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Regular article

Place-based Policies and Household Wealth in Africa ☆

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ABSTRACT

This paper provides novel evidence on the impact of a prominent place-based policy – Special Economic Zones (SEZs) – on the economic well-being of African households. Exploiting time variation in SEZ establishment on a dataset of repeated cross-sections of households in 10 African countries during 1990-2020, we show that households living near SEZs become wealthier relative to the national average after SEZ establishment. The effect accrues mostly within 10 km of SEZs, is not driven by selective migration, and is accompanied by improved access to household utilities, higher consumption of durable goods, increased educational attainment and a shift away from agricultural activities.

1. Introduction

Place-based programs – governmental policy tools used to promote local economic development and reduce regional disparities – have gained significant prominence in developing countries in recent decades (e.g., Duranton and Venables, 2021). These policies primarily aim to attract investments, create jobs, and improve the welfare of local residents. Despite their growing popularity, empirical evidence on their effectiveness in developing countries, particularly in Africa, remains limited. Prior research from developed countries (Glaeser and Gottlieb, 2008; Busso et al., 2013; Neumark and Kolko, 2010; Kline, 2010; Ham et al., 2011; Kline and Moretti, 2014a; Neumark and Simpson, 2015; Blouri and Ehrlich, 2020) cannot be directly extrapolated to the developing world. Meanwhile, existing studies on developing countries have largely focus on traditional firm-level outcomes, such as productivity, wage, and employment (e.g., Schminke and Van Biesebroeck, 2013; Wang, 2013; Lu et al., 2019; Görg and Mulyukova, 2024), with little attention paid to the welfare and distributional effects on households (e.g., Picarelli, 2016).

This paper provides novel evidence on the effects of Special Economic Zones (SEZs) – one of Africa's most widespread place-based programs – on household economic well-being. The term SEZ is used in this paper as an umbrella term that encompasses various types of zones, such as industrial parks, export processing zones, or technology parks. SEZs are designated geographical areas that provide specific incentives and regulations to foster economic growth, innovation, and job creation in the target area. According to AEZO (2021), the number of SEZs in Africa has risen from 20 in the early 1990s to more than 200 in 2021. While SEZs have become an important development tool, the question of whether SEZs have contributed to improved welfare of households remains unclear.

To estimate the local impact of SEZs, we compile a novel dataset of repeated cross-sections of households residing in various non-overlapping distance bands around SEZs in 10 African countries over

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[1] Information on population, GDP, and income status are from the World Bank, World Development Indicators 2023 and the Kiel Institute Africa Monitor (Krantz, 2023).

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the period of 1990 to 2020 using the Demographic and Health Surveys (DHS) and georeferenced SEZs data. The 10 countries – Egypt, Ethiopia, Ghana, Kenya, Mali, Mozambique, Nigeria, Tanzania, Uganda, Zambia – are a fair reflection of the policy in the African continent, as they host more than half of the SEZs and account for nearly half of the continent's population as well as its Gross Domestic Product (GDP). In terms of economic development, they range from lower-middle-income countries to low-income economies, with some still heavily reliant on agriculture and mining (e.g. Mozambique, Zambia), while others have more diversified economies (e.g. Kenya).[1]

We measure the overall economic well-being of a household with the DHS wealth index, a composite indicator based on household survey responses regarding asset ownership and access to household services. To identify the effect of SEZ policies on household wealth, we employ a difference-in-differences (DiD) framework with staggered treatment timing, leveraging time variation in the establishment of SEZs. Focusing on ultimately treated locations, our estimate compares wealth trajectories of households within the same distance band of early-versus late-treated SEZ sites, with treatment defined as the establishment of a nearby SEZ. The estimated wealth effect reflects changes relative to the national average, as the wealth index is normalized by construction. We complement this result by examining the wealth components, such as refrigerator ownership and electricity access, to illustrate absolute wealth changes.

Estimating the causal effect of SEZs on households is not trivial due to the non-random assignment of SEZs in space. If, for example, governments use SEZs to attract foreign investors, then it is more likely that relatively developed areas get treated, which may lead to an upward bias in the estimates. We address this issue in our baseline estimation both by concentrating on locations that are ultimately treated and by accounting for SEZ fixed effect to absorb any time-invariant differences between the locations, such as differences in initial conditions. However, our estimates may still be biased if, in the absence of SEZ, household wealth is trending differently in early than in late-treated locations. We provide event studies and robustness checks to show that a common trend assumption is not violated. A complementary event study, using nightlight intensity as the outcome variable, provides additional confirmation of parallel trends in pre-treatment economic activity. We also perform alternative estimations with households located farther from the SEZs as a control group, combined with propensity score matching, and find similar estimates. Finally, to address the issue of potential biases stemming from treatment effect heterogeneity across time and location, we provide estimates using the estimator of Callaway and Sant'Anna (2021) and the Extended Two-Way Fixed-Effects method proposed by Wooldridge (2021).[2]

Our baseline estimate suggests that households residing within 10 km of an SEZ experience an increase of 0.25 standard deviation of the wealth index relative to the country average after the establishment of the SEZ. This effect is economically substantial and comparable to owning a computer in Nigeria in 2008. The effect diminishes rapidly with distance, both in terms of magnitude and statistical significance, and disappears beyond 20 km. Moving beyond the average effect, we also show that the wealth effect is distributed broadly across wealth classes, as shown by Quantile Treatment Effect (QTE) estimation at the deciles of the wealth distribution (Firpo, 2007; Firpo and Pinto, 2016). The largest point estimates occur near the middle of the local wealth distribution, suggesting that SEZ policies have contributed to strengthening Africa's middle class.

Place-based policies impact population migration in two ways: new economic opportunities drive in-migration, while increasing local demand and rising prices may trigger out-migration (Ehrlich and Overman, 2020; Kline and Moretti, 2014b; Glaeser and Gottlieb, 2008). We argue that the estimated wealth effect is not merely a byproduct of changes in the local population caused by the in-migration of wealthier and the out-migration of poorer households. On a sub-sample where the migration history of households is observed, we find that SEZ establishment does not lead to any noticeable out-migration of native households, although it does increase the in-migration of relatively young households. Furthermore, the wealth effect is fairly evenly distributed across households with different migration backgrounds, with natives benefiting at least as much as the average wealth gain.

An examination of the components of the wealth index reveals that the establishment of SEZs leads to improved access to household utilities such as electricity and sanitation, enhanced housing quality, and increased household consumption of durable goods such as televisions, refrigerators, and automobiles. While these improvements can be seen as manifestations of higher household incomes, better access to electricity and sanitation facilities may also result from the physical infrastructure upgrades that often accompany SEZs developments (e.g., Farole and Akinci, 2011).

An important channel through which SEZs improve the economic well-being of households is the creation of new, better jobs (Neumark and Simpson, 2015; Moretti, 2024). Several studies have documented that place-based policies have increased employment in surrounding areas (Ham et al., 2011; Gobillon et al., 2012; Kline and Moretti, 2014a; Chaurey, 2017; Criscuolo et al., 2019; Gibbons et al., 2019; Blakeslee et al., 2022). Looking at SEZ programs, Zheng et al. (2017) and Lu et al. (2019) find that the establishment of SEZs in China positively impacts both employment levels and employment growth in designated areas. This effect is further accompanied by wage increases for local workers (Wang, 2013) and a sectoral shift towards non-agricultural activities (Zhao and Qu, 2024).

The DHS data offer limited scope to study the employment channel. We observe employment outcomes for a selective sample of household members, namely females in reproductive age with children and their male partners, and find little to no evidence that SEZ establishment increases the probability of employment in this group. However, consistent with prior literature, we document a shift from agriculture to non-agricultural occupations among household members following SEZ establishment. We interpret this result as evidence for an urbanization trend, as locals transition from agricultural occupations to non-agricultural jobs.

Further, place-based policies can affect local economic growth through their impact on investment in human capital. Technology-oriented policies have been shown to lead to higher educational attainment as they increase the returns to education (e.g., Lu et al. (2023) on Chinese SEZs or Oster and Steinberg (2013) on a new IT center in India). In contrast, export-oriented manufacturing zones can discourage students to finish their education because of increased opportunity costs (Atkin, 2016). This study demonstrates that SEZ policies in Africa are associated with improved educational outcomes, as young household members are more likely to complete secondary education following the establishment of a nearby SEZ. The positive educational response may be driven by rising household incomes or increased skilled wage premiums.

Other channels through which place-based policies can affect local economic development include consumption spillovers and demand multipliers. Employment and wage increases may generate local demand for housing, retail and other amenities, as demonstrated, e.g., by Zheng et al. (2017) in the context of China's industrial parks, which in turn fosters additional job creation through a local multiplier effect. Moretti (2010) estimates that each additional manufacturing job in a city generates 1.6 jobs in the non-tradable sector of the same city, with significantly larger effects for skilled jobs. While such developments tend to lead to higher local living costs (Busso et al., 2013), these are often offset by increased earnings, resulting in higher worker purchasing power despite rising expenses (Wang, 2013; Hornbeck and Moretti, 2024). Place-based policies can also influence local economic

[2] For further discussion see Goodman-Bacon (2021), de Chaisemartin and D'Haultfœuille (2020), Callaway and Sant'Anna (2021), and Wooldridge (2021).

development by generating environmental externalities, particularly when they are implemented alongside stricter adherence to sustainable environmental practices, contributing to improved environmental quality and higher living standards (Hua et al., 2023). Further, rising house prices can also drive the upgrading of the local housing stock, ultimately improving housing conditions (Koster and Van Ommeren, 2019). While similar mechanisms are likely contributing to the estimated wealth effect of SEZs in Africa, we cannot test their presence due to data limitations, as disaggregated data on wages, consumption, retail activity, or housing prices are not available at the required level of granularity.

Three papers are most closely related to our work. The first is the paper by Picarelli (2016), which examines the effect of Export Processing Zones (EPZs) on the level of per capita expenditure in Nicaraguan municipalities. She finds that, on average, the consumption level increased by 12% in the treated municipalities. Second, using household surveys from Cambodia, Brussevich (2024) finds that SEZs increase employment and decrease income inequality at the district level. In contrast to these studies, which use administrative areas as the unit of analysis, we use geolocated household information and assign treatment to DHS clusters, which correspond to a village or urban neighborhood and are thus more granular than districts or municipalities. This leads to more precise estimates and reduces concerns about measurement error while at the same time allows us to look at households as the unit of analysis. The third closely related work is by Shenoy (2018), who finds that infrastructure and investment subsidies increased the availability of public goods and improved household welfare. In contrast to Shenoy (2018), who looks at one state and a single tax transfer program in India, we examine 10 African countries and all SEZs incentives, which allows us to provide evidence-based policy advice at the aggregate level.

Previous studies on SEZs have primarily used firm-level data to analyze the effects of the policy. Wang (2013) and Lu et al. (2019), for instance, show that the establishment of SEZs in China increases foreign direct investments, productivity, employment and wages of manufacturing firms. The increase in wages is higher than the increase in the local cost of living, generating net benefits for workers. Schminke and Van Biesebroeck (2013) show that preferential regional policies promote exporting activity among manufacturing firms both in terms of volume and the number of destination countries. In India, Görg and Mulyukova (2024) show that the establishment of SEZs does not have any discernible effect on the productivity growth of firms, whereas Alkon (2018) documents that the program did not bring any local socio-economic development. Other related work evaluating place-based tax incentive programs is by Chaurey (2017), Hasan et al. (2021) and Blakeslee et al. (2022) who find that industrial policies significantly increase firm entry and employment.

The literature assessing the impact of SEZs in Africa is scarce and, if anything, predominantly descriptive in nature, using individual SEZs as case studies.[3] Moreover, quite a few of these studies focus on the employment generation or wage effects of SEZs, often with an emphasis on female empowerment (see, e.g. Glick and Roubaud (2006) for Madagascar and Obeng et al. (2015) for Ghana). On the contrary, we leverage detailed household-level data to explore the average effect of SEZs and how the policy effects vary along the wealth distribution. Our data, moreover, allow us to decompose the aggregate effect and look at the components of the wealth index separately. To the best of our knowledge, this is the first paper to examine the effects of African SEZs from these perspectives.

More broadly, our paper relates to the literature on the effects of place-based policies. Prior research has focused primarily on evaluating spatially targeted policies in developed countries, see, e.g. Glaeser and Gottlieb (2008), Busso et al. (2013), Kline (2010), Neumark and Simpson (2015). Kline and Moretti (2014a) find that the Tennessee Valley Authority program increased manufacturing employment and generated agglomeration economies. Neumark and Kolko (2010), on the contrary, find no employment gains following California's enterprise zone program. Ham et al. (2011) show that State Enterprise Zones have a large positive impact on the local labor market. Blouri and Ehrlich (2020) document that regional policies have contributed to welfare gains. While these studies generally suggest that such policies yield improvements in the well-being of the intended beneficiaries, the results cannot be directly extended to developing countries due to the differences in the programs and institutions.

The remainder of the paper is structured as follows. Section 2 offers a brief overview of SEZ policies in Africa, while Section 3 provides a detailed description of the data used in the empirical analysis. In Section 4, we outline the estimation framework and address key challenges related to identification. Section 5 presents the main results, including robustness checks, an analysis of migration patterns, and an exploration of effect heterogeneities and distributional impacts. Section 6 delves into the role of individual wealth components and examines the policy's effects on employment and education. Finally, Section 7 concludes.

2. Institutional background

African countries are relative latecomers in applying SEZ policies. The establishment of SEZs has only spread across the continent in the last two decades. However, by 2022, most countries had adopted active SEZ programs, resulting in approximately 203 operational SEZs and nearly 100 more under construction (Rodríguez-Pose et al., 2022). Fig. 1 illustrates the geographical distribution of all operational SEZs in Africa, with red dots indicating those in the ten countries analyzed. This growing trend reflects the adoption of SEZs by policymakers to align with the goals of the African Union's Agenda 2063 and the UN Sustainable Development Goal 9, which underscores the role of industrialization in generating employment and elevating living standards.[4]

Two main patterns account for the recent increase in SEZs in Africa. Firstly, countries with established SEZ programs such as Ghana, Egypt, Nigeria and Kenya are actively expanding and diversifying their existing SEZ portfolios. Their goal is to drive structural transformation, enhance participation in Global Value Chains (GVC), and create employment opportunities through established production networks. Secondly, emerging players are establishing new SEZs to attract Foreign Direct Investment (FDI), boost exports, and generate employment, particularly for low-skill laborers (UNCTAD, 2021). These active initiatives highlight the evolving significance of SEZs in shaping the economic landscape of countries in the continent.

The placement of SEZs is a strategic decision. This multifaceted process is influenced by various factors such as the intended purpose, ownership structure, and the economic development level of the host country (UNECA, 2022). SEZs initiated by the state typically align with local development priorities and resource availability, often concentrating in less urban areas with thriving agriculture and natural resource extraction. In contrast, SEZs proposed by private investors, subject to state approval, typically operate in the manufacturing or services sectors and are strategically located in urban and semi-urban

[3] See, for instance, Thompson (2019) for South Africa, Phiri et al. (2020) for South Africa and Zambia, Xu and Wang (2020) for Ethiopia and Zambia, or Adunbi (2019) for Nigeria.

[4] Agenda 2063 is a strategic framework that aims to achieve inclusive and sustainable growth, reflecting the Pan-African drive towards unity, self-determination, freedom, progress, and collective prosperity as expressed in the ideals of Pan-Africanism and the African Renaissance. Goals 1 and 4 of the agenda aim to improve the standards of living and promote well-being for all by creating employment opportunities and transforming economies through industrialization. For the UN Sustainable Development Goal 9, see https://sdgs.un.org/goals/goal9.

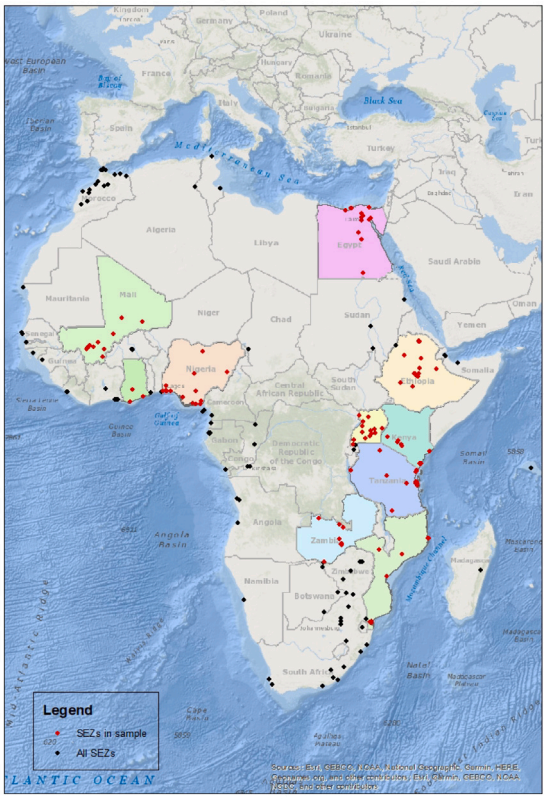

Fig. 1. Map of SEZs in Africa.

areas, often near airports and seaports. The strategic positioning serves a dual purpose: leveraging transportation hubs and natural resources to attract investors while fostering the agglomeration of firms capable of providing employment opportunities for both urban and rural populations (UNCTAD, 2021). While the location choices for SEZs in some sample countries are not explicitly stated, it is worth noting that political considerations, in addition to economic factors, may also influence these decisions.

While governments or private investors can initiate SEZs, a state entity tends to oversee their progress. This entity can be an independent SEZ authority, a relevant ministry like the Ministry of Industry, or a national Investment Promotion Agency (IPA).[5] These authorities have a range of functions, including approving applications to establish private SEZs and regulating private SEZs. The designated SEZ institutions also play a role in developing, authorizing firm operations and managing state-owned SEZs (Farole and Moberg, 2017).

Aligned with the conventional SEZ incentive structure, African SEZs offer various forms of fiscal and non-fiscal benefits such as reductions or exemptions from corporate and local taxes, alongside waivers of import duties on machinery and production inputs and outputs, as well as infrastructural upgrades (UNCTAD, 2019). Some countries also offer tax deductions for skill development programs sponsored by SEZ-based firms that target local workers, hire local workers, use local content or meet designated export targets. Additionally, few countries provide one-stop service centers within the SEZs to deliver government services to the SEZs firms (UNCTAD, 2021). Importantly, when essential infrastructure is lacking, either the state or private developers take the lead in providing the necessary amenities such as roads, electricity, drainage, and residential accommodations before the construction and commissioning of the SEZ. Notable examples of such efforts include the Kigali SEZ in Rwanda (studied in Steenbergen and Javorcik (2017)) and the Mombasa SEZ in Kenya.[6]

The majority of the SEZs in our study are oriented towards industrial activities, particularly manufacturing and assembling, agro-processing, and natural resource-intensive activities (Appendix Table A2). SEZs specialized solely in services constitute only slightly over 10% of our sample SEZs, while a significant number of SEZs are engaged in mixed (i.e. both industrial and service) activities. The service sector SEZs often function as logistics hubs providing commercial and warehousing services near transportation hubs. Overall, the sector distribution of the SEZs points to a lack of specialization in most African zones.

3. Data

One of the main challenges in evaluating the impact of SEZs on household wealth in developing countries, particularly in Africa, is the limited availability of data on households located near the zones. To overcome this limitation, we employ a novel approach by geocoding SEZ locations and spatially joining them with georeferenced household data, which to our knowledge has not been used in the literature on place-based policies before.

3.1. Households

We derive our household data from the Demographic and Health Surveys (DHS) Program administered by the United States Agency for International Development (USAID). The DHS is a comprehensive and nationally representative survey that collects data on various aspects of population, health, nutrition, demographics, and socioeconomic characteristics in developing countries. Conducted periodically, typically every five years, it provides standardized and comparable information across countries. The data is used widely by researchers, policymakers, and international organizations to inform policy decisions and guide intervention strategies.[7]

The selection of countries for our analysis is primarily guided by data availability. We begin by leveraging our SEZs data (discussed in the next section) to identify all African countries with at least one active SEZ. Subsequently, we narrow our focus to countries that have geocoded DHS with information on household asset wealth both before and after the establishment of at least one of their SEZs. We collect data from both standard DHS and Interim DHS (I-DHS) rounds. Additionally, we incorporate data from the Malaria Indicator Surveys (MIS) and the AIDS Indicator Surveys (AIS) of the DHS Program, provided they offer information on households' wealth and georeferenced location. Although the different survey types differ in their topical focus and sample size (with the Standard DHS being the largest), they are all based on a representative sample of a country's population.

Our final sample comprises 10 African countries, spanning a total of 57 DHS survey rounds. Table 1 provides a comprehensive list of the countries in our sample and the corresponding periods covered by our data. Except for Mozambique and Zambia, countries in our sample have more than five rounds of DHS, affording us extensive temporal coverage to analyze the impact of SEZs on household wealth.

Given the survey's primary focus on population, health and nutrition outcomes, there are limited socio-economic indicators at the household level, such as employment, wages, or consumption expenditure. Consequently, we primarily rely on the household asset wealth index (referred to as the wealth index) to gauge the economic status

[5] One example of an independent SEZ authority is the Ghanaian Free Zones Authority, while an example of an IPA is the Ethiopian Investment Commission.

[6] The information is from https://openjicareport.jica.go.jp/pdf/12338448.pdf. Accessed on: 22.01.2024.

[7] For a detailed description of the data visit: https://dhsprogram.com/Methodology/Survey-Types/DHS.cfm.

Table 1
DHS survey rounds by country.

Country	Survey rounds
Egypt	1995, 2000, 2003 (I-DHS), 2005, 2008, 2014
Ethiopia	2000, 2005, 2011, 2016, 2019 (I-DHS)
Ghana	1993, 1998, 2003, 2008, 2014, 2016 (MIS), 2019 (MIS)
Kenya	2003, 2008–09, 2014, 2015 (MIS), 2020 (MIS)
Mali	1995–96, 2001, 2006, 2012–13, 2015 (MIS), 2018
Mozambique	2009, 2011, 2015, 2018 (MIS)
Nigeria	1990, 2003, 2008, 2010 (MIS), 2013, 2015 (MIS), 2018
Tanzania	1999, 2003–04 (AIS), 2007–08 (AIS), 2010, 2011–2012 (AIS), 2015–16, 2017 (MIS)
Uganda	2000–01, 2006, 2009 (MIS), 2011, 2014–15 (MIS), 2016, 2018–19 (MIS)
Zambia	2007, 2013–14, 2018

Note: Standard DHS surveys unless otherwise noted. I-DHS: Interim DHS, MIS: Malaria Indicator Survey, AIS: AIDS Indicator Survey. If a survey was conducted in two consecutive years, we assign the data to the first year, unless more than two thirds of the interviews took place in the second year, in which case we assign the data to the second year.

of households. The DHS wealth index has been used extensively to measure household economic well-being in the development economics literature (e.g., von der Goltz and Barnwal, 2019; Lowes and Montero, 2021) and is particularly valuable in countries with no reliable income or expenditure data. It serves as a composite measure depicting the cumulative living standards of a household, as it is constructed using Principal Component Analysis based on household responses regarding ownership of selected assets and access to services.[8] The DHS wealth index is standardized by design to have a mean value of zero and a standard deviation of one for each survey, providing a measure of a household's wealth position relative to the national average in the survey year.[9]

3.2. SEZs

In constructing our SEZs dataset, we draw information from two primary sources; the Open Zone Map and the Africa Economic Zones Organization (AEZO), supplemented by our independent data collection.[10] Our list of SEZs covers a wide range of zone types, including industrial parks, export processing zones, technology parks, and others, all of which are grouped under the umbrella term SEZ. However, single-company zones are excluded from the analysis.[11]

We obtain information on the location, management type, land size, and operational status of SEZs from the Open Zone Map and the AEZO. Due to inconsistencies in data related to the establishment dates of some SEZs from these sources, we manually collect the year of establishment from various web-based sources, including the official websites of the SEZs and country-level institutions responsible for managing the SEZs. Similarly, we collect information on the sectoral specialization of individual SEZs and define four broad categories: Industry, Services, Mixed activities, and Not identified for those SEZs whose sectoral information is not available.[12]

A total of 114 SEZs are present across the ten countries for which the aforementioned data is available. This is reduced to 51 SEZs in the final estimation sample, determined by the availability of DHS data. The distribution of SEZs by country and time of establishment is shown in Appendix Table A1 while the distribution by management type (private, public, private-public partnership), sectoral activity, and land area size is detailed in Appendix Table A2, for both the complete set of SEZs and those in the estimation sample. The establishment dates indicate that SEZ policy is a relatively recent phenomenon in Africa, with more than half of the SEZs being established after 2010, and an additional 20% during the 2000s. The relative majority of SEZs are publicly managed, specialized in industrial activities, and are of medium in land size, ranging between 100 and 1000 hectares. The number of SEZs that are large in land size or specialized only in services is relatively low.

SEZs are important players in the local economies. Although comprehensive data on the economic importance of SEZs in Africa are not available, we have information on the number of firms and jobs for 15 of the 51 SEZs in our estimation sample. The 15 SEZs, which are all either small or medium-sized in terms of land area, host an average of 42 businesses and generate an average of 12,300 jobs.[13] This indicates that SEZs represent a considerable economic force.

3.3. Combining DHS and SEZ data

We spatially join the household and SEZ data by utilizing the GPS coordinates of both the SEZs and DHS clusters. Fig. 2 visually demonstrates that the DHS clusters in our sample countries align closely with the locations of SEZs. Since the DHS is a nationally representative survey, this spatial overlap indicates that the SEZs in our sample countries are situated in regions where most of the surveyed population live. As a result, a significant number of households are exposed to the potential impact of SEZs, enhancing the relevance of our analysis.

To construct our estimation sample, we adopt the following methodology. Initially, we define the inner area of each SEZ by drawing a circle around the centroid of the SEZ with a radius proportional to the SEZ official area size. Then, we create distance bands by incrementally extending the radius by 10 km.[14] By doing so, we establish non-overlapping distance bands around each SEZ and assign households to these bands based on the geographical coordinates of the survey clusters (villages or urban neighborhoods) where the households are located. In cases where a household's neighborhood is within the

[8] These services and assets include the source of drinking water, the type and privacy level of toilet facilities, the material composition of the main floor, walls, and roof, and type of windows in the house. Additionally, it incorporates the type of cooking fuel, household services and possessions (such as electricity, TV, radio, watch, and vehicles), agricultural land size and type of ownership, the number of owned animals, and the presence of a bank account. For more details of the wealth index construction, see https://dhsprogram.com/topics/wealth-index/.

[9] For a visual representation of the wealth index score distribution among sample households in the first and last DHS by country, please refer to Appendix Figure A1.

[10] Open Zone Map provides a comprehensive mapping of SEZs worldwide. The Adrianople Group maintains it and can be accessed at: https://www.openzonemap.com/. The AEZO Atlas is available upon subscription at: https://www.africaeconomiczones.com/aezo-atlas/.

[11] Single-firm zones differ from the zones we cover as they do not refer to a specific geographical area. They also tend to be small and employ relatively few workers (UNCTAD, 2021).

[12] Industry includes manufacturing, agro-processing and energy. Services include transport, logistics, R&D, ICT, medical and financial services.

[13] The authors would like to thank the AEZO for providing access to this information.

[14] This method takes into account the differences in the size of the SEZ areas. SEZs with larger inner areas can reach greater distances.

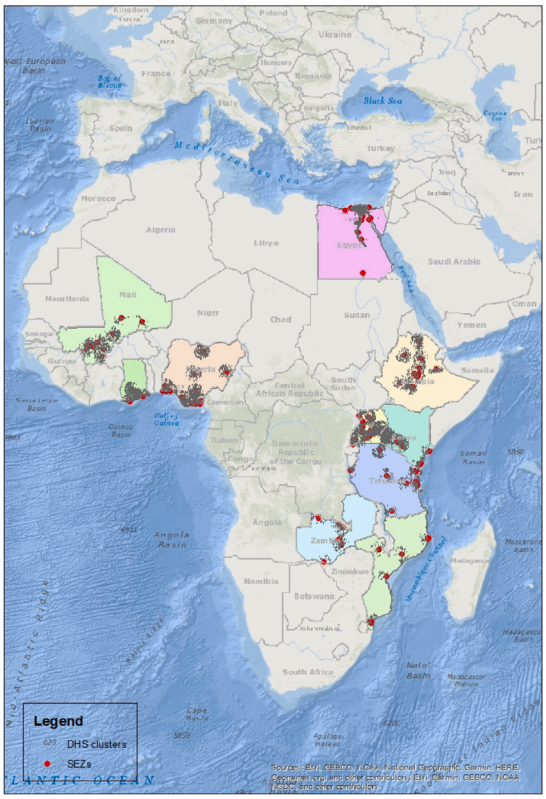

Fig. 2. The location of DHS clusters and SEZs in our sample.

vicinity of multiple SEZs, the household is assigned to the SEZ that was established first. This ensures that the surrounding areas of different SEZs do not overlap and that households are treated as soon as the first SEZ is established in their neighborhood.

In our baseline analysis, we include distance bands up to a maximum of 30 km and differentiate between households living inside or not farther than 10 km from the boundary of the SEZ inner area (within 10 km), households living 10 to 20 km and households living 20 to 30 km from the boundary of the SEZ inner area. We opted against using narrower distance bands (less than 10 km) to minimize the random measurement error arising from the geographic masking of DHS cluster coordinates.[15] The rationale for not going beyond 30 km is to approximate commuting zones within which households are likely to be affected by the presence of SEZs. This approach aligns with the existing literature demonstrating that the effect of place-based policies rapidly decays with distance (Frick et al., 2019). Further, in a robustness check where the maximum distance is extended to 120 km, we show that further distance bands are not relevant.

Our final sample comprises repeated cross-sections of households observed within the 30-km-radius circles surrounding each SEZ established from 1990 to 2020. SEZs established before the first wave of the country's DHS, i.e., outside the sample period, and households residing near them are completely excluded from the analysis. The estimation sample encompasses a total of approximately 90,000 household-year observations in three non-overlapping distance bands around 51 unique SEZs.[16]

3.4. Characteristics of SEZ locations

SEZs are not randomly located in space, and therefore their locations have distinctive characteristics even before the establishment of the SEZs. Available information suggests that African SEZs tend to be located in populous, urbanized places. About 80% of the SEZs in our sample are no more than 10 km from a populous city and roughly a third of them are located near a major airport or seaport (Appendix Figure A2).

This section examines the characteristics of SEZ locations with regard to their pre-treatment levels of development and urbanization. Table 2 reports pre-treatment means and standard deviations of the household wealth index and three variables capturing urbanization in DHS clusters falling into the various distance bands. Urban residence is a binary variable, which is 1 for urban and 0 for rural survey clusters. The built-up index quantifies the presence of built structures in a survey cluster on a scale of 0 to 1 and refers to the year 1990. Population density is the number of persons per square kilometer in a survey cluster, referring to the year 2000.[17]

The upper panel of Table 2 shows that locations chosen to host SEZs and their 10 km immediate neighborhoods tend to be richer, more urbanized and more densely populated than the more distant neighborhood already before the establishment of the SEZs. The mean of the wealth index of households living within 10 km of SEZs is 0.78, indicating that their wealth is 0.78 standard deviation higher than the country average. In contrast, households in the 10–20 km band are about the same affluent (−0.05), while households in the 20–30 km band are poorer (−0.14) than the country average. Similar decreasing patterns are observed for the degree of urbanization and population density. The SEZs are thus located close to urban centers.

The middle and lower panels of Table 2 illustrate how the aforementioned location patterns vary by the date of establishment. We distinguish between "early" SEZs, defined as SEZs established before 2015, and "late" SEZs established in 2015 or later. We observed that SEZs set up earlier are assigned to larger urban areas than SEZs established later. The neighborhoods of early SEZs exhibit a higher built-up presence and population density, which decline less sharply with distance from the SEZs. This aligns with the observation that early SEZs are, on average, closer to major airports than late SEZs (Appendix Table A3). The above characteristic is robust to using other cut-off years than 2015 to classify SEZs into early and late groups. Moreover, the pattern that later SEZs are systematically placed in less favorable locations is in line with the literature (e.g., Lu et al., 2019).

Guided by this descriptive evidence, we follow an identification strategy that compares households within the same distance bands of SEZs while taking into account differences in the initial characteristics of SEZ locations.

4. Empirical framework

We define household exposure to SEZs based on the geographical proximity to an active SEZ. Since our sample consists only of SEZs and their surrounding areas, all locations (and their households) are

[15] DHS randomly displaces the GPS latitude/longitude positions for all surveys in order to ensure that respondent confidentiality is maintained. The displacement is randomly carried out so that: (i) urban clusters contain a minimum of 0 and a maximum of 2 kilometers of error; (ii) rural clusters contain a minimum of 0 and a maximum of 5 kilometers of positional error with a further randomly-selected 1% of the rural clusters displaced a minimum of 0 and a maximum of 10 kilometers (https://dhsprogram.com/pubs/pdf/SAR7/SAR7.pdf, accessed on 07.09.2023). This geographic masking introduces classical measurement error and should not bias our estimation results.

[16] None of the SEZ surrounding areas in our baseline sample happen to extend to neighboring countries. This does not, however, apply to samples extending beyond 30 km.

[17] The urban residence variable is sourced from the DHS household surveys. The built-up index and population density are taken from the DHS Geospatial Covariate Dataset, https://spatialdata.dhsprogram.com/covariates/.

Table 2
Pre-treatment location characteristics of SEZs.

	(within 10 km)			(10–20 km)			(20–30 km)		
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.
All SEZ Locations									
Wealth index	20,706	0.78	1.09	10,663	−0.05	0.91	12,773	−0.14	0.83
Urban residence	20,706	0.76	0.43	10,663	0.29	0.45	12,773	0.21	0.41
Built-up presence index	20,441	0.21	0.24	10,368	0.07	0.17	12,447	0.09	0.25
Log population density	20,531	7.05	1.66	10,628	6.00	1.30	12,738	5.69	1.43
Early SEZ Locations (Establishment before 2015)									
Wealth index	5113	1.10	1.28	1989	0.50	1.20	3421	0.34	1.08
Urban residence	5113	0.83	0.37	1989	0.54	0.50	3421	0.40	0.49
Built-up presence index	5113	0.42	0.33	1989	0.24	0.28	3421	0.28	0.40
Log population density	5113	7.25	1.83	1989	6.44	1.84	3421	6.02	2.01
Late SEZ Locations (Establishment in 2015 or later)									
Wealth index	15,593	0.68	1.00	8674	−0.18	0.78	9352	−0.31	0.63
Urban residence	15,593	0.74	0.44	8674	0.23	0.42	9352	0.14	0.35
Built-up presence index	15,328	0.14	0.15	8379	0.03	0.09	9026	0.02	0.06
Log population density	15,418	6.98	1.60	8639	5.90	1.12	9317	5.57	1.12

Note: Summary statistics for pre-treatment household-year observations. The wealth index is specific to households, the other three indicators are specific to DHS survey clusters. Urban residence is a binary variable taking value 1 for urban and 0 for rural locations. The built-up presence index ranges on a scale of 0 to 1, with a higher value indicating more built structures. Population density is expressed as log number of persons per square kilometers. The latter two variables refer to a single year, 1990 and 2000 respectively. 1990 is a pre-treatment year for all SEZs, 2000 for all but 5 SEZs in our sample. With a cut-off year of 2015, there are 22 early and 29 late SEZs in our estimation sample.

eventually exposed to an SEZ, albeit at different points in time. We exploit this time variation in SEZ establishment to estimate the wealth effect of SEZs and follow a staggered treatment difference-in-differences approach.

Our empirical model describes the relationship between household wealth and SEZs as follows.

$$W_{hzct} = \beta \text{SEZ}_{zt} + \gamma X_{hzct} + \alpha_z + \alpha_{ct} + \varepsilon_{hzct} \quad (1)$$

The wealth index of household h living in the proximity of SEZ z in country c and observed in year t depends on whether the SEZ has already been established in t, which is captured by the time-varying binary treatment variable SEZ_{zt}. The treatment variable switches from 0 to 1 in the year when the SEZ starts to operate and remains 1 throughout the sample period.

In addition, Eq. (1) controls for a set of household-specific variables, X_{hzct}, as well as SEZ-specific and country-year specific fixed effects, α_z and α_{ct}, respectively. The household-specific variables are indicators of basic characteristics of households that capture the changing composition of households between surveys and across distance bands. They include binary variables for above-median household size, above-median age of household head, and female household head. Summary statistics of the household variables are reported in Appendix Table A4. We chose not to control for the urban status of the survey cluster of the household to avoid overcontrolling for mechanisms that may underlie the treatment effect. Agglomeration forces driven by SEZ policy often manifest as population concentration in urban clusters near SEZs, a dynamic captured by the time-varying urban indicator. Nevertheless, as shown in Appendix Table A9, our main estimate remains statistically significant, although its magnitude decreases, when the urban indicator is included as a control.

The SEZ-specific fixed effects absorb differences between SEZ locations in their initial characteristics. As shown in Section 3.4, SEZs established later are systematically placed in poorer locations than SEZs established earlier. Because our treatment variable varies by SEZ and year, the inclusion of SEZ fixed effect ensures that the treatment effect is identified from the time variation in households' relative wealth position (compared to their respective country average). Specifically, our coefficient of interest, β, captures how the relative wealth position of households near an SEZ changes after its establishment, compared to similar households near SEZs with different opening dates.

Eq. (1) also controls for country-year fixed effects, which partial out country-specific trends in the relative wealth positions of the sample households as well as any effects specific to the survey rounds. Although the wealth index as provided by the DHS program is purged of country trends due to standardization, households in our sample may still exhibit trends in relative wealth independent of SEZ, justifying the inclusion of country-year effects. However, we find that our results remain robust even when controlling only for common year effects.[18]

We estimate Eq. (1) separately for each distance band with Ordinary Least Squares (OLS) and cluster standard errors at the level of the SEZ.[19] Following von der Goltz and Barnwal (2019), we estimate without DHS sampling weights but demonstrate that the results remain robust when weights are applied.

Our empirical approach addresses the challenge of endogenous treatment assignment in two ways. First, our estimation only considers locations that eventually become treated. Specifically, it compares the wealth trajectories of earlier versus later-treated households living in the same distance band (within 10 km, between 10–20 km or between 20–30 km) around their respective SEZ. Second, the potential systematic differences between earlier and later treated locations (and their households) are addressed by the inclusion of SEZ-specific fixed effects, which absorb any time-constant differences between SEZ locations.

However, our estimates may still be biased if, in the absence of SEZ, household wealth is trending differently in early than in late-treated locations.[20] SEZ locations with varying growth dynamics may also vary in their responsiveness to the policy. To address this concern, we conduct an event study analysis to confirm the absence of pre-treatment trends and perform three types of robustness checks. First, we show that our baseline results are robust to the inclusion of SEZ-specific linear trends in the estimation. Second, we allow for differential trends based on the initial level of development of the SEZ locations. Third, we conduct placebo tests by shifting SEZ establishment dates back to periods when no SEZs were active at the sites. A statistically insignificant β estimate in the placebo tests would further validate the robustness of our findings.

[18] These results are available upon request.

[19] To minimize the impact of measurement error, we exclude from the estimation sample survey clusters with less than 15 households. Further, we winsorize the lower and upper 1% of the wealth index distribution in every survey to eliminate potential outliers in our dependent variable.

[20] Differential trends that purely arise from diverging country-specific trends are absorbed by country-year effects.

In the event study analysis, we estimate the following version of Eq. (1),

$$W_{hzct} = \sum_{k=-K}^{-1} \beta_k \text{SEZ}_{zt}^{pre(k)} + \sum_{k=1}^{L} \beta_k \text{SEZ}_{zt}^{post(k)} + \gamma X_{hzct} + \alpha_z + \alpha_{ct} + \varepsilon_{hzct}, \quad (2)$$

which estimates treatment effects for various event periods k preceding and following SEZ establishment, with the coefficient for the base period k=0 set to zero. Given the sparse nature of our data both across countries and over time, we define event periods as multi-year intervals to ensure a sufficient number of independent events underlying each estimate. We choose the period immediately preceding the year of SEZ establishment (treatment year) as the base period. In determining the length of the base period, we account for the fact that the establishment of SEZs is often preceded by planning and construction phases, which can span from less than a year to several years and potentially influence local economic activity prior to formal implementation. To inform our choice, we gathered supplementary information on the duration of this "construction" phase for the sample SEZs, finding that it can last up to six years.[21] Consequently, we define the base period as encompassing the six years preceding the treatment year, i.e., years −6 to −1.

For our main event study, we further divide the timeline into two pre-treatment periods and two post-treatment periods. Specifically, period $k = -1$ includes years −12 to −7, and period $k = -2$ captures years −13 and earlier. For the post-treatment timeline, period $k = 1$ spans years 0 to 9, while period $k = 2$ covers year 10 and beyond, which allow us to distinguish between medium-term and long-term effects.

The selection of period lengths is primarily guided by data availability, with the aim of achieving a roughly balanced distribution of observations and SEZs across event periods.[22] A persistent limitation, however, is that the longer-term treatment effect is identified from a relatively small number of early-established SEZs. Specifically, only 12 SEZs contribute to the estimation of the long-term coefficient under the baseline specification, increasing to 17 when the two post-treatment event periods are split at year 5 instead of year 9. Nevertheless, the overall findings of the event study remain qualitatively consistent across alternative post-treatment period splits.

Our empirical model (1) corresponds to a two-way fixed-effects (TWFE) model at the level of the SEZ, even though the analysis is conducted at the household level. An emerging literature subjects the TWFE model in staggered treatment settings under scrutiny and argues that, unless the treatment effect is homogeneous across time and units, the estimated average treatment effect is biased (Roth et al., 2023).[23] Several alternative estimation methods have been proposed to account for this problem, but only few can accommodate repeated cross-sectional data. A further challenge in applying some of these methods in our setting is the sparsity of the DHS data. We, therefore, opt to perform our baseline estimation with OLS, which we then complement with robustness checks using the estimation method of Callaway and Sant'Anna (2021) and the Extended TWFE estimation proposed by Wooldridge (2021).

Another approach used in the literature to deal with endogeneity concerns is to use propensity score matching. Zheng et al. (2017) and Lu et al. (2019) compare the areas surrounding SEZs to the matched areas located further away. We implement this strategy as a robustness check complementary to our baseline results. Thus, we match those households located within 10 km of an SEZ to the households located between 30 and 50 km. The rationale behind choosing this distance band is that the control group should be located further away to not be affected by the spillover effect, but not too far away to be comparable to the treated group. Treatment probabilities are estimated using covariate balancing propensity score (CBPS) due to Imai and Ratkovic (2014), which are then transformed into weights.[24] Finally, we apply these weights in the weighted version of our baseline regression.

Besides the average effect of the policy, we are also interested in the effects along the wealth distribution and examine how household wealth at different quantiles of the local wealth distribution changes following the establishment of an SEZ. Relying on empirical model (1), we perform the Quantile Treatment Effect (QTE) estimation method proposed by Firpo (2007) and Firpo and Pinto (2016) to estimate the treatment effect at each decile of the wealth distribution.[25] There are two caveats concerning QTE estimation in the current context. Firstly, it only allows us to measure changes in the wealth distribution at specific deciles but not changes in the wealth position of individual households at these deciles. Secondly, the results refer to the *local* distribution, i.e. the distribution of wealth in the specific distance band of the SEZs, and not to the wealth distribution of the country as a whole. Nevertheless, the analysis can be of policy significance because it provides insight into how broadly the benefits of SEZs are distributed across the local population and what types of households benefit the most.

5. Results

5.1. Main results

Results from estimating Eq. (1) for the mutually exclusive distance bands – within 10 km, 10–20 km, and 20–30 km – are presented in Table 3. The estimates indicate that SEZ policy has a positive overall impact on the relative wealth position of households. Following the establishment of an SEZ, the wealth of households living within 10 km rises by 0.25 standard deviation of the wealth index (significant at 1%) relative to the country's average. Converted into real terms, this increase is substantial, roughly equivalent, ceteris paribus, to owning a computer in Nigeria in 2008 or having a finished floor made of ceramic tiles in Kenya in 2014.[26] The wealth gain is also sizeable when expressed in monetary terms: in 2008, the average price of a personal computer was USD 692, which was about 60% of Nigeria's annual per-capita gross national income (GNI) in that year.[27]

[21] Examples of SEZs with construction periods spanning multiple years include the Manga-Mungassa SEZ in Beira, Mozambique, where construction began in 2012, three years before its official establishment; the Kano Free Trade Zone in Nigeria, where construction started six years prior to its establishment in 1998; and the Tema Export Processing Zone in Ghana, which underwent a four-year construction phase from 1995 to 1999.

[22] The period definitions may be adjusted in event studies conducted on smaller samples, for instance, when analyzing alternative outcome variables or employing different estimation methods.

[23] In a staggered treatment setting, the TWFE estimand is a weighted average of the different two-by-two difference-in-differences estimates comparing the individual groups treated at different times. The source of the identification issue is that some of these comparisons are incorrect, such as using an earlier-treated group as a control for a later-treated group. See, among others, Goodman-Bacon (2021), de Chaisemartin and D'Haultfœuille (2020), Callaway and Sant'Anna (2021), and Wooldridge (2021).

[24] The main advantage of CBPS is that it exploits the dual characteristics of the propensity score by estimating conditional treatment probabilities while at the same time optimizing covariate balance. This alleviates the need to check the balance of the covariates. Treatment probabilities are transformed into weights such that the treatment group receives a weight of $\frac{1}{Pr(D_i=1|X_i^0)}$ and the control group is weighted by $\frac{1}{1-Pr(D_i=1|X_i^0)}$, where X_i^0 is the set of pre-treatment covariates.

[25] We use the Stata function `rifhdreg`, which is a two-step estimation procedure relying on recentered influence functions and which allows for high-dimensional fixed effects (Rios-Avila, 2020).

[26] These comparisons are drawn from detailed information on the construction of the wealth index in each DHS survey, provided by the

Table 3
Baseline results for three distance bands around SEZs.

Depvar: Wealth index	(1) (within 10 km)	(2) (10–20 km)	(3) (20–30 km)
SEZ	0.2493***	0.1464*	−0.1293
	(0.0753)	(0.0757)	(0.1190)
N	39,537	24,452	24,764
Number of clusters	51	47	49
R-squared	0.267	0.431	0.404

Note: Results from estimating Eq. (1) with Ordinary Least Squares on the repeated cross-section of households in various distance bands around SEZs. All regressions include SEZ fixed-effects, country-year dummies and household-level control variables. Standard errors (in parentheses) are clustered at the SEZs level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

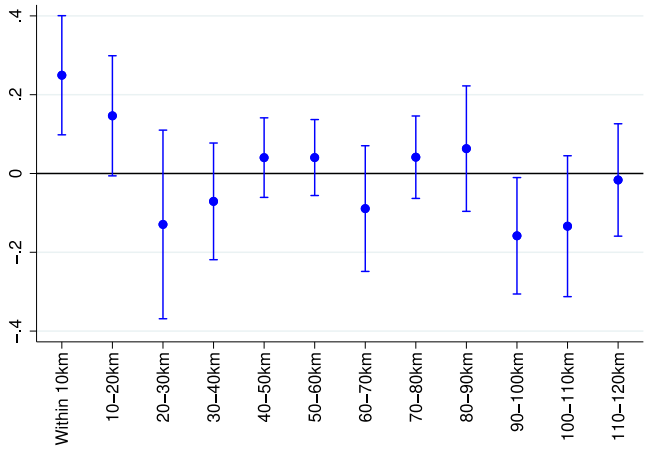

Fig. 3. Estimates for distance bands up to 120 km.
Note: Treatment effect estimates with 95% confidence intervals from estimating Eq. (1) for various distance bands up to 120 km distance from the SEZs. Households residing in a different country than the country of the SEZ are excluded from the estimation sample.

The estimated effect decays as the household's distance from the SEZ increases, with the effect within the 10–20 km band remaining positive (0.15 standard deviation) but only marginally significant, while the estimate for the 20–30 km band is statistically zero. This suggests that the impact of SEZs on relative wealth is largely confined to the immediate vicinity of SEZ locations. We find no clear evidence of treatment effects, whether positive or negative, even when extending the analysis to distances of up to 120 km from the SEZ, as illustrated in Fig. 3.[28]

The main results are qualitatively unchanged when we estimate with survey sampling weights (rescaled to account for the modified sample size), as presented in Appendix Table A7. The estimate for the within 10 km band is very similar to the unweighted estimate (showing

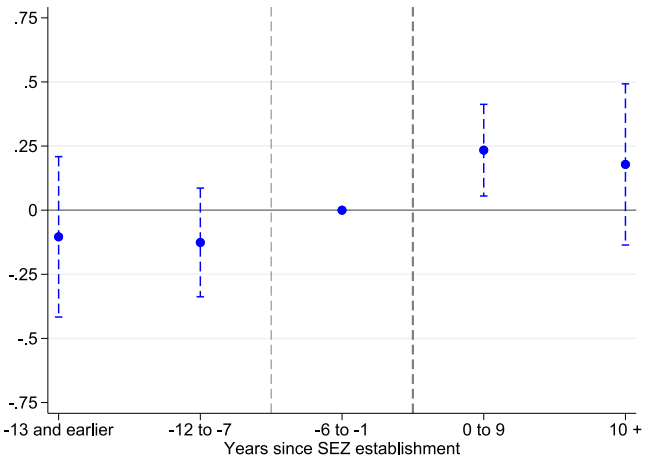

Fig. 4. Event study graph for households within 10 km.
Note: The figure plots OLS estimates with 95% confidence intervals on the sample of households living within the 10 km distance band. The coefficient for the base period (six years preceding SEZ establishment) is set to zero. Estimation with binned endpoints.

an increase of 0.26 standard deviation, significant at the 1% level). However, the distance decay is more pronounced, as the estimate falls to zero beyond 10 km.

The baseline results are also consistent with the findings of von der Goltz and Barnwal (2019), who in a similar way estimate the local wealth effects of living near an operating mine using DHS household data in 44 developing countries. Their estimate for households living within 5 km of mines is 0.26 standard deviations of the wealth index (significant at the 1% level) and is also characterized by a sharp distance decay.

The broad time pattern of the treatment effect, along with 95% confidence intervals, is illustrated in the event study graphs in Fig. 4 for the within 10 km distance band, and in Appendix Figure A3 for the 10–20 and 20–30 km distance bands. The base period, where the coefficient is set to zero, is indicated by two vertical lines in the graphs.

Focusing on the pre-treatment coefficients, no statistically significant deviation in wealth can be observed at conventional significance levels, indicating the absence of pre-trends for both the within 10 km distance band and the other two distance bands. Following treatment, the wealth of households within 10 km increases significantly relative to the base period, with a magnitude similar to the average estimate in Column (1) of Table 3. Subsequently, the estimate decreases slightly and loses statistical significance over the longer time horizon. The lack of significance of the long-term effect is likely due to the aforementioned limitation, as this coefficient can only be identified from a relatively small number of early-established SEZs.

5.2. Robustness

In this section, we perform a series of robustness checks to demonstrate that our finding of a positive treatment effect within 10 km of the SEZs is robust to the identification issues discussed in Section 4.

Controlling for differential trends. We check whether our results are driven by differential trends that are specific to SEZ locations in two ways. Firstly, we introduce SEZ-specific linear trends in Eq. (1). The trends control for the possibility that SEZ locations do not only differ in their initial level of development but also in their long-term growth paths. Secondly, we allow the treatment effect to vary with a location's initial level of development to allow for the likelihood that local economies may respond differently to the SEZ policy. We capture initial development using the built-up presence index from 1990, the first year of our sample. Since the index varies by survey cluster, we take its

DHS project at https://dhsprogram.com/topics/wealth-index/Wealth-Index-Construction.cfm. Accessed on 24.08.2023.

[27] Source of information on the average selling price of desktop personal computers worldwide is Statista, https://www.statista.com/statistics/203759/average-selling-price-of-desktop-pcs-worldwide/, accessed on 25.01.2024. The Nigerian GNI per capita was USD 1150 in 2008, as reported by the Africa Development Indicators of the World Bank (accessed on 25.01.2024).

[28] Because our outcome variable is measured relative to the country average, an increase in one location should, by construction, be accompanied by decreases in other locations. The absence of negative estimates in the more distant bands suggests that decreases in relative wealth are spatially diffuse and do not manifest in a clear or discernible pattern within the spatial structure employed in our analysis.

Table 4
Robustness results for within 10 km.

Depvar: Wealth index	SEZ-specific time trend (1)	Built index 1990 ×SEZ (2)	Control group 30–50 km (3)	Callaway-Sant'Anna (4)	Extended TWFE (5)	Placebo date −10 years (6)
SEZ	0.2098**	0.3842***	0.251***	0.8132**	0.1776**	−0.1484
	(0.0874)	(0.0777)	(0.0792)	(0.3186)	(0.0708)	(0.1628)
N	39,537	39,537	80,954	21,944	39,537	16,294
Number of clusters	51	51	51	51	51	33
R-squared	0.277	0.267	0.432	.	0.221	0.294

Note: Column (1): SEZ-specific linear time trend included in Eq. (1). Column (2): Interaction of treatment variable with the level of development of the distance band in 1990 included in Eq. (1). Column (3): Control group is household in 30–50 km distance band and CBPS matching. Column (4): Callaway and Sant'Anna estimation using the doubly robust DiD estimator based on stabilized inverse probability weighting (Sant'Anna and Zhao, 2020). Column (5): Extended Two-Way Fixed-Effects (ETWFE) estimator proposed by Wooldridge (2021). Columns (6): Results from regression (1) with placebo treatment dates backdated by 10 years. The placebo estimation sample exclude observations following the true dates of establishment. Standard errors (in parentheses) are clustered at the SEZs level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

population-weighted average value in each SEZ-specific distance band. Then, we include it in interaction with the treatment variable in Eq. (1) as an additional control variable. The estimated results in Columns (1) and (2) of Table 4 indicate that our baseline estimate remains robust to the alternative model specifications.

Distant households as control group. Our main result is also robust to using households located between 30 and 50 km from SEZs as controls, combined with covariate balancing propensity score (CBPS) matching. The covariates used for matching include the built-up presence index in 1990, log proximity to national borders, log day land surface temperature in 2000, log night land surface temperature in 2000 and log population density.[29] As Column (3) of Table 4 shows, the magnitude of the effect remains close to the baseline estimate, indicating that households located within 10 km of an SEZ experience SEZ-induced wealth increases compared to similar households located between 30 and 50 km.[30]

CS and ETWFE estimations. To illustrate that our results are robust to recent criticism of the TWFE estimation with staggered treatment, we implement the Callaway-Sant'Anna (CS) and the Extended Two-Way Fixed-Effects (ETWFE) estimation methods. The two methods overcome the shortcomings of the TWFE estimation under effect heterogeneity in different ways. The CS method computes every valid two-by-two DiD estimate between cohort groups and then aggregates them to obtain a single estimate for the average treatment effect on the treated (ATT), using only comparison groups that are not yet treated and thus avoiding incorrect comparisons. In contrast, the ETWFE estimation method, proposed by Wooldridge (2021), is a regression-based method, which accounts for effect heterogeneities by cohort and time by including a full set of dummies for all possible interactions between cohort groups and post-treatment periods.[31] Columns (4) and (5) of Table 4 present the ATT estimates. Our baseline finding of a positive and statistically significant treatment effect within 10 km of SEZs remains robust to applying the alternative estimation methods. Both estimates are positive and significant at 5% level and, considering the estimated standard errors, do not differ statistically from the baseline estimate. An event study analysis using the CS method also confirms the absence of pre-trends, as shown in Appendix Figure A4. However, we note that the CS method produces less precise estimates compared to the TWFE and ETWFE estimators, likely due to the sparse database in this context. This reduced precision is attributable to the CS estimator relying on a subset of the available sample and data variation, which limits its statistical power.[32]

Placebo treatment date. In a further robustness exercise, we estimate the baseline model with placebo dates of SEZ establishment, assuming all SEZs in our sample opened earlier than they actually did. The aim is to check for a treatment effect in the years between the placebo and actual treatment dates, during a period when no SEZs are active in the given location. Observations after the true establishment dates are excluded from the placebo estimation sample, resulting in a smaller sample size. The estimate for placebo dates set 10 years prior to the actual establishment dates are presented in Column (6) of Table 4. The placebo estimate is statistically insignificant, with a relatively large standard error. This result holds consistently, irrespective of the specific number of years selected for backdating.[33] Overall, the placebo exercise supports the validity of our baseline result, indicating that the observed positive wealth effect is driven by SEZ establishment rather than systematic pre-treatment differences between earlier- and later-treated households.

Event study for nightlight intensity. Nighttime light is widely recognized in the literature as a reliable proxy for regional economic development (e.g., Otchia and Asongu, 2021; Hu and Yao, 2022). In this robustness check, we conduct event study analysis using nightlight intensity at SEZ locations as the outcome variable to illustrate the increase in economic activity following SEZ establishment. The nightlight data used in this analysis is sourced from Li et al. (2020), which provides harmonized time series spanning 1992 to 2018.[34] Using ArcGIS, we calculate the average nightlight intensity at SEZ locations, defining an SEZ location either as the inner area of the SEZ or the area within a 10 km radius of it. Consequently, this analysis is conducted at the SEZ level rather than the DHS cluster level.[35] The nightlight values range from 0, representing no nighttime light, to 63, indicating the highest level of luminosity. Given the presence of zeros, particularly in earlier years, we apply the inverse hyperbolic sine (arcsinh) transformation to the nightlight variable (Gibson et al., 2021). This transformation approximates the natural logarithm while preserving zeros in the data.[36] A key advantage of the nightlight data over the DHS data is its full

[29] Appendix Table A8 presents summary statistics for those variables for each distance band.

[30] The estimate is very similar if we extend the group of control households to those living between 30 and 100 km of SEZs. This result is available upon request.

[31] We implement the above estimations with the user-written `csdid` and `jwdid` commands in Stata created by Fernando Rios-Avila, Pedro H. C. Sant'Anna and Brantly Callaway and Fernando Rios-Avila, respectively.

[32] Due to this limitation, it is not possible to replicate the event study with the same pre-treatment periods as shown in Fig. 4 using the CS estimator.

[33] Estimates for alternative backdating periods, other than the 10-year period, are available upon request.

[34] Data are available under: https://figshare.com/articles/dataset/Harmonization_of_DMSP_and_VIIRS_nighttime_light_data_from_1992-2018_at_the_global_scale/9828827/2 Accessed on 15.11.2024.

[35] This approach is chosen because the geomasking of DHS cluster coordinates reduces spatial accuracy and could introduce measurement error in estimating average nightlight intensity.

[36] Bellemare and Wichman (2020) show that as a rule of thumb, if the mean of the dependent variable is greater than 10, coefficients from a regression with the arcsinh-transformed dependent variable can be interpreted as semi-elasticity as is the case with log-transformed variables. In our sample, the mean of nightlight is 11.89.

time series dimension, which enables a more precise estimation of pre-treatment temporal patterns. The two event study graphs plotted in Appendix Figure A5 show no evidence of discernible trends during the pre-treatment period, including the years −6 to −1 when SEZ construction activities may take place, thereby alleviating concerns about potential pre-trends. Post-treatment coefficients reveal a gradual increase in nightlight intensity following SEZ establishment. In the SEZ inner areas, this increase becomes statistically significant at the 5% level starting in the fourth post-treatment year, while coefficients for the within 10 km area achieve significance only at the 10% level. These findings align with the expectation that most SEZ-related economic activity is concentrated within the inner areas, with more moderate contributions to increased luminosity in neighboring areas.

5.3. Population migration

The literature has documented that the effects of place-based policies are determined in part by household mobility and the elasticity of housing supply (Ehrlich and Overman, 2020; Kline and Moretti, 2014b; Glaeser and Gottlieb, 2008). By creating job opportunities, spatially targeted policies can attract workers from other regions, thereby causing in-migration. Migrants bring assets and generate income, which affects local demand for housing, raises property values, and boosts consumption, causing rents to rise. This gentrification process may subsequently prompt some local residents to out-migrate. Related empirical evidence is mixed. Busso et al. (2013) find no significant effect on in-migration or out-migration following the opening of urban empowerment zones (EZs). Reynolds and Rohlin (2015), on the other hand, provide evidence that EZs have become more attractive to high-income households. Picarelli (2016) finds no evidence of population mobility across municipalities after the opening of EPZs in Nicaragua. Several other studies have shown that location-based tax incentives do not induce labor migration and that the benefits accrue to local residents (Chaurey, 2017; Shenoy, 2018; Blakeslee et al., 2022).

This subsection examines SEZ-induced migration patterns and explores whether the policy's effects vary based on households' migration history. A key policy concern is that in-migration may constrain economic opportunities for native residents, thereby reducing the benefits they derive from the policy. Furthermore, understanding migration patterns is particularly important when estimation of the policy's effect relies on repeated cross-sectional data. If SEZs primarily attract wealthier in-migrants while prompting out-migration of less affluent households, the estimated wealth effect could be driven solely by a compositional shift towards a more affluent population.

Although the DHS database lacks comprehensive information on migration flows, some surveys include a question about the migration history of household members. Specifically, interviewed females aged 15–49 are asked how long they have lived continuously in their current place of residence. We classify a household as a "never-mover" if all its interviewed female members responded "always" to this question.[37] Among households with some migration background, we further identify those with recent migration history if all interviewed females reported to have lived in their current place of residence for less than 5 years.

The sub-sample with observed migration information is considerably smaller than the baseline sample but covers most SEZs (46 out of 51) and is broadly comparable to the baseline sample in terms of key household characteristics (Appendix Table A4). Among households with migration data, close to 40% are classified as never-movers, while approximately 20% are recent immigrants. Households within 10 km of SEZs exhibit notable differences in some key characteristics based on migration status (Appendix Table A5). Recently arrived households are, on average, smaller and have younger, more educated household heads compared to those that have lived there longer. While recent immigrants have similar wealth levels to other households with a migration background, they tend to be wealthier than never-mover households.

Table 5
Migration status as outcome.

	(1) (within 10 km)	(2) (10–20 km)	(3) (20–30 km)
Depvar: Recent migrant			
SEZ	0.0836***	−0.0543	−0.0111
	(0.0270)	(0.0503)	(0.0308)
R-squared	0.176	0.207	0.165
Depvar: Never-mover			
SEZ	−0.0247	−0.0362	−0.0547
	(0.0554)	(0.0621)	(0.0534)
R-squared	0.226	0.286	0.240
N	15,289	7,788	8,334
Number of clusters	46	44	45

Note: Estimation results from a version of Eq. (1), where the outcome variable is a binary variable for either recent migrant household or never-mover household. Population weights used as frequency weights. Recent migrants have lived in their current place of residence for less than 5 years as of the year of the DHS survey. All regressions include SEZ fixed-effects, country-year dummies and household-level control variables. Standard errors (in parentheses) are clustered at the SEZs level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

We next analyze the migration consequences of SEZ policies by using households' migration background as an outcome variable. An increase in in-migration is reflected by a growing prevalence of recent migrant households in SEZ neighborhoods, while out-migration corresponds to a declining prevalence of never-mover households. To examine these dynamics, we estimate Eq. (1) on the sub-sample where migration status is observed, using binary (0/1) dependent variables for recent migrant and never-mover households, respectively. The resulting estimates will capture changes in the likelihood of a household being a recent migrant or a never-mover following the establishment of SEZs. These regressions are weighted using population-adjusted survey weights to account for potential changes in the size of the local population over time.[38] Without applying these weights, the estimated coefficients would merely capture shifts in shares. The estimates, shown in Table 5 for all three distance bands, indicate that SEZs significantly drive in-migration within a 10 km radius. Specifically, the likelihood of a household being a recent migrant increases by 8 percentage points, a result that is statistically significant at the 1% level. An event study graph for in-migration in Appendix Figure A6 confirms that the prevalence of recent migrants rises only after SEZ establishment. In contrast, no noticeable out-migration of native households is detected. These results suggest that the wealth effect estimate within a 10 km radius of SEZs is likely influenced by in-migration but not by the out-migration of natives.

To demonstrate that the wealth effect is not solely driven by selective in-migration, but that native households also experience wealth gains, we estimate heterogeneous treatment effects by migration status

[37] This definition of a never-mover is conservative, as it accounts for the entire migration history rather than focusing solely on the period following SEZ establishment.

[38] Intuitively, the population-adjusted weights measure the number of people in the country's population that a surveyed household represents. Taking advantage of the fact that the DHS surveys are nationally representative, we create population-adjusted weights for households in each survey round by multiplying the survey weights by the county's population size in the survey year. In the calculation of the population-adjusted weights, we follow the instructions on the IPUMS-DHS website (https://www.idhsdata.org/idhs/population_weights.shtml#table1, accessed on 20.09.2024). Source of the population count data is the United Nations Population Estimates (https://population.un.org/wpp/Download/Standard/Population/, accessed on 20.09.2024).

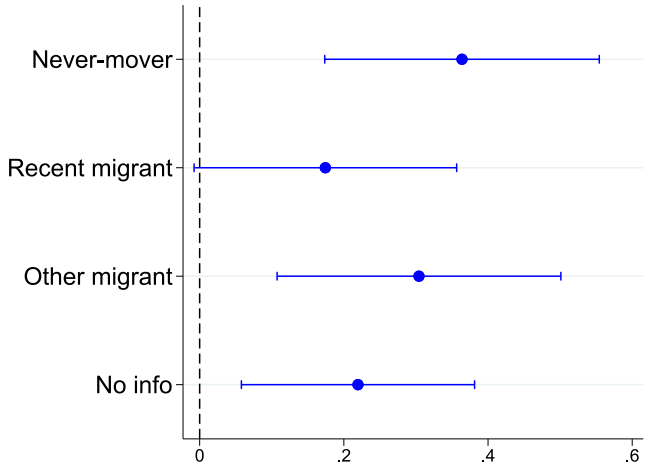

Fig. 5. Results by households' migration status within 10 km.
Note: Treatment effect estimates by migration status with 95% confidence intervals estimated from a version of Eq. (1), where the SEZ dummy and all household-level control variables are interacted with a categorical variable for the migration status of households. Recent migrants have lived in their current place of residence for less than 5 years as of the year of the DHS survey. All regressions include SEZ fixed-effects, country-year dummies and household-level control variables. Standard errors (in parentheses) are clustered at the SEZs level.

within 10 km of SEZs. Specifically, we estimate an interacted version of Eq. (1), with treatment effects for four household categories: never-mover, recent migrant, other migrant, and unknown migration background. The estimates, plotted in Fig. 5 with 95% confidence intervals, indicate that never-mover households experience wealth gains comparable to the average wealth effect. Moreover, the overlapping confidence intervals suggest no statistically significant differences in the estimated wealth effects across the four household groups. This finding, in conjunction with the absence of native out-migration, is interpreted as evidence that SEZ policies in Africa deliver substantial benefits to native households.

Interpreting the wealth effect estimates for migrant household groups is less straightforward, as they reflect both SEZ-induced shifts in the wealth composition of incoming migrants and the wealth changes these migrants experience after relocating near SEZs. However, given that recent immigrants are defined as households that arrived within the past five years – approximately the interval between two DHS survey rounds – the wealth estimate for recent migrants can be reasonably interpreted as an approximation of changes in the wealth composition of immigrants. This estimate is positive, though statistically significant only at the 10% level, suggesting that SEZs tend to attract households that are slightly more affluent than the typical population of recent immigrants in the absence of SEZ policies.

Overall, our findings on migration indicate that SEZ policies promote the in-migration of more educated and affluent households to areas within 10 km of SEZs. Importantly, this in-migration is not accompanied by native out-migration, nor does it hinder native households from benefiting from the policy.

5.4. Heterogeneity

A question of considerable policy interest is which types of SEZs generate the most significant household wealth growth. SEZs differ in various dimensions, including size, age, activity type, form of management, and locational features. We explore possible effect heterogeneities along these dimensions using the baseline sample and the interacted version of Eq. (1), where the treatment indicator and household-specific covariates are interacted with a heterogeneity variable of interest.

We consider the following categorical heterogeneity variables: the level of development of the host country, management type of SEZ, activity of SEZ, land size, age of SEZ, and distance to major airports and seaports. Host countries are classified into low-income (Mozambique, Mali, Uganda, Ethiopia) or lower-middle-income groups (Egypt, Ghana, Kenya, Nigeria, Zambia, Tanzania) based on their level of development. Management type can be Private, Public or Public–private partnership. Activity is classified into Industry, Services, Mixed activity and Not identified, where the latter includes SEZs with no available information on activity. We distinguish between three categories of SEZ size: Small (up to 100 hectares), Medium (100 to 1000 hectares) and Large (more than 1000 hectares). In terms of age, we distinguish between SEZs below the median age (10 years) and SEZs older than that.[39] Finally, we split the sample by the distances to major airports and seaports at the respective sample medians (22 km and 41 km, respectively) and create two binary variables for below-median distances.

The results, which we present in Appendix Table A10 for households within the 10 km distance band, show no clear evidence for effect heterogeneities at conventional significance levels. The signs of the interaction coefficients in Columns (1), (2) and (3) suggest that the treatment effect might be the largest for privately managed SEZs, SEZs specialized in industrial activities and SEZs located in low-income countries, but the differences are not significant statistically.[40] Estimates in Column (4) show that the entire wealth gain occurs in the first nine years of the SEZ's operation, with no further positive effect thereafter, a pattern also apparent from the event study graph in Fig. 4. Proximity to a major airport or seaport does not appear to contribute to greater household wealth gains, as shown in Columns (5) and (6). In fact, the treatment effect is larger for SEZs that are relatively far (more than 22 km) from a major airport, while distance to a major seaport does not matter.

5.5. Distributional impacts

We explore the policy's distributional effects by estimating treatment effects for each decile of the wealth distribution, using Quantile Treatment Effect (QTE) estimation (Firpo, 2007; Firpo and Pinto, 2016). Fig. 6 illustrates the results for the within 10 km distance band, showing positive treatment effects across all deciles. The point estimates exhibit some variation in magnitude across the deciles, though large standard errors render these differences statistically insignificant. The effects tend to grow larger towards the middle of the distribution, particularly between the third and sixth deciles, while they are somewhat smaller for higher wealth deciles.[41] The estimates for the lowest two deciles – though comparable to the average treatment effect – are not significant at conventional levels due to large estimation errors.

These results suggest that the benefits of SEZs on household wealth are broadly distributed, improving the relative wealth position of all wealth classes within local communities. However, the main beneficiaries of SEZ policies appear to be members of the local (lower) middle class. This aligns with recent evidence on the growing prosperity of the continent's middle class during the past decades (AfDB, 2011; Shimeles and Ncube, 2015).

[39] The age of the SEZ is a time-varying variable, defined as the year of the DHS survey minus the year of SEZ establishment plus 1.

[40] Only SEZs with unknown type of activity have a significantly lower treatment effect than industrial SEZs (the base category), which is plausible if we assume that lack of information is related to poor SEZ performance.

[41] The estimates for the upper deciles may be less reliable due to limitations of the DHS wealth index, which may not fully capture further economic improvements for wealthiest households that already own all assets included in the index.

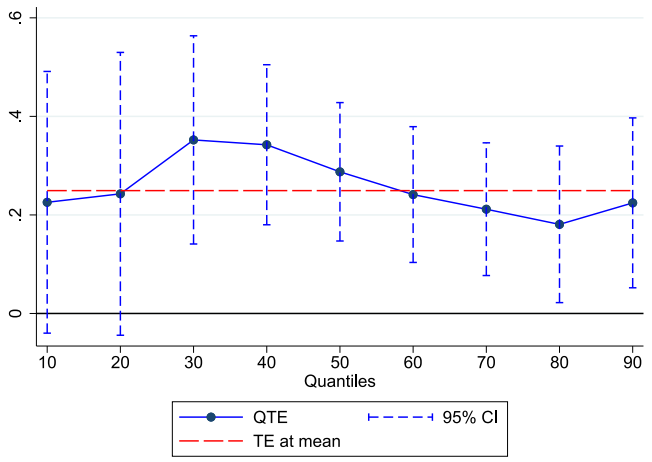

Fig. 6. Treatment effects along the wealth distribution for households within 10 km. *Note:* The figure plots treatment effect estimates for each decile of the wealth distribution with 95% confidence intervals as well as the treatment effect estimate at the mean (horizontal line). Quantile Treatment Effect estimation method, based on Eq. (1) and the sample of households in the within 10 km distance band.

6. Components and drivers of the wealth gain

In what follows, we delve into the details of the SEZ-induced wealth gains. First, we decompose the aggregate wealth measure and look separately at the components of the wealth index. Next, we present evidence for two potential channels through which SEZs might influence household wealth – employment creation and investment in human capital – using individual-level data on household members.

Summary statistics of the variables used in this section are reported in Appendix Table A4 and A6. Given that the main wealth effect occurred on households within 10 km of an SEZ, we focus on this distance band, providing additional results for the 10–20 km and 20–30 km bands in the Appendix.

6.1. Wealth components

To investigate the components of the observed increase in the wealth index following the establishment of SEZs, we estimate Eq. (1) using various outcome variables representing the components of the wealth index. The analysis is structured by grouping the relevant survey variables into three categories: (1) access to utilities, (2) ownership of household durables and housing quality, and (3) ownership of land and livestock.

6.1.1. Utility accessibility

SEZs may improve infrastructure facilities, including transportation networks and utilities like electricity, water supply and telecommunications in order to attract firms. Zeng (2015) and Wang (2013) show that robust infrastructure may result in a better business environment inside the zone, attracting new enterprises and ultimately contributing to the development of the surrounding area. Particularly, electricity has been shown as an important determinant of productivity growth of manufacturing firms (Abeberese, 2017).

Indeed, the establishment of SEZs is typically accompanied by infrastructure projects around the area. Take as an example the Lusaka South Multi-Facility Economic Zone established in 2010 in Zambia. To attract both local and foreign firms, developers have invested in 19.5 km of all-weather gravel roads, a 20 km of bituminous road, along with water supply and 33 KV electricity infrastructure. The objective is to deliver fully serviced land to potential investors and provide 99% electricity uptime.[42] Another example is Mombasa SEZ in Kenya, where the government has signed a grant agreement with the Japan International Cooperation Agency. The grant of 6,000 million yen is designated for the development of water supply facilities and rainwater drainage channels in Dongo Kundu area, where the zone is located.[43] These benefits of improved infrastructure facilities may extend to households residing in the immediate vicinity of SEZs.

We look at whether households are more likely to have access to electricity, improved sanitation and drinking water after SEZ establishment. Following the guide to DHS,[44] we define a household with improved sanitation if it has a flush toilet to a piped sewer system, a septic tank or to pit latrine, ventilated improved pit, pit latrine with slab, and composting toilet. Similarly, we create a binary variable for improved water if households have drinking water piped into dwelling/yard/plot, piped to a neighbor, public tap/standpipe, tube well or borehole, protected well/spring, rainwater, tanker truck, cart with a small tank and bottled water.[45]

Fig. 7 presents regression coefficients and their 95% confidence intervals for the individual wealth components, with utility access variables displayed in the first panel. We find that households residing within 10 km of an SEZ are significantly more likely to have electricity and improved sanitation following an SEZ establishment. Households in the 10–20 km and 20–30 km distance bands are also more likely to have improved sanitation facilities which is shown in Figures A7 and A8 in the Appendix, however, the effect decays rapidly with distance. The establishment of SEZs had no significant impact on drinking water facilities.[46]

Consequently, the increased wealth observed among households within 10 km, following SEZ establishment, can plausibly be explained by improved access to household utilities, particularly electricity and enhanced sanitation facilities.

6.1.2. Household durables and housing quality

The SEZ-induced improvement in households' material well-being can be reflected in increased consumption of durable goods and improved housing quality. Higher household incomes – whether directly from SEZ employment or indirectly through local demand multipliers – can enable households to acquire essential household durables and invest in superior materials for housing construction, further contributing to improving housing quality.

To investigate this aspect, we assess whether the establishment of SEZs has an impact on household ownership of durable goods, such as televisions, refrigerators, telephones, mobile, computers, bicycles and cars, and the materials used for floors, roofs and walls of the dwelling. In defining the housing quality, we follow Tusting et al. (2017). A finished floor is a binary indicator of whether a floor is made of parquet/polished wood, vinyl, asphalt strips, ceramic tiles, cement, and carpet. A finished roof is a dummy variable, taking one if a roof is made of metal, wood, calamine or cement fiber, ceramic tiles, cement, and roofing shingles. Lastly, the finished wall dummy equals one if a

[42] The information is taken from the official website https://www.lsmfez.co.zm/infrastructure-developments/. Accessed on: 23.12.2023.

[43] The information is from the official website https://www.jica.go.jp/Resource/english/news/press/2022/20220621_21_en.html. Accessed on: 23.12.2023.

[44] Available at: https://dhsprogram.com/data/Guide-to-DHS-Statistics/Type_of_Sanitation_Facility.htm. Accessed on: 19.06.2023.

[45] The classification of household drinking water is available at: https://dhsprogram.com/data/Guide-to-DHS-Statistics/Household_Drinking_Water.htm. Accessed on: 19.06.2023.

[46] Our findings on no effect on improved drinking water are supported when looking at the time needed to get to the water source. There, we also do not find any statistically significant effect, even though all coefficients have a negative sign. Results are available upon request.

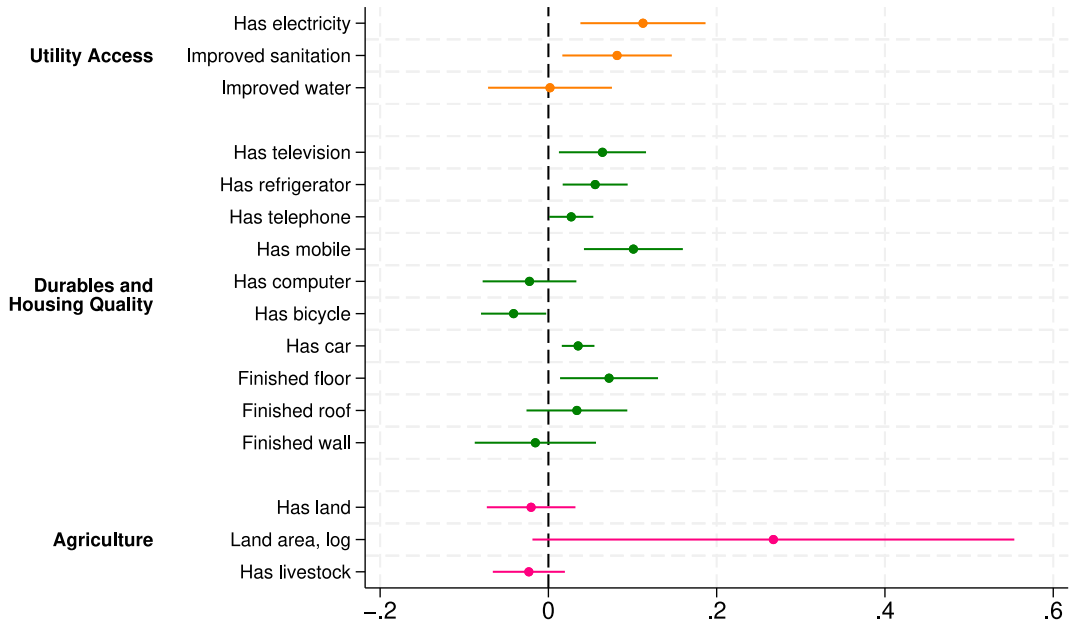

Fig. 7. Decomposition of aggregate wealth index for households within 10 km.
Note: The figure plots OLS estimates with 95% confidence intervals from estimating Eq. (1) on the sample of households living in the within 10 km distance band. Land area is winsorized at the top 95 percentile. All regressions include SEZ fixed-effect, country-year dummies and household-level control variables for the household size and the age and gender of the household head.

wall is made of cement, stone with lime, bricks, cement blocks, covered adobe, and wood planks or shingles.

Estimated coefficients depicted in the second panel of Fig. 7 indicate that households within 10 km of an SEZ are significantly more likely to possess a television, refrigerator, telephone, and mobile phone following SEZ establishment. However, we find no significant effect on owning a computer, suggesting that SEZs potentially create low-skilled manual jobs. Moreover, households are switching from owning a bicycle to owning a car, indicating growing urbanization. Whereas the main beneficiaries are households within 10 km of SEZs, households between 10 and 20 km also benefit in terms of television and mobile phone ownership. There is no effect beyond 20 km, as is shown in Appendix Figure A8. Looking at the housing quality, households within 10 km are significantly more likely to have finished floors, whereas there is no effect on the roof or wall quality.

6.1.3. Land and livestock

A frequently discussed aspect of SEZ development in the literature pertains to land acquisition. SEZ developers require land for their projects, and taking farming land may displace households dependent on agriculture, potentially leading to unemployment and a reduction in the well-being of local farmers (Levien, 2013; Aggarwal and Kokko, 2022). On the other hand, SEZs may provide jobs that can potentially decrease household reliance on agricultural activities. Further, SEZs development may increase housing and land prices, further exacerbating the income divide (Reynolds and Rohlin, 2015).

To explore these dimensions, we examine whether the establishment of SEZs is associated with subsequent changes in the probability of owning land, the land area owned (expressed in log hectares), and the likelihood of having livestock. The third panel of Fig. 7 plots the coefficient estimates for these outcomes, none of which are statistically significant at the 5% level. Specifically, we find no clear association between SEZ establishment and land or livestock ownership within a 10 km radius. However, the coefficient on land area owned is positive and marginally significant at the 10% level. An increase in household-owned land area is not necessarily inconsistent with SEZs occupying local land, as households may expand their holdings by acquiring previously underutilized areas or land farther from the SEZ.

Taken together, these results indicate that, contrary to claims in the literature, SEZ development has not led to widespread land acquisitions, dispossessing local households from their lands. Instead, the policy may have enabled some households to expand their landholdings, aligning with the observed wealth effects.

6.2. Employment and occupation

One direct mechanism through which the establishment of SEZs can influence household wealth is by creating more and better-paying jobs, thereby increasing household income. Building on this premise, we investigate the employment and occupational implications of SEZ establishment using individual-level data on female and male household members. However, analyzing employment effects with DHS data has notable limitations. While the survey is representative at the household level, the individual-level sample is selective, including only women aged 15–49 with children and their male partners or husbands. This excludes males without such female partners, as well as older women and childless women. Additionally, DHS employment data do not distinguish between formal and informal employment — an important aspect of job quality.

Employment-related data are based on a survey question asking whether an individual worked at any time during the 12 months preceding the survey. Another variable provides information on the broadly defined sector of employment. Using these variables, we construct binary outcome measures for working in the past 12 months, agricultural occupation, and non-agricultural occupation. Non-agricultural activities primarily include sales, services, manual labor, and professional, technical, and managerial positions. Descriptive statistics, shown in Appendix Table A6, indicate that the average probability of working in the 12 months preceding the survey is 60% for women and 87% for their male partners within 10 km of SEZs. Employment in agricultural occupations accounts for approximately 12% of jobs.

To estimate the employment effects, we run individual-level versions of Eq. (1) using the three outcome variables for both female and male samples. In addition to household-level controls, we include a

Table 6
Results for employment and occupation within 10 km.

	(1) Worked in the last 12 months	(2) Occupation in agriculture	(3) Occupation in non-agriculture
Female Outcome			
SEZ	−0.0222	−0.0573***	0.0311
	(0.0323)	(0.0195)	(0.0361)
R-squared	0.209	0.217	0.168
N	23,744	23,744	23,744
Number of clusters	50	50	50
Husband/Partner Outcome			
SEZ	0.0158*	−0.0196	0.0353
	(0.0092)	(0.0285)	(0.0279)
R-squared	0.858	0.118	0.464
N	19,317	19,317	19,317
Number of clusters	49	49	49

Note: Results from OLS estimation on individual-level female and male samples of versions of Eq. (1). All regressions include SEZ fixed-effects, country-year dummies, household-level control variables and female-specific control variables. Standard errors (in parentheses) are clustered at the SEZs level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

set of female-specific characteristics as control variables.[47] The main results are summarized in Table 6, with estimates for females shown in the upper panel and males in the lower panel. Heterogeneous effects based on skill level and migration background are presented in Appendix Table A11, where having high skill is defined as having completed secondary education.

Overall, we find little to no evidence that SEZ establishment increases the probability of employment among sampled individuals within a 10 km radius, as shown by the estimates in Column (1) of Table 6. The point estimate for females is statistically insignificant, while the estimate for males is positive but only marginally significant, reflecting a modest 1.58 percentage-point increase (significant at the 10% level). This finding largely persists when the estimates are disaggregated by skill or migration status, as shown in Column (1) of Appendix Table A11.

However, the above findings should be interpreted with caution due to the selective nature of the individual-level sample. Notably, the sample includes only females who are mothers, potentially overlooking employment effects on other demographic groups, such as young single women. Prior research highlights that this group often constitutes key beneficiaries of SEZ-related job creation in developing countries (Glick and Roubaud, 2006; Brussevich, 2024).

A more robust finding emerges regarding sectoral shifts among female household members. Specifically, SEZ establishment is associated with a significant move away from agricultural occupations, as evidenced by the negative and significant estimate in Column (2) of Table 6 and the event study graph in Appendix Figure A9. The estimate indicates a 6 percentage-point decrease in the probability of females working in agriculture following SEZ establishment. Decomposing this estimate by educational attainment reveals that the effect is statistically significant only among unskilled females, underscoring the limited role of agriculture in skilled employment. Additionally, heterogeneous estimates by migration background indicate that this sectoral shift is observed among both never-mover and migrant females, suggesting that the effect is not solely driven by the in-migration of non-agricultural workers.

For unskilled males, a similar sectoral shift appears to occur, marked by a significant decline in agricultural occupations at the 5% level, alongside a significant increase in non-agricultural employment at the 1% level (Columns (2) and (3) in Appendix Table A11). However, event study analyses for these outcomes suggest that these trends may have already been present among unskilled males prior to SEZ establishment, raising questions about the extent to which the observed shift can be attributed to SEZs.[48]

Taken together, these findings provide no strong evidence that SEZ establishment leads to net job creation in our sample of mothers and their male partners. Nevertheless, the results highlight an important sectoral shift away from agricultural employment, which may signal improvements in both pay and job quality as households transition to non-agricultural occupations.

6.3. Educational implications

According to Lu et al. (2023), SEZs can influence educational outcomes through three primary channels. The first is the income channel, whereby increased household income from SEZ-related opportunities may encourage parents to invest more in their children's education. The second is the job opportunity channel, suggesting that access to low-skill jobs may reduce high school enrollment, while the prospect of high-skill jobs could incentivize continued education. The third is the wage premium channel, which posits that SEZs create higher wage premiums for high-skill jobs, motivating individuals to pursue further education.

Education decisions depend on weighing the long-term benefits of schooling against its short-term costs. SEZs focused on low-skill industries may reduce incentives to pursue further education, as individuals are drawn to immediate employment opportunities. Conversely, technology-oriented SEZs often generate higher returns for educated workers, encouraging longer stays in education. For example, Oster and Steinberg (2013) find that the establishment of an Information Technology Center in India led to a 5% increase in primary school enrollment.

To assess whether the wealth gains associated with SEZ establishment translate into higher educational attainment among nearby residents, we examine the educational outcomes of female household members and their male partners living within 10 km of SEZs. Our primary outcome variable for educational attainment is a binary indicator for completing at least secondary education. Descriptive statistics reveal that females in our sample are, on average, less likely than their male partners to have completed secondary education (37% vs. 48%, as shown in Appendix Table A6).

As detailed in Section 5.3, SEZ establishment often induces in-migration, with recently arrived households having, on average, a more highly educated head than native households (Appendix Table A5). To disentangle the effects of skilled in-migration from changes

[47] These include age, age squared, marital status, pregnancy status, and the number of children under 5 years old. However, the results remain robust even without the inclusion of individual-specific controls.

[48] The respective event study results are available upon request.

Table 7
Female educational attainment within 10 km.

Dep.var: At least secondary education	(1)	(2)	(3)
SEZ	0.0848***		
	(0.0254)		
SEZ × Never-mover		0.0789**	
		(0.0314)	
SEZ × Migrant		0.0896***	
		(0.0300)	
SEZ × Never-mover × Age < 25			0.1717***
			(0.0329)
SEZ × Never-mover × Age ≥ 25			0.0491
			(0.0352)
SEZ × Migrant × Age < 25			0.1344***
			(0.0310)
SEZ × Migrant × Age ≥ 25			0.0717*
			(0.0362)
R-squared	0.199	0.201	0.202
N	18,222	18,222	18,222
Number of clusters	46	46	46

Note: Results from OLS estimation on the individual-level female subsample, where migration information is observed. Versions of Eq. (1) interacted with never-mover status and a binary indicator of being less than 25 years old. All regressions include SEZ fixed-effects, country-year dummies, household-level control variables and female-specific control variables. Standard errors (in parentheses) are clustered at the SEZs level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8
Husband/partner educational attainment within 10 km.

Dep.var: At least secondary education	(1)	(2)	(3)
SEZ	0.0743***		
	(0.0213)		
SEZ × Never-mover		0.0892***	
		(0.0312)	
SEZ × Migrant		0.0685**	
		(0.0261)	
SEZ × Never-mover × Age < 30			0.1349**
			(0.0562)
SEZ × Never-mover × Age ≥ 30			0.1006***
			(0.0337)
SEZ × Migrant × Age < 30			0.0946**
			(0.0417)
SEZ × Migrant × Age ≥ 30			0.0823***
			(0.0291)
R-squared	0.180	0.182	0.187
N	14,923	14,923	13,517
Number of clusters	46	46	46

Note: Results from OLS estimation on the individual-level husband/partner subsample, where migration information is observed. Versions of Eq. (1) interacted with never-mover status and a binary indicator of being less than 30 years old. All regressions include SEZ fixed-effects, country-year dummies, household-level control variables and female-specific control variables. Standard errors (in parentheses) are clustered at the SEZs level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

in locals' human capital accumulation, we focus our analysis on the DHS subsample where migration information is available and estimate separate treatment effects for individuals classified as never-movers and migrants. Females and their male partners are defined as never-movers if they belong to never-mover households.[49] All other individuals are classified as migrants.

Since younger individuals are more likely to adjust their educational decisions in response to policy changes, we further estimate treatment effects separately for young and old age groups. Gender-specific cutoff ages (25 for females and 30 for males) are used, reflecting the ten-year average age gap between male partners and females (Appendix Table A6) and to avoid small sample sizes in the young male subgroup. Notably, the male results remain robust when using smaller cutoff ages.[50]

The estimation results are presented in Tables 7 and 8 for females and their husbands/partners, respectively. Complementary results for the years of education of husbands/partners (an outcome variable not available for females) are provided in Appendix Table A12. We find that SEZ establishment is associated with a statistically significant improvement in the educational attainment of both females and males residing within 10 km of an SEZ. The coefficient estimates in Column (1) indicate increases in the probability of completing secondary education by 8.5 percentage points for females and 7.4 percentage points for males, both significant at the 1% level.

These results remain consistent in magnitude and significance for individuals in both never-mover and migrant households (Column (2)), which suggests that, beyond the effects of skilled in-migration, there is evidence of human capital accumulation among the local population.

When the coefficients are further disaggregated by age group in Column (3) of Table 7, the results indicate that the positive effect observed for never-mover females is driven predominantly by younger females under the age of 25, while the coefficient for older never-mover females is not statistically significant. In contrast, for migrant females, we observe a marginally significant positive coefficient for the older age group as well. This aligns with our expectation that educational improvements in older cohorts are primarily attributable to in-migration.

In contrast, the decomposed estimates for males in Column (3) of Table 8 are positive and statistically significant across all age groups and migration backgrounds, including older males in never-mover households. This raises concerns about potential upward selection bias in the male results, as more highly educated young females may preferentially select highly educated (and perhaps older) males as partners. Consequently, we consider the findings for male partners less suitable for isolating the effects of the human capital accumulation channel.[51]

Overall, the findings on education provide evidence that SEZ policies in Africa contribute to improving educational attainment in SEZ neighborhoods. These improvements are driven not only by skilled in-migration but also by enabling and incentivizing local residents – particularly young females – to achieve higher levels of education.

7. Conclusion

Many countries across Africa have implemented varied SEZs over the last two decades to promote export diversification, GVC participation, and local economic development by using incentive packages to attract firms into the desired location. Despite the wide spread of SEZs in the continent, there is limited empirical evidence on the economic implications of the policy, particularly at the micro level, largely due to data unavailability.

Using geocoded DHS data, we demonstrate that the establishment of SEZs in Africa contributes to the growth of asset wealth of households living within 10 km of the SEZs. While the benefits of SEZs are broadly distributed across households, the primary beneficiaries appear to be

[49] Recall that a household is classified as a never-mover if all its interviewed female members responded "always" to the question about how long they had lived continuously in their current place of residence.

[50] These robustness estimates are available upon request.

[51] Regressions using the household head's completion of secondary education as the outcome variable, regardless of whether the head is male or female, produce results similar to those observed in the male regressions. These results are available upon request. However, an additional selection issue may affect these estimates: in labor markets where qualifications are more highly rewarded, highly educated individuals are better positioned to become household heads.

members of the (lower) middle class. We further observe that increases in household asset wealth are associated with increased access to household utilities, greater consumption of durable goods, improved dwelling quality, higher educational levels of household members, and a shift away from agricultural employment. Importantly, the wealth gains are evident among both native and immigrant households, countering concerns that place-based policies, such as SEZs, disproportionately fail to benefit local populations. Collectively, the findings suggest that SEZ policies act as a catalyst for urbanization in the target areas.

Nevertheless, our analysis has two limitations that call for cautious policy interpretation and suggest directions for future research. The first stems from the non-random placement of SEZs, which in Africa are typically located near urban centers. As a result, our findings may not necessarily apply to SEZs established in less developed rural areas. Likewise, our distributional results may not capture the poorest households in the countries, as they are more likely to reside in remote rural locations.

Second, the employment data available from DHS primarily focus on a specific demographic group, namely females of reproductive age with children and their male partners, potentially overlooking broader employment effects across other population segments. Moreover, the absence of net employment gains among this demographic group of females contrasts with prior literature, which identifies young females as key beneficiaries of SEZ policies in developing countries. This discrepancy raises important questions about the role of motherhood as a potential constraint on female participation in SEZ-driven employment opportunities.

Finally, our results highlight the policy implication that SEZ policies can serve as effective tools for policymakers in developing countries to stimulate urbanization and improve the welfare of residents in targeted locations. This is particularly relevant for Africa, where significant regional disparities in terms of economic opportunities persist. By addressing these inequalities, SEZs have the potential to contribute to more inclusive economic growth across the continent.

CRediT authorship contribution statement

Matthew Amalitinga Abagna: Writing – review & editing, Writing – original draft, Resources, Investigation, Conceptualization. **Cecília Hornok:** Writing – review & editing, Writing – original draft, Visualization, Supervision, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Alina Mulyukova:** Writing – review & editing, Writing – original draft, Visualization, Methodology, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary material

Appendix Figures A1–A9 and Tables A1–A12 can be found online at https://doi.org/10.1016/j.jdeveco.2025.103482.

Data availability

The authors do not have permission to share data.

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