

Each column presents results of an OLS regression including individual fixed-effects, a quadratic function of age, time-varying effects of gender and literacy, region time-varying effects, and time-varying effects of census characteristics. Note that group-specific time fixed effects are always included, ensuring that the results are not driven by convergence or divergence among households with differing agricultural productivity. The sample is restricted to below-median baseline market access, omits Vision 2040 districts, and standard errors clustered by subcounty are in parentheses. *** p<0.01, ** p<0.05, and * p<0.1.

Table 6: Farmgate Price and Quantity Sold

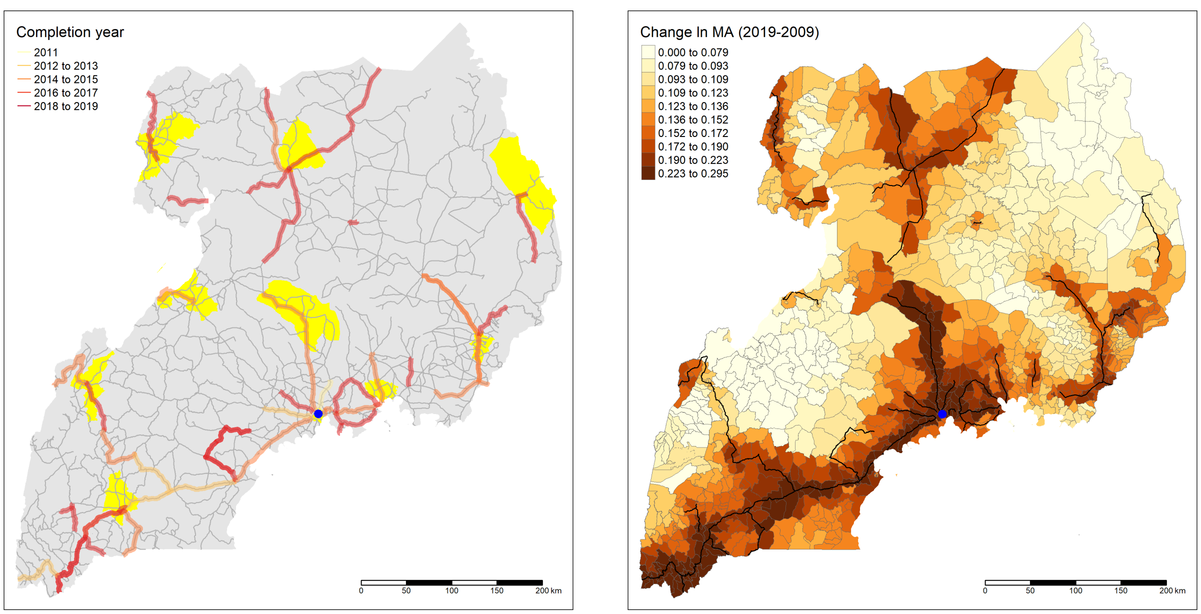
	(1)	(2)
Dep. variables	lnP	lnQ
lnMA	0.474***	-1.613*
	(0.165)	(0.828)
Observations	14,656	14,656
N of subcounties	225	225
N of crops	34	34

Each column presents results of an OLS regression including household-crop, crop-time, region-time fixed effects, and time-varying effects of baseline census characteristics. Unit of observation is a household-crop-survey time triplet. The sample is restricted to below-median baseline market access, omits Vision 2040 districts, and standard errors double-clustered by subcounty and crop are in parentheses. *** p<0.01, ** p<0.05, and * p<0.1.

Figures

Figure 1: Example Road Upgrade

Aerial image of an upgraded road in the Bukomansimbi District extracted from Google Earth.



(a) National Road Network Upgrades

(b) Long-Run Market Access Growth

Figure 2: Upgraded Roads and Market Access Growth

In panel (a), lines denote the NRN, districts containing strategic cities are yellow, and in-sample road upgrades are highlighted by completion year. In Panel (b), black lines are upgraded roads and subcounty colors denote deciles of in-sample market access changes. Kampala is shown as a blue point.

A Appendix Tables

Table A.1: UNPS Summary Statistics

	Panel A: Individual occupation characteristics					
	Family farm	Employee	Self employed	Pension	Income tax	Paid leave
Full sample	0.491	0.134	0.130	0.019	0.027	0.023
No Vision 2040	0.535	0.119	0.118	0.016	0.023	0.020
	Panel B: Occupation distribution among employees					
	Prof.	Serv.	Craft	Agri.	Mining	Manuf. & trans.
Full sample	0.253	0.154	0.076	0.214	0.004	0.064
No Vision 2040	0.258	0.124	0.075	0.259	0.004	0.060
	Panel C: Household agricultural activity					
	Pesticide	Fertilizer	Improved seed	No. plots	No. crops	Maize
Full sample	0.102	0.121	0.140	3.10 (1.75)	3.72 (1.90)	0.526
No Vision 2040	0.107	0.126	0.139	3.10 (1.74)	3.74 (1.89)	0.532
	Panel D: Demographic and socio-economic characteristics					
	Male	Age	Literate	Household size	Non-customary land	Commercial farming
Full sample	0.483	33.4 (17.2)	0.777	5.35 (2.83)	0.496	0.027
No Vision 2040	0.485	33.5 (17.4)	0.760	5.41 (2.84)	0.508	0.030

Panel A reports shares of individuals older than 14 years old for whom each occupational characteristic is true; pension, income tax, and paid leave are coded as zero for the unemployed, the self-employed, and those working on family farms. Panel B reports the share of individuals in each occupation conditional on them being employed workers. Listed occupation categories are professionals, services and sales, crafts and related trades, elementary agriculture, mining and construction, and elementary manufacturing. Panel C presents means of agricultural input and output variables across households who farm. Panel D summarises individual demographic and household socio-economic characteristics in the same sample. Individuals less than 14 years old and missing occupation characteristics are omitted from the table. Standard deviations of continuous variables are in parentheses.

Table A.2: Market Access Summary Statistics

	(1) All S.C.	(2) All S.C. UNPS-weights	(3) No Vision 2040 UNPS-weights
(1) Baseline Time to Kampala	5.42 (2.77)	4.87 (2.95)	5.08 (2.74)
(2) Δ lnTime to Kampala$^{2009-2019}$	-0.21 (0.07)	-0.22 (0.07)	-0.22 (0.07)
(3) lnMA	7.19 (0.37)	7.31 (0.41)	7.25 (0.31)
(4) Δ lnMA$^{2009-2019}$	0.15 (0.05)	0.16 (0.06)	0.15 (0.06)

Cells present means and standard deviations (in parenthesis) of levels or long-differences in log of subcounty market access or driving time to Kampala in hours. Column 1 includes all subcounties, Column 2 weights them according to the number of household-years in the UNPS sample, and Column 3 drops Vision 2040 subcounties.

Table A.3: Balance Test

Variable	(1) Low MA Growth Mean	(2) High MA Growth Mean	(3) Difference	(4) SE
Baseline market access (log)	7.162	7.172	0.008	0.019
Population (log)	11.971	12.045	0.074	0.045
% moved in $\leq$5 yrs	0.048	0.044	-0.005	0.004
% of females	0.501	0.504	0.003	0.001
% aged 18 to 64	0.779	0.786	0.007	0.002
% completed primary school	0.211	0.254	0.042	0.006
% subsistence farmers	0.805	0.755	-0.050	0.013
% for wages	0.080	0.106	0.026	0.008
% self-employed	0.038	0.053	0.015	0.003

We compare baseline-year characteristics of subcounties that experienced above- versus below-median market access growth from 2009 to 2019. The two groups exhibit comparable demographic characteristics, such as sex ratio, prime-age population share, and population size. Encouragingly, baseline market access also appears quite similar across groups. The balance test further shows that areas receiving more treatment tend to be more oriented toward non-agricultural activities and attract more educated workers. However, since our main specification always controls for these baseline demographic and economic characteristics, any location-specific trends driven by them should not confound the results. The comparison is restricted to subcounties surveyed by the UNPS and is conducted within regions, consistent with our empirical strategy, which controls for region time-varying effects throughout. A subcounty is classified as ”High” if its market access growth exceeds the regional median; otherwise, it is assigned to the ”Low” group. Baseline characteristics are drawn from the 2002 Population Census, except for market access, which is constructed from our dataset. Columns 1 and 2 report sample means, while Columns 3 and 4 present the difference between groups and the corresponding standard error. These are obtained by regressing each characteristic on a High group indicator, controlling for region fixed effects.

Table A.4: Heterogeneity in Job Switch: by Individual and Household Demographics

Dep. variable: Employee	(1)	(2)	(3)	(4)
Age≤24	0.374**			
	(0.145)			
Age 25∼45	0.368***			
	(0.133)			
Age>45	0.249*			
	(0.143)			
Male		0.504***		
		(0.182)		
Female		0.148		
		(0.125)		
Literate			0.334**	
			(0.136)	
Illiterate			0.301**	
			(0.139)	
Num. working age members				0.268
below median				(0.187)
Num. working age members				0.388**
above median				(0.191)
Observations	23,117	23,117	23,117	23,117
p-value of test $\beta_1 = \beta_2$	0.178	0.024	0.529	0.650
N of Clusters	263	263	263	263

Each column presents results of an OLS regression including individual fixed-effects, a quadratic function of age, time-varying effects of gender and literacy, region time-varying effects, and time-varying effects of census characteristics. Note that group-specific time fixed effects are always included, ensuring that the results are not driven by convergence or divergence between different demographic groups. The sample is restricted to below median baseline market access, omits Vision 2040 districts, and standard errors clustered by subcounty are in parentheses. *** p<0.01, ** p<0.05, and * p<0.1.

Table A.5: Benefits of Paid Positions

	(1)	(2)	(3)
Dep. variables	Pension	Income tax	Paid leave
lnMA	0.001	0.101**	0.121**
	(0.058)	(0.047)	(0.059)
Observations	23,081	23,090	23,086
N of Clusters	263	263	263

Each column presents results of an OLS regression including individual fixed-effects, a quadratic function of age, time-varying effects of gender and literacy, region time-varying effects, and time-varying effects of census characteristics. The sample is restricted to below median baseline market access, omits Vision 2040 districts, and standard errors clustered by subcounty are in parentheses. *** p<0.01, ** p<0.05, and * p<0.1.

Table A.6: Summary Statistics of Occupation Uniqueness and Urbanness

Occupation	Uniqueness	Urbanness
Upper rank:		
Software and applications developers and analysts	1.000	0.673
Managing directors and chief executives	1.000	0.606
Middle rank:		
General managers	0.998	0.343
Mining and construction labourers	0.998	0.222
Bottom rank:		
Primary school and early childhood teachers	0.981	0.059
Subsistence crop farmers	0.333	0.002

This table presents examples of occupations from the top, middle, and bottom tertiles based on job uniqueness. Uniqueness is calculated as one minus the share of 2002 census observations in each occupation and may appear as 1.00 due to rounding. Urbanness is calculated as Kampala's share of each occupation's national employment.

Table A.7: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variables				Employee		
lnMA	0.289**	0.289**	0.491***	0.388**	0.394**	0.338**
	(0.136)	(0.135)	(0.163)	(0.174)	(0.160)	(0.139)
Local road			-0.025			
			(0.017)			
Observations	23,117	23,117	23,117	17,557	23,117	21,877
N of Clusters	263	263	263	209	263	247
	(7)	(8)	(9)	(10)	(11)	(12)
Dep. variables		Employee			ΔEmployee$^{05-09}$	Employee
lnMA	0.344**	0.325**	0.324**	0.417***		0.254**
	(0.143)	(0.135)	(0.135)	(0.150)		(0.116)
$\Delta \ln \text{MA}^{2009\text{-}2019}$					0.121	
					(0.162)	
Observations	22,071	23,117	23,117	19,520	2,639	23,117
N of Clusters	253	263	263	157	157	263

The sample is restricted to below-median baseline market access and omits Vision 2040 districts. Columns 1 to 10 use the central regression specification, controlling for individual fixed-effects, a quadratic function of age, time-varying effects of gender and literacy, region time-varying effects, and time-varying effects of census characteristics. Column 1 adds a quadratic function of baseline year market access, interacted with time fixed effects. Column 2 controls urban-area time-varying effects, with urban area defined according to the UNPS classification (see Footnote 45 for details). Column 3 introduces a dummy indicator of local road upgrades, while Column 4 exclude subcounties crossed by road upgrades. Column 5 employs the leave-out IV method, with the corresponding first-stage regression yielding a coefficient of 0.969 and an associated F-statistic of 508.3. Column 6 excludes individuals who moved between subcounties during the study period. Column 7 exclude subcounties along the Kampala-Hoima Route and the Northern Trade Corridor. Column 8 controls for natural disaster exposure, and Column 9 for access to electricity. Column 10 limits the sample to locations surveyed in the 2005-2006 UNHS. Column 11 uses a first-difference regression, controlling for a quadratic function of age, gender, literacy, region fixed effects, and census characteristics. Column 12 employs the recentered-IV to address confounding factors instead and control for a quadratic function of age, time-varying effects of gender, literacy, and region time-varying effects. The corresponding first stage regression delivers a coefficient estimate of 1.132 and associated first stage F-statistic at 1441.7. Standard errors clustered by subcounty are in parentheses. *** p<0.01, ** p<0.05, and * p<0.1.

Table A.8: Robustness Checks: Alternative MA Specifications

Dep. variable: Employee	(1)	(2)	(3)	(4)
lnMA	0.296**	0.312**	0.325**	0.274**
	(0.129)	(0.137)	(0.130)	(0.122)
Observations	23,117	23,117	23,117	23,117
N of Clusters	263	263	263	263
Model-based MA	Yes	No	No	No
Trade flow elasticity to costs	6.0	3.8	8.2	-
Match θ to Buys et al. (2010)	No	No	No	Yes

This table presents the robustness of the main results to alternative MA specifications. Column 1 uses a model-based MA measure, while Columns 2 and 3 calibrate θ using the lower and upper bounds of trade flow-cost elasticity estimates from the literature. In Column 4, we calibrate θ to match the estimated elasticity of cross-country trade flows to travel time in Sub-Saharan Africa. Following Peng et al. (2024), we rescale the independent variables so that their standard deviations match those in the baseline specification, ensuring the comparability of coefficient magnitudes across specifications. This adjustment addresses rescaling issues discussed in Jedwab and Storeygard (2022). The rest of the regression specification follows Table 1, Column 3. Standard errors are clustered by district and reported in parentheses. *** p<0.01, ** p<0.05, and * p<0.1.

Table A.9: Additional Outcomes

Dep. variables	(1) Subsistence farming	(2) Ill/Injury	(3) Commercial agriculture	(4) Any agriculture
lnMA	-0.492**	-0.486**	0.048	-0.267
	(0.243)	(0.207)	(0.103)	(0.172)
Observations	8,716	23,074	8,716	8,812
N of Clusters	265	263	265	265

Each column presents results of an OLS regression including individual or household fixed-effects, region time-varying effects, and time-varying effects of census characteristics. For regressions at the individual level, we also control for a quadratic function of age, and time-varying effects of gender and literacy. The sample is restricted to below median baseline market access, omits Vision 2040 districts, and standard errors clustered by subcounty are in parentheses. *** p<0.01, ** p<0.05, and * p<0.1.

Table A.10: Agricultural Practice

Dep. variable	(1) Fertilizer	(2) Improved seed	(3) Pesticide	(4) ln(Plots)	(5) ln(Crops)	(6) Maize	(7) ln(TFP)
lnMA	-0.524**	-0.521***	-0.003	-1.372***	-1.306***	0.277	-0.659
	(0.234)	(0.163)	(0.165)	(0.400)	(0.376)	(0.250)	(0.695)
Observations	14,616	14,616	14,616	14,616	14,616	14,616	11,649
N of Clusters	258	258	258	258	258	258	250

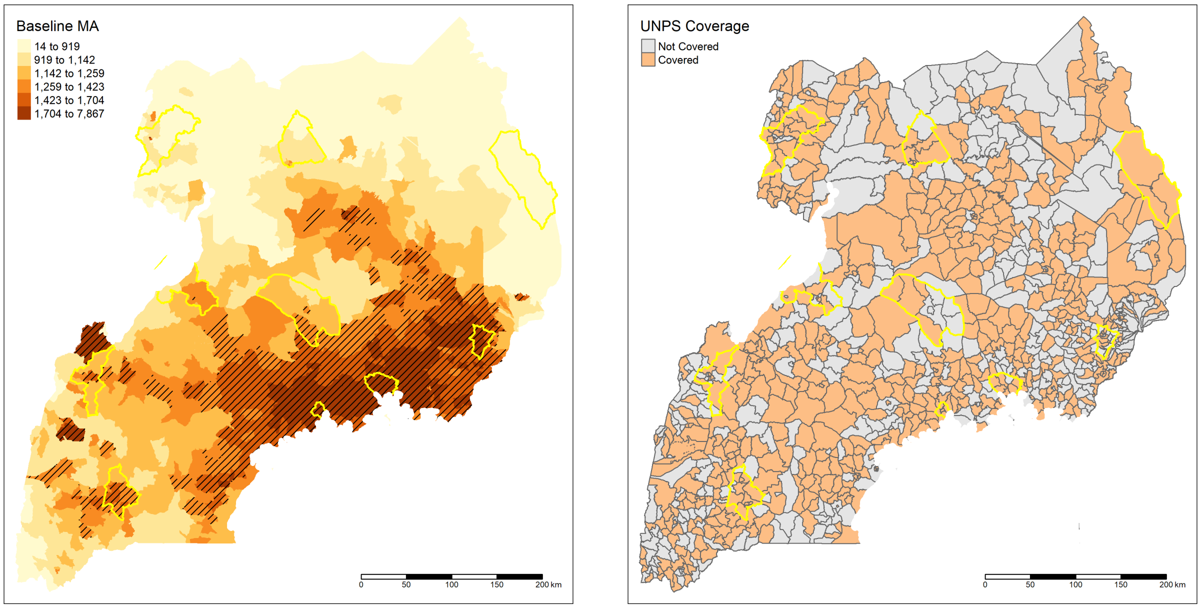
Each column presents results of an OLS regression including household fixed-effects, region time-varying effects, and time-varying effects of census characteristics. Column 7 includes fewer observations because some households can be surveyed before the harvest season, and agricultural output is required to compute TFP. The sample is restricted to below median baseline market access, omits Vision 2040 districts, and standard errors clustered by subcounty are in parentheses. *** p<0.01, ** p<0.05, and * p<0.1.

Table A.11: Probability of Moving

Dep. variables	(1) In-migration during 2009 and 2014	(2)
$\Delta \ln \mathbf{MA}_{2009 \text{ to } 2014}$	0.073	0.074
	(0.202)	(0.220)
$\Delta \ln \mathbf{MA}_{2009 \text{ to } 2014} \times \ln(pop_{2002})$		-0.003
		(0.262)
Observations	1,241,655	1,241,655
N of Clusters	106	106

Each column presents the results of an individual-level cross-sectional regression. The outcome variable is a binary indicator that equals one if an individual moved into their current district within the last five years (from 2009 to 2014). The primary independent variable is the change in market access in the individual's current district from 2009 to 2014. Column 2 includes an interaction term between the main independent variable and the logarithm of the local population in the baseline year (2002). Similar to the individual panel regressions, we exclude Vision 2040 districts from our analysis and control for individual characteristics (age as a quadratic function, gender, and literacy), region fixed effects, and local characteristics in 2002 (log of population, proportion of households that moved within five years, proportion aged 18 to 64, proportion that completed primary school, proportion identifying as female, proportion working as subsistence farmers, for wages, or in self-employment). Standard errors are clustered by district and reported in parentheses. *** p<0.01, ** p<0.05, and * p<0.1.

B Appendix Figures



(a) Remote Areas (b) Sample Coverage

Figure B.1: Remote Areas and Sample Coverage

In Panel (a), colors represent baseline market access and the non-remote sample is hatched out. In Panel (b), subcounties not covered in UNPS are grey. Districts containing strategic cities are circled yellow.

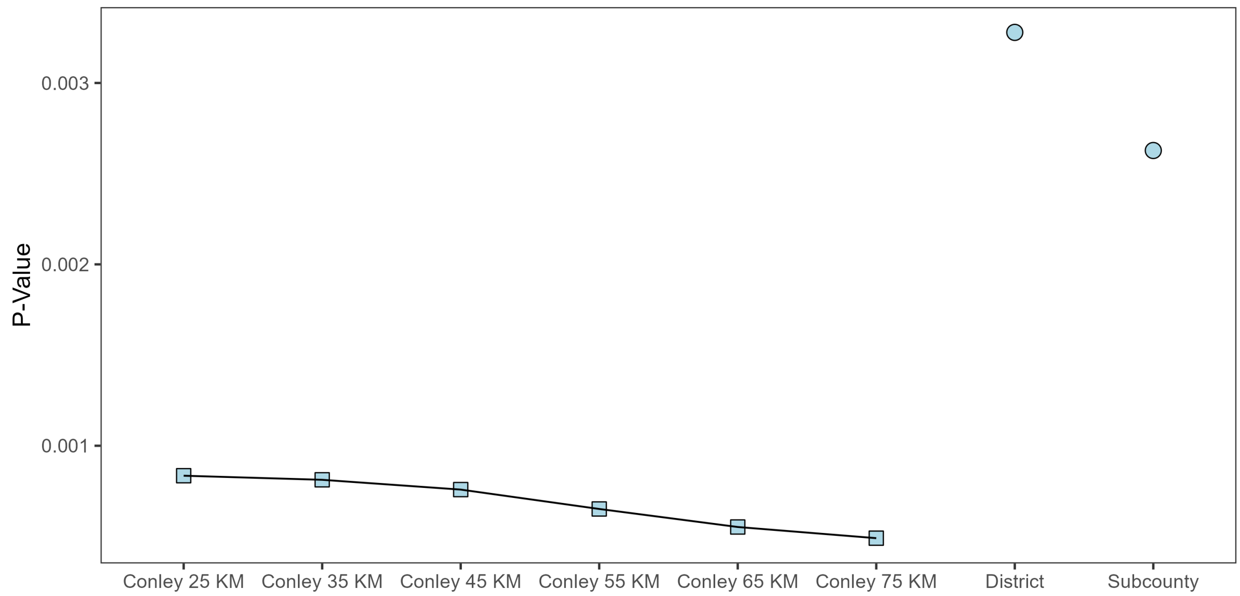


Figure B.2: Alternative Standard Errors

Points are p-values for market access' effect on remote paid employment (Table 1, Column 3) computed using alternative variance matrices. Conley p-values account for household clusters and account for cross-sectional spatial correlation using a Bartlett kernel and distance cutoffs ranging from 25km to 75km. All regressions omit controls for subcounty female and age 18 to 64 shares for computational reasons; the point estimate is 0.390.

C Data Appendix

This section lists the variables taken from the UNPS and provides their definitions as well as additional summary statistics.

- **Employee**. A dummy variable that turns to one if an individual identifies paid position as her main job.
- **Family farm**. A dummy variable that turns to one if an individual identifies working on the family farm as her main job.
- **Self-employed**. A dummy variable that turns to one if an individual identifies employer, own-account worker, apprentices, or unpaid helpers in a household business as her main job.
- **Unemployed**. A dummy variable that turns to one if an individual does not report any main job. Most individuals enter the labor force by the time they reach 14, and the UNPS surveys labor force participation for all individuals aged 10 and above.
- **Employee by sector**. Using the industry classification of paid positions, we categorize them into agricultural and non-agricultural positions.
- **Benefits of paid positions**. Paid employees are asked whether their employer contributes to a pension fund, deducts income taxes, and offers paid leave.
- **Occupation uniqueness and urbanness**. These two variables are defined using detailed occupation information associated with an individual's main job. The UNPS classifies occupations based on the ISCO classification system. Prior to 2013, the UNPS used 154 three-digit level occupations. In 2013, a new classification system with 129 three-digit level occupations was adopted. We manually cross-walk the two systems, to create 104 time-consistent occupation categories. Each occupation's uniqueness (urbanness) is calculated using data from the 2002 population census; therefore, any changes in uniqueness (urbanness) do not reflect national shifts in the occupational distribution.
- **$\triangle$Employee from 2005 to 2009**. Information on whether an individual was a paid employee in 2005 is extracted from the Uganda National Household Survey (UNHS) 2005-2006. The UNPS built upon the UNHS by following a subset of individuals originally surveyed, while also expanding to include new subcounties and households. Person IDs used in 2005 are therefore saved in the 2009 UNPS for those surveyed in both years, allowing us to link individual-level data between the two years. Significant changes in question designs and individual dropouts prevent us from including the 2005 UNHS in the main panel dataset.
- **Household enterprise performance**. Households are asked to provide information on their non-agricultural household enterprises. This information includes revenue, expenses, and the number of hired workers in the past 12 months. We compute profit using reported revenue and expenses, and we compute per-employee wages using the total wage bill and number of hired workers. We winsorize these variables at 1% and 99%.

- **Agricultural inputs, outputs, and TFP**. We observe plot-level indicators of improved seed, pesticide, and fertilizer use that we aggregate to the household level. Value and kilograms sold are reported at the plot-crop level and aggregated to the crop level. This information allows us to calculate the farmgate price for each crop, which we then winsorize at 1% and 99%.

- **Non-customary land owner**. We classify a household as a non-customary landowner if they possess any freehold, leasehold, or mailo land. These forms of land tenure are considered to provide higher levels of land security.

- **Weather shock**. A dummy variable that turns to one if a household reports one of the following natural disasters taking place in the past 12 months: drought or irregular rains, floods, landslides or erosion, unusual pests and crop disease, and unusual livestock disease. 28% of households report experiencing weather shocks in a survey year. Nearly 80% of reported natural disasters are drought or irregular rains, followed by floods (10%) and crop pests and disease (8%).

- **Risks of natural disasters**. This variable varies at the subcounty level and is defined as the likelihood of households in a subcounty experiencing any of the natural disasters surveyed by the UNPS. We use this variable to distinguish areas exposed to high versus low risks of natural disasters.

- **Illness and injuries**. A dummy variable that turns to one if an individual reports of being ill or injured in the past month.

- **Subsistence farming**. Household reports of whether subsistence farming is their primary income source. This variable indicates the importance, rather than just the presence, of agricultural employment.

- **Commercial farming**. Household reports of whether commercial farming is their primary income source.

- **Any agriculture**. A dummy variable that turns to one if a household reports any agricultural production in a given year.