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# Does Trade reduce Infant Mortality? Evidence from Sub-Saharan Africa

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## Abstract

Trade can affect the development process of a country via various direct and indirect mechanisms. Empirically, it is difficult to identify causal effects, as trade is likely to be endogenous to other socio-economic factors that also affect development. To overcome this problem, this study uses a trade policy experiment called the African Growth and Opportunity Act (AGOA) which conferred many sub-Saharan African countries largely duty-free and quota-free access to US markets. Using retrospective birth histories from Demographic and Health Survey (DHS), I develop a large *micro panel* dataset that spans 30 sub-Saharan African countries and carry out a within-mother variation in survival of infant to find a causal impact of the policy. Identification in this analysis is based on each country's exposure to the trade policy at different points in time. I find that the policy reduces infant mortality by about 9% of the sample mean, even after controlling for country-time linear trends as well as mother's time invariant characteristics. Event study reveals no effects prior to AGOA implementation, corroborating that the decrease in infant mortality is due to AGOA. I also find strong heterogenous effects at the country and household level. The effects range from there being no significant effect to a strong increase or a strong decrease in infant deaths at the country level. The effect of AGOA on infant survival is stronger for countries that export large amounts of agricultural goods and mineral ores as compared to oil exporting countries. At the micro level, I see stronger effects for the uneducated, rural, and poor women via those women employed in agriculture or using manual labor. This study provides the first estimates of the effects of AGOA on an economic development indicator like infant mortality and adds to the quantitative impact of trade on health.

***Keywords:*** Infant Mortality; Child Health; Trade Openness; sub-Saharan Africa.

***JEL Codes***: F13, I15, J21, J82, O15, O24.

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## 1 Introduction

Historically, trade routes have played a major role in increasing the prosperity of nations.[2] Free trade can create access to a better variety of goods, increase women labor force participation, increase incomes, and often leads to improvements in infrastructure investment (Broda & Weinstein, 2006; Dollar & Kraay, 2002; Storeygard, 2013; Wood, 1991). Trade can affect the development process of an economy through affecting poverty, inequality, schooling, child labor decisions, and population health (Edmonds et al., 2010; Herzer, 2017; Porto, 2004; Topalova, 2010; Winters et al., 2004). There is a rich literature that investigates the link between trade liberalization and child mortality around the world (Barlow, 2018; Levine & Rothman, 2006; Olper, Curzi, & Swinnen, 2018; Owen & Wu, 2007). However, the quantitative impact of trade on health is mixed, depends on the country setting, has made limited use of individual-level data, and is still in its infancy (Burns et al., 2016). This paper fills this gap in literature by providing quantitative causal estimates of the effect of being exposed to a trade policy on infant and neonatal mortality by collating individual-level household surveys across 30 sub-Saharan African countries. Moreover, given the varied impact on population, this study analyzes the heterogeneous effects both at the macro and micro level and examines the possible pathways in the context of sub-Saharan Africa.

Empirically, it is difficult to identify causal effects, as trade is likely to be endogenous to other socio-economic factors that also affect development. To overcome this, identification in previous literature has come from using instrumental variables like predicting trade volumes as a ratio of GDP using geographic factors (Frankel & Romer, 1999; Levine & Rothman, 2006). However, these approaches might have potential threats to validity as geographical trade share may be correlated with other factors that affect children's welfare like presence of strong institutions.[3] A recent strand of literature uses Synthetic Control Method (SCM) for comparative case studies of

[2] The infrastructure created to boost trade becomes the main arteries of countries and turn cities into "engines of growth". Railroads had a great impact on the counties in American economy due to a change in that county's "market access" (Donaldson & Hornbeck, 2016).

[3] For example, Mauritius is surrounded by sea and has experienced an export boom in garments. But this boom has been attributed to a sound economic strategy by the government underpinned by social and political arrangements. Mauritius also has very low infant mortality rates which would have been brought about by the safety nets provided by the government. Hence, role of institution has been playing an instrumental role in decreasing infant mortality as well as increasing trade, which may not be properly captured by an instrumental variable approach. For more details on institutions, see (Rodriguez & Rodrik, 2001)

policy experiments across time across the world (Barlow, 2018; Olper et al., 2018). The use of SCM allows the researchers to compare the post-reform effect of the outcome by comparing it to the outcomes for a combination of similar countries. The analysis is done at the country level and is matched on *observable* characteristics in the pre-reform period. This paper uses a different empirical methodology, a difference-in-difference analysis with household fixed effects, to assess the average and heterogenous impact of a trade policy on infant deaths at the household level after controlling for household and individual-level characteristics.

The non-reciprocal trade agreement that this paper analyzes is the African Growth and Opportunity Act (AGOA), which conferred on many sub-Saharan African countries a largely duty-free and quota-free access to US markets. The head of the countries signing these agreements hoped the agreement would increase export volumes and spur economic growth in these economies. Frazer and Van Biesebroeck (2010) found that AGOA had a large and robust impact on exports to US without decreasing the country's export to Europe. Some countries like Kenya experienced an almost 700% increase in exports to US (from $36 million in 2000 to $284 million in 2010) (Onyango & Ikiara, 2011). This agreement took effect in 2000 with 34 sub-Saharan African countries eligible for the trade benefits included in the AGOA.

Identification in this analysis is based on each country's exposure to the trade policy at different points of time. Using retrospective birth histories from Demographic and Health Survey (DHS), I develop a micro panel dataset that spans 30 sub-Saharan African countries, with about 686,000 children born to 212,000 mothers. By observing different children of the same mother conceived before and after the trade policy change between AGOA affected and non-AGOA affected countries, a within-mother variation in survival of infant is carried out rather than cross-country or within-country variation. This analysis ensures that it is able to separate the effect of trade policy from other country level confounding factors like differential rates of spread of HIV/AIDS epidemic or differential country-level health policies or civil unrest since it is able to control for country-specific time trends. This empirical specification is also able to control for unobservable time invariant characteristics of mothers and countries, which may affect both effectiveness of trade policy and child health outcomes. Moreover, since this developed dataset are collated micro-level surveys across countries, it also overcomes the problem of small samples endemic to cross-country analysis.

The results of this analysis suggest that infant mortality falls by about 0.7 to 0.8 percentage points, or about 9%-10% of the sample mean, even after controlling for country-time linear trends as well as mother's time invariant characteristics. The results are also robust to controlling for some time variant country characteristics, which have been shown to be important in this literature. A large portion of this fall comes from a decrease in neonatal mortality. Moreover, the benefits of AGOA are not equally distributed across countries and across time. There are differential effects over a period of time in the drop in infant mortality which is explored via the dynamic treatment effects. The event-study reveals that there exists no effect prior to AGOA being implemented, corroborating that the decrease in infant mortality is due to AGOA.

The theoretical effect of trade on household is ambiguous and therefore is an empirical question. Trade could improve health outcomes via mechanisms like improved nutrition, increased incomes, improved access to sanitation and health care (Blouin et al., 2009; Levine & Rothman, 2006) but could also worsen child health by worsening environmental conditions (Blouin et al., 2009). I find strong heterogenous effects at the country level. The effects range from there being no significant effect to a strong increase or a strong decrease in infant deaths. This is partly explained by the composition of exports at the country level and the employment of women in these export-intensive sectors. The effect of AGOA on infant survival is stronger for countries that export large amounts of agricultural goods and mineral ores as compared to oil exporting countries.

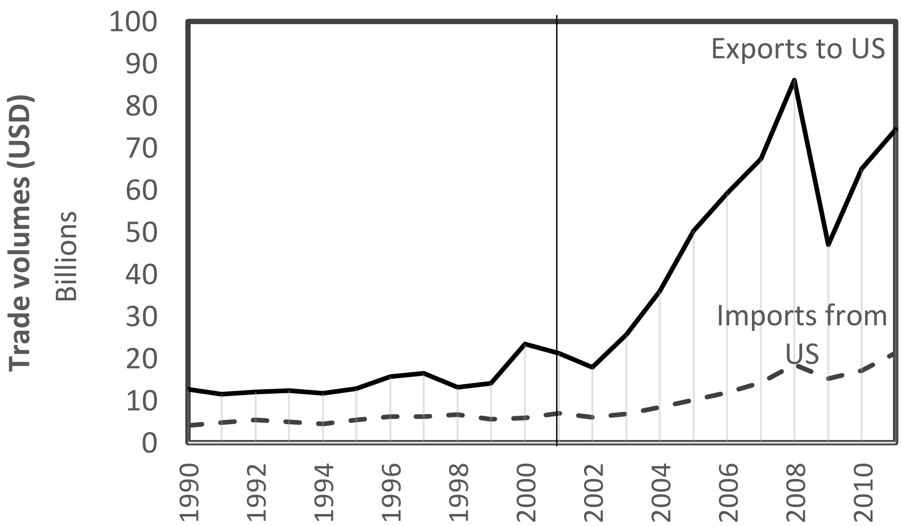
Trade increases employment opportunities and, especially in a developing country context, opportunities for low-skilled labor. Increasing opportunities for employment of mothers may contribute towards improving health of the child, due to increasing incomes (income effect) or may even deteriorate health of child as the mother stays away from home (substitution effect).[4] Keeping this in mind, this paper delves into heterogeneity at the household level. Infant deaths fall more for employed women than unemployed women, hinting towards affecting working women in export sectors. I find that infant mortality falls more for the uneducated, rural, and poor women via those women employed in agriculture or manual labor. Results point towards dominance of an income effect in reducing infant mortality in this setting.

[4] (Kishor & Parasuraman, 1998), find using NFHS Data for India in 1992-93 that mothers who are employed have a 10 percent higher IMR for their children and 36 percent higher child-mortality than mothers who are not employed. Many studies find strong relationship between increased female employment and increased exports (Standing, 1999; Wood, 1991).

## 2 Background

The African Growth and Opportunity Act (AGOA) provide for preferential treatment of exports from sub-Saharan Africa in the form of duty-free and largely quota-free access to US markets. It entails a series of incentives provided to African countries by the US opening its market for exports originating from these countries. AGOA has been part of the US international cooperation efforts for Africa since 2000. At the onset of the legislation, 34 countries were eligible for AGOA benefits.[5] AGOA was initially set to expire in 2008 but was eventually extended. Under AGOA provisions, four main sectors account for over 90% of the exports - energy-related products, textiles and apparel, transportation equipment, and minerals and metals. Figure 1 plots the total US imports and exports from sub-Saharan Africa. There is a significant increase in exports from sub-Saharan Africa to US after 2001 when the AGOA took effect. Overall, total US imports from AGOA countries have increased from \$5B in 2000 to over \$25B in 2005 (Páez et al., 2010).

**Figure 1: Trade Volumes between US and sub-Saharan Africa**




Note: This graph has been plotted using the data from International Trade Administration, U.S. Department of Commerce. It depicts the total exports and imports between US and all the sub-Saharan African countries from 1990-2011. The solid black line represents the imports into US from sub-Saharan Africa while the dotted line represents the exports from US to sub-Saharan Africa. It is observed that both exports and imports from sub-Saharan Africa increase dramatically after 2001. A more distinct increase in exports from sub-Saharan Africa to US is observed.

[5] More countries were added for the benefits later and some were removed due to failures regarding political or democratic freedom. Cote D'Ivoire was removed from the list in January, 2005. Effective December 23, 2009, the President removed Guinea, Madagascar, and Niger from the list of AGOA eligible countries.

AGOA was implemented not only to boost exports but also improve and foster economic growth. Country eligibility for AGOA is determined by the US President, and takes into account whether countries have made efforts to improve human rights, follow open market economic policies, protect worker rights and remove child labor, combat corruption, and establish rule of law among others.[6] The eligibility criteria for the Generalized System of Preferences (GSP), a US trade preference program that applies to more than 120 developing countries, and AGOA substantially overlap but AGOA covers more product lines and includes additional criteria. Under initial AGOA legislation, 1800 additional items were allowed to be exported duty-free in addition to the 4,600 under GSP. These newly added lines included items such as footwear, handbags, many agricultural products, chemicals, steel etc. These constituted the non-apparel exports under AGOA and could be exported at zero import duties as soon as the countries were declared AGOA beneficiary.

AGOA also places heavy emphasis on Africa's emerging textile and apparel industry as the primary sector for trade benefits. While AGOA removes import duties on eligible African imports, preferential market access is granted only upon compliance with the relevant Rules of Origin. These rules prescribe the percentage value added that must take place locally in an AGOA-beneficiary country, while special provisions relating to apparel outline what processing must take place locally. However, the lesser-developed countries were eligible for a Special Rule and could source raw materials from all over the world until 2004 while still receiving AGOA benefits.[7] AGOA also benefits these signatory countries as the exports under AGOA are not subject to a maximum volume ceiling as under GSP. However, with the ending of Multi-Fiber Agreements (MFA) in 2005, the apparel exports from African countries have decreased in the face of competition from China, Bangladesh, and India.

Many studies have been conducted to study the effectiveness of AGOA in increasing exports to US. Many studies find a positive and significant impact of AGOA on exports (Collier & Venables,

[6] Country eligibility is listed in Section 104 of African Growth and Opportunity Act. It states that countries need to "have established, or are making continual progress toward establishing the following: market-based economies; the rule of law and political pluralism; elimination of barriers to U.S. trade and investment; protection of intellectual property; efforts to combat corruption; policies to reduce poverty, increasing availability of health care and educational opportunities; protection of human rights and worker rights; and elimination of certain child labor practices".

[7] The lesser-developed beneficiary countries are countries with a per capita income of less than $1,500 in 1998. By the end of 2002, 33 countries were beneficiaries of Special Rule provision.

2007; Condon & Stern, 2011; Frazer & Van Biesebroeck, 2010).[8] Thompson (2004) and Mattoo, Roy, & Subramanian (2003) show that the largest share of US imports from Africa remain to be the oil and energy sectors. These studies provide evidence on heterogeneous effects of AGOA based on country's main item of export and volume of exports from these countries. However, the focus of these studies is mainly to look at the effectiveness of the policy in increasing exports from these countries. This study provides the first estimates of the effects of AGOA on an economic development indicator like infant mortality, to the best of my knowledge.

Quantitative evidence of macroeconomic trade policies on microeconomic outcomes like child health has been understudied. Levine and Rothman (2006) use Frankel and Romer's approach in predicting how much a country will trade based on exogenous geographical characteristics and then use this predicted trade share to obtain a cross-sectional effect of trade on children's health. They find that for an average country, a 15-percentage point increase in predicted trade as a share of GDP (an increase of about 1 standard deviation) corresponds to approximately 4 fewer infant deaths per 1000 births. However, they do not use a panel data set and hence are unable to capture how the change in trade affects change in infant mortality and trade volumes may capture other country specific institutional characters. The country specific effects are taken care by using a panel data structure. Owen & Wu (2007) use a panel of 219 developed and developing countries to find that trade openness improves life expectancy and child mortality. However, the impact does not always hold and for developing countries, the effect of trade on child mortality is insignificant. Many factors that change over country and time are not accounted for at this level of panel data analysis. The recent evidence of the association between trade and child mortality is provided by Olper at al. (2018) and Barlow (2018). Using Synthetic Control Methods, these studies better capture the time-varying country effects and show that the effect of trade is heterogenous. Barlow (2018) shows that, on average, trade liberalization had no impact on child mortality in low- and middle-income countries between 1963 and 2005, but there are distinct heterogenous effects in magnitude and significance. Olper et al. (2018) show that in 50% of their sample they find an average effect of reduction in child mortality rates of about 17 percentage points and no significant effect in 45% of the cases. However, these studies look at a set of very different countries across

[8] There have been few studies questioning the distribution of benefits of AGOA inside the country. Paulos et al. (2010) review the progress of a decade of AGOA and find that even though exports may be increasing, it may not be benefitting the countries internally.

the world through time and do not utilize individual-level data. This paper using the individual-level rich dataset within 30 sub-Saharan African countries for a particular trade policy that affected the region, is able to control for various confounding factors and provides new evidence of a causal effect of trade on child mortality.

## 3 Data

The micro level health data for the sub-Saharan African countries comes from the Demographic and Health Surveys (DHS). DHS are nationally representative household surveys that provide data for population, health and nutrition The DHS questionnaire is (mostly) standardized across countries and rounds, and so allows for comparisons across countries. The Standard DHS Surveys have large sample sizes (about 5,000 households) and are typically conducted about every 5 years. Information regarding child health, immunizations, antenatal care, etc. is found in the surveys, along with mother and household characteristics.

DHS collects data using three types of questionnaires – household, women's and men's questionnaires. Household questionnaire is used to collect data on household dwelling units, nutritional status, and anemia; while women's questionnaire is used to collect data from women about the characteristics, reproductive behavior, contraception, children's health etc. Women of reproductive age (15-49 years) are interviewed about the date of birth and death (if applicable) for up to 20 children they have had. This kind of retrospective survey gives an opportunity to build a panel dataset of mothers, with the time dimension being the child birth year.

One problem with the recall data is measurement error. To be robust to measurement error and to capture the maximum effect of comparing the children born to the same mother before and after the policy treatment, all children born before 1990 were dropped from the sample.[9] This ensures that the siblings are not very far apart in age and hence are broadly comparable. This also reduces the recall bias since all the surveys used in the analysis are carried out in 2000s. Moreover, some sub-Saharan African countries in the sample gained their independence between 1975 and 1990 and also experienced higher rates of civil wars, which may muddle with the effects of trade on

[9] The results are not dependent on the year of birth cut off. Other models with different year of birth cutoffs or using the full sample gave similarly significant results. See Online Appendix B for results.

infant mortality.[10] Since the most recent year in the surveys have few observations, they have been merged with the previous year to prevent sharp spikes in infant death due to limited observations in the last year of survey. Even though, Mozambique, Liberia, Ethiopia, and Cote D'Ivoire were given AGOA rights, in the sample they effectively behave as not treated since the law came into effect in the last year of the survey.[11] These are in the non-treated group, along with Zimbabwe, which is not AGOA eligible.

There are 36 DHS Surveys publicly available for the sub-Saharan countries where DHS survey has been carried out at least once.[12] The surveys for Central African Republic, Comoros, Gabon, South Africa, Sudan, and Togo were all carried out before AGOA was implemented in these countries. The remaining 30 surveys are included in this analysis.[13] A dummy variable indicating if the child has died before reaching the age of 1 year is constructed based on mother's birth history. This will be the indicator for *individual-level* infant mortality since the unit of analysis is the household. As long as at least one round of survey has been conducted in a particular country, a panel dataset of mothers for that country can be built.[14]

After dropping data for children born within twelve months of the survey, to ensure full exposure for every child in the sample and reduce measurement error, the sample includes 686,093 children born to 212,738 mothers. Infant (Neonatal) mortality rate is the number of deaths of children before reaching the age of one year (month) per 1000 live births. In this sample, it is calculated by multiplying the sample mean child deaths (in the appropriate age group) by thousand. The sample average infant mortality rate is 8.15% of live births while the sample neonatal mortality rate is 3.8% of live births. Since determinants of neonatal mortality may differ from infant mortality, an indicator for child dying before the age of 1 month is also constructed and analyzed.

In Table 1, I show average infant deaths for the whole population, as well as infant mortality based on different characteristics of mother like education, place of residence and wealth levels. Infant

[10] Mozambique (1975), Cape Verde (1975), Comoros (1975), Sao Tome and Principe (1975), Angola (1975), Zimbabwe (1980), Namibia (1989)

[11] Results are robust to not merging the last year and including Mozambique, Liberia, Ethiopia, and Cote D'Ivoire in the treated sample for that short period of time.

[12] These are most recent surveys at the time of analysis. Newer DHS has been carried out in past 2-3 years.

[13] The list of DHS surveys used and respective sample periods are listed in Table A8 in Appendix.

[14] DHS dataset has been used in this manner to study the effect of income fluctuations on infant mortality (Bhalotra, 2010; Paxson & Schady, 2005) and effect of democracy on infant mortality (Kudamatsu, 2012). This paper follows that methodology.

mortality varies significantly (based on t-statistic) between AGOA and non-AGOA countries. Infant and neonatal deaths also vary between AGOA and non-AGOA countries for women of different socio-economic status. Since DHS does not have income data, definition of poor is based on possession of assets. The wealth index is calculated using easy-to-collect data on a household's ownership of selected assets, such as televisions and bicycles; materials used for housing construction; and types of water access and sanitation facilities using principal components analysis and is reported in DHS. A mother is categorized as poor if the wealth index is marked as poor or poorer, while mothers with wealth index being middle, richer, or richest are categorized as non-poor. Rural or urban are defined by the place of residence of mother during the time of interview. A mother is labeled as educated if she has attended any type of school and uneducated is defined as mother did not attend any school, for brevity.[15] Among the child characteristics, birth order differs between these countries and is also included in controls.[16] It is observed that these countries are similar in terms of sex composition, but the composition of mother's age at birth is different across these countries. I include controls for maternal age at birth.

Figure 2 plots the sample mean infant mortality rates by year for countries affected by AGOA by 2001, affected after 2001 and never affected by AGOA. It shows that AGOA affected countries have higher infant mortality than non-AGOA countries at the time of first implementation of AGOA in 2001. All three groups of countries exhibit a declining trend in infant mortality over the years and the difference seems to be decreasing after AGOA has been implemented. The differential trends in infant mortality will be accounted for by using country time trends in this analysis. The mean infant mortality rates for the 25 AGOA affected sub-Saharan African countries in the sample by year of birth of child, 1990 onwards is shown in Appendix Figure A2. The data shows a declining trend in infant mortality over time for all countries. A sharp fall in infant deaths in some of the countries after the year AGOA is implemented is observed, more prominently than others.

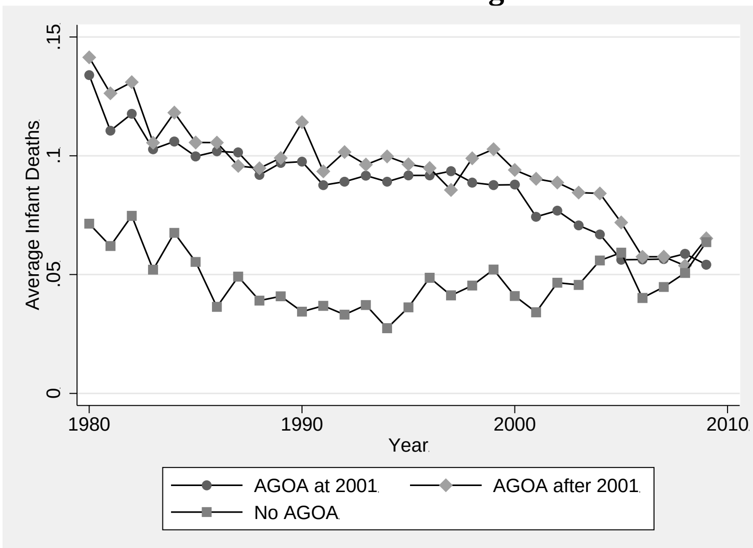
[15] Educated includes primary schooling, secondary schooling, and higher education. I disaggregate these effects later in the heterogeneity analysis.
[16] Country specific birth order is also controlled for in the robustness checks in Online Appendix B.

**Table 1: Summary Statistics – Child Variables**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | **All** | **Before AGOA** | **After AGOA** | **Non-AGOA** | **T-test** |
| *Child Variables* | | | | | |
| **Infant Mortality** | 0.0815 | 0.089 | 0.065 | 0.089 | 9.57 |
| *Uneducated* | 0.0939 | 0.1016 | 0.0719 | 0.104 | 8.34 |
| *N* | *342382* | *201754* | *93566* | *47062* | |
| *Educated* | 0.0691 | 0.075 | 0.059 | 0.074 | 4.26 |
| *N* | *343693* | *174889* | *124351* | *44453* | |
| *Poor* | 0.0902 | 0.101 | 0.069 | 0.098 | 6.42 |
| *N* | *300418* | *161300* | *97069* | *42049* | |
| *Non-Poor* | 0.0747 | 0.081 | 0.061 | 0.082 | 6.53 |
| *N* | *385675* | *215353* | *120856* | *49466* | |
| *Rural* | 0.0866 | 0.096 | 0.067 | 0.095 | 8.27 |
| *N* | *501284* | *272892* | *163277* | *65115* | |
| *Urban* | 0.0677 | 0.071 | 0.057 | 0.076 | 5.73 |
| *N* | *184809* | *103761* | *54648* | *26400* | |
| **Neonatal Mortality** | 0.038 | 0.041 | 0.032 | 0.040 | 3.40 |
| **Female** | 0.492 | 0.492 | 0.493 | 0.492 | -0.06 |
| **Multiple Births** | 0.035 | 0.034 | 0.037 | 0.033 | -2.45 |
| **Birth Order** | 3.47 | 3.38 | 3.64 | 3.45 | -1.96 |
| **Month of birth** | 6.15 | 6.10 | 6.23 | 6.07 | -7.03 |
| **Mother's age at birth(20-29yrs)** | 0.50 | 0.499 | 0.483 | 0.49 | -6.43 |
| **Mother's age at birth(30-39yrs)** | 0.24 | 0.23 | 0.26 | 0.24 | -1.16 |
| **Mother's age at birth(40-49yrs)** | 0.02 | 0.015 | 0.045 | 0.027 | 5.88 |
| **N** | 686093 | 468168 | 217925 | 91515 | |

Note: Sample means of all child level variables are reported. Column (1) is for the whole sample with AGOA affected and non-affected countries. Columns (2) and (3) report the sample mean infant mortality before and after the implementation of AGOA in AGOA affected countries. Column (4) reports the sample mean in non-AGOA countries. Column (5) gives the t-statistic testing if the means are significantly different between AGOA and non-AGOA countries. N refers to the number of observations in each sample. Educated implies having attended any type of school and uneducated is defined as mother did not attend any school. Poor is defined by a wealth index as defined as poor or poorer vis-à-vis with mothers who are non-poor based on the wealth index being middle, richer or richest. Rural and Urban are defined by the place of residence of mother during the time of interview. Female is 1 if sex of child is female. Multiple birth is a dummy variable indicating if the child is born in a multiple birth. It is 0 for a single birth and 1 for twins, triplets or quadruplets.

**Figure 2: Infant Deaths for AGOA and not-AGOA eligible countries**




Note: This graph plots the sample infant mortality rates for countries affected by AGOA by 2001, countries affected by AGOA after 2001 and never affected by AGOA countries, by year of child birth. The countries affected by AGOA by 2001 are – Benin, Cameroon, Chad, Congo, Ethiopia, Ghana, Guinea, Kenya, Lesotho, Madagascar, Malawi, Mali, Mozambique, Namibia, Niger, Nigeria, Rwanda, Sao Tome and Principe, Senegal, Eswatini, Tanzania, and Zambia. Countries which received AGOA benefits after 2001 are – Angola, Burkina Faso, Burundi, Cote D'Ivoire, Democratic Republic of Congo, Liberia and Sierra Leone. Zimbabwe has never been an AGOA beneficiary.

Since the estimation strategy includes maternal fixed effects, it is the mothers giving birth both before and after AGOA that contribute to the identification of effect of AGOA. Thus, in Table 2, I compare the characteristics of mothers having two or more births before and after AGOA (column (3)) with mothers in the entire sample (column (1)) as well as mothers in AGOA countries (column (2)). Mothers who give birth at least twice and both before and after AGOA are less well-educated, live in rural areas, and poor.

Since mother fixed effects estimation derives the effect of AGOA on infant mortality using those mothers giving birth both before and after AGOA, Table 3 shows the sample mean infant and neonatal mortality rates for mothers giving birth both before and after AGOA and for mothers with more than two births only before or after AGOA. The sample mean infant and neonatal mortality rates fall for mothers giving birth both before and after AGOA, after AGOA is implemented. Comparing Column (1) and Column (2) shows that the mean infant and neonatal mortality is similar for both the groups of mothers before AGOA is implemented.

**Table 2: Mother Characteristics – Full Sample and 2+ Mothers**:

| | (1) | | (2) | | (3) | | (4) |
|---|---|---|---|---|---|---|---|
| | **Full Sample** | | **AGOA Countries All mothers** | | **2+ Sample both before and after AGOA** | | **T-test** |
| | **Mean** | **Std. Dev.** | **Mean** | **Std. Dev.** | **Mean** | **Std. Dev.** | |
| **Mother's age at birth** | 25.72 | 6.44 | 25.72 | 6.42 | 26.03 | 6.39 | -23.7 |
| **Mother's Education** | 0.501 | 0.499 | 0.503 | 0.499 | 0.478 | 0.499 | 24.78 |
| **Mother's wealth index** | 2.86 | 1.402 | 2.86 | 1.39 | 2.77 | 1.38 | 35.92 |
| **Mother's Residence (Rural)** | 0.73 | 0.444 | 0.73 | 0.442 | 0.77 | 0.422 | -37.29 |
| **N** | 686093 | | 594578 | | 391425 | | |
| **N (M.Educ)** | 686075 | | 594560 | | 391414 | | |

Note: Sample means and standard deviations are reported for different samples of mothers. N refers to the number of observations in each sample. Column (1) gives the mean and standard deviation for different mother characteristics for the whole sample with AGOA affected and non-affected countries. Column (2) reports the same for all mothers in AGOA affected countries. Column (3) reports the sample mean and standard deviation for mothers with two or more children giving birth before and after AGOA. All variables are categorical variables except mother's age at birth. Column (4) provides a difference in means t-test between (2) and (3).

**Table 3: Mean Infant and Neonatal Mortality for Sample of 2+ Mothers in AGOA Countries**

| | (1) | | (2) | |
|---|---|---|---|---|
| | **Both before and after AGOA** | | **Only before or after AGOA** | |
| | **Mean** | **Std. Dev.** | **Mean** | **Std. Dev.** |
| **Infant Mortality** | | | | |
| Before AGOA | 0.090 | 0.286 | 0.091 | 0.286 |
| After AGOA | 0.063 | 0.243 | 0.077 | 0.267 |
| **Neonatal Mortality** | | | | |
| Before AGOA | 0.041 | 0.198 | 0.0436 | 0.202 |
| After AGOA | 0.029 | 0.168 | 0.0425 | 0.187 |
| **N** | 391425 | | 165098 | |
| Before AGOA | 247784 | | 117811 | |
| After AGOA | 143641 | | 47287 | |

Note: Sample mean is reported in the top row and number of live birth observations for AGOA affected countries in the bottom row. Column (1) gives the sample mean and standard deviation for infant and neonatal mortality for the sample of mothers giving birth both before and after AGOA. Column (2) reports the sample mean and standard deviation for mothers with two or more children either only before AGOA or after AGOA. N represents the number of live births.

# 4 Empirical Strategy

To analyze the effect of trade on infant mortality, I estimate the following equation using a linear probability model and a difference-in-difference specification with sibling fixed effects:

$$IMR_{imct} = \alpha_m + \beta_t + \gamma T_{ct} + X_{imct}\delta + \mu_c.t + \varepsilon_{imct} \quad (1)$$

for child i, born to mother m in country c in year t. IMR is a dummy which takes the value 1 if child i dies before reaching the age of 1 year, $\alpha_m$ is mother fixed effect, $\beta_t$ is birth-year fixed effect and $\mu_c.t$ captures the country-time specific trend. $T_{ct}$ takes the value 1 if the specific country was under AGOA throughout time t. $X_{imct}$ is a vector of control characteristics including sex of the child, whether or not they are born in multiple births (i.e. twins, triplets, etc.), dummies for their birth order, mother's age at birth, and birth month of the child. It may also be argued that birth order trends differ between countries. As a robustness check, country specific birth orders are also controlled for. $\gamma$ provides the estimate of the effect of AGOA on infant mortality. The standard errors are clustered at the country level to take into account any correlation of the error across space and time within each country.[17]

There have been concerns on using trade volumes as a measure of trade policy (Rodrik, 2001). Trade volumes generally reflect the outcomes of many different things like economy's overall performance as well as productive capacity of the economy. Hence, trade volumes are not entirely controlled by the government, while trade policies are. Keeping this in mind, this study abstracts away from using trade volumes as an indicator of trade policy. Treatment in this paper is defined as a child's exposure to AGOA. This is a dummy variable which takes the value 1 if the child is born *after* AGOA is implemented. This ensures that the child has been fully exposed to AGOA through their lifespan (including in-utero and prenatal exposure). For example, if AGOA was signed and passed on 1st October, 2001 for country $C_1$, then AGOA takes the value of 2002 for $C_1$. If instead, AGOA is passed on January 1, 2003 for country $C_2$, then AGOA takes the value 2003

[17] Cameron and Miller (2015) place emphasis on the bias-variance tradeoff in clustering and emphasize on using bigger and more aggregate clusters when possible to avoid bias. Similar results are drawn with regard to sampling design, with the clustering of errors done at the minimum level of the primary sampling unit (PSU), though often the correlations are observed at a broader level and clustering of errors should then be done at the broader level. This guides the decision to cluster the errors at the country level to capture most correlations at the aggregate level. However, I also check for robustness of the results at different clustering levels (PSU and mother-cohort level) and the results are significant at all levels of clustering. Results are presented in Online Appendix B. The most conservative standard errors are reported for the main results.

in $C_2$. A child is then said exposed to AGOA if in $C_1$, they are born in 2003 or later while in $C_2$, they are born in 2004 or later. However, the results are not sensitive to this definition. Even if the actual AGOA date is used to create the treatment, the results still remain similar in magnitude and significant.[18]

In the next specification, an interaction between mother's birth cohort by child's birth year ($\beta_{bt}$)[19] fixed effects are included:

$$IMR_{imbct} = \alpha_m + \beta_{bt} + \gamma T_{ct} + X_{imbct}\delta + \mu_c.t + \varepsilon_{imbct} \quad (2)$$

This specification accounts for the possibility that women may be delaying their fertility based on improvements in survival of their children overtime. $\gamma$ captures the average difference in changes in probability of death of infants born to the same mother between those countries that have been affected by AGOA vis-à-vis those that are not. Since AGOA implementation varies by countries as well as time, fixed effects estimation can be carried out for more robust estimates.

For the estimates to be unbiased, the error cannot be correlated with any of the covariates and outcomes, not only contemporaneously but also in leads and lags as the same mother gives birth. Specifically:

$$E(\varepsilon_{imbct} | T_{ct}, \beta_{bt}, \alpha_m, \mu_c.t, X_{imbct}) = 0 \quad (3)$$

This specification also assumes that treatment selection can be based on unobserved heterogeneity at country level, but within country which mothers and children got the treatment is unrelated to the gain from the program. Another concern is that mothers affected by AGOA in AGOA affected countries do not behave differently than mothers in non-AGOA countries, if they had been AGOA affected. Thus, mothers cannot be timing their fertility in response to AGOA. I test to see later if fertility selection bias is a major concern in the data.

The main concern in studying the effect of such a policy is the difficulty of disentangling the effect of this policy from the prerequisites for being a signatory on the AGOA. In terms of disentangling this effect, this study does better than cross-country studies. Time-invariant heterogeneity regarding geography, history, culture, politics, and attitudes etc. are taken care of by the mother

[18] Results presented in Online Appendix B.
[19] Subscript b denotes the mother's birth cohort.

fixed effects ($\alpha_m$) since this implicitly also acts as country fixed effects because mothers of the children belong to a certain country of residence and there is no significant cross-country migration. The mother fixed effects specification subsumes country fixed effects. Moreover, any differences in family characteristics and mother's time invariant ability in taking care of the child is taken care of in this model.

The year fixed effects ($\beta_t$) control for an aggregate time variation involving improvement of health technology and year shocks. This takes care of GDP expansion cycles or wars and civil unrests. $\beta_{bt}$ controls for changing time of mother's age at birth. The mother cohort by year fixed effects controls for fertility changes overtime in that region due to improvements in health technology. The country specific trends ($\mu_c$.t), in fact, also allow country specific improvement in infant and maternal health i.e. differential states of development of the countries. This controls for any country-specific time varying health changes like expansion of HIV/AIDS epidemics or country specific health policies that vary with time, which may affect infant mortality.

But there may be time variant heterogeneity which may affect both trade and infant mortality rates. Implementation of AGOA or how well the country does after its implementation may depend on the country's political situation, GDP per capita, average female education of the country etc. Countries with a higher GDP per capita or in a democratic regime may experience a lower IMR too (Kudamatsu, 2012). Hence these may bias the estimates. As a robustness check, at the country level there is a control for additional characteristics ($Z_{ct}$) like GDP per capita, political regime of the country, whether it is a democracy, degree of openness overtime, average level of female education etc. which may help control some of the time variant heterogeneity at the country level. To capture these effects, I estimate the following equation:

$$IMR_{imbct} = \alpha_m + \beta_{bt} + \gamma T_{ct} + X_{imbct}\delta + \mu_c.t + \lambda Z_{ct} + \varepsilon_{imbct} \quad (4)$$

To check for heterogeneity based on mother's level of education, place of residence, and possession of assets, the mother-FE regression is run with interactions to tease out the effects:

$$IMR_{imbct} = \alpha_m + \beta_{bt} + \gamma(T*MC)_{ct} + X_{imbct}\delta + \mu_c.t + \varepsilon_{imbct} \quad (5)$$

Where, MC defines the mother's characteristics. The interaction term $(T*MC)_{ct}$ provides an estimate of treatment effect of AGOA on probability of infant death for a specific subsection of the population based on assets, education, employment and place of residence in comparison to the reference population.

Heterogeneity at the country level is also necessary to observe given the difference in exports variety and volumes across AGOA beneficiaries. To capture these effects, I estimate the following regression:

$$IMR_{imbct} = \alpha_m + \beta_{bt} + \gamma(T*CC)_{ct} + X_{imbct}\delta + \mu_c.t + \varepsilon_{imbct} \quad (6)$$

CC captures differences in country characteristics like whether a country is a predominant petroleum exporter, apparel exporter, agricultural exporter, low income country, region etc. The interaction term will indicate which types of countries are actually accruing the most benefits in reducing infant deaths via AGOA.

# 5 Results

## 5.1 Event-Study Graph

I create an event-study graph for the treated countries to show the effect of AGOA on infant and neonatal mortality. Figure 3 graphs the likelihood of child death by year, with respect to the treatment, for the treated countries. The plotted estimates depict the differential trends in infant and neonatal mortality over four years before and after the AGOA announcement (with the year before the announcement being the omitted year).[20] The estimates $\theta_j$ are derived from the following regression:

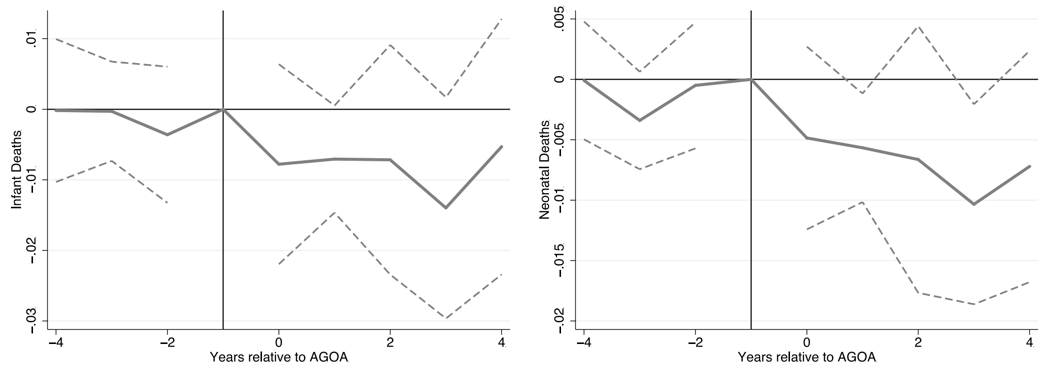
$$Death_{imct} = \alpha_m + \beta_t + \sum_{j=-5}^{5} \theta_j T_{c,t+j} + X_i'\delta + \varepsilon_{imct} \quad (7)$$

where $T_{c,t+j}$ is 1 for j years of announcement of AGOA in country c. The specification controls for mother and year fixed effects. The covariates included are mother's age at birth, sex of the child, birth order, birth month, and whether born in multiple birth. The standard errors are clustered at the country level.

[20] The years at -5 and under and +5 years and over are binned and included in the regression. Given that these estimates are binned, they are not plotted on the graph but presented in the regression output in Appendix Table A12. The binned estimates should be inferred with caution.

In both neonatal and infant mortality, there are no noticeable trends in the pre-treatment period. Consequently, F-test rejects the null hypothesis of joint significance of pre-treatment years. After AGOA is announced, there is a fall in both infant and neonatal mortality and it seems to be increasing over the first three years. The coefficients are jointly significant post treatment. The point estimates and F-statistics are presented in Appendix Table A12. The infant and neonatal mortality is below the pre-treatment level even after four years of implementation.

**Figure 3: Event-Time Study**




Note: These are the θj estimates plotted from estimating this equation:

$Death_{imct} = \alpha_m + \beta_t + \sum_{j=-5}^{5} \theta_j T_{c,t+j} + X_i'\delta + \varepsilon_{imct}$

Death takes the value of infant mortality of neonatal mortality. The sample is restricted to treated countries. The solid line at -1 indicates the year just before the announcement of AGOA. The control variables are whether born in multiple birth, birth order, birth month, and mother's age at birth. Both the specifications control for year and mother fixed effects. The solid line on the figure on the left depicts the fall in infant deaths and the solid line on the figure on the right depicts fall for neonatal deaths. The dotted lines are the upper and lower confidence intervals of the estimates, arrived at by clustering errors at the country level.

## 5.2 Main Results

Table 4 provides the main regression results of the effects of treatment on infant mortality Column (1) shows the results for country fixed effects, without controlling for linear country time trend. The resulting coefficient on AGOA is negative, but not statistically significant. Since there are country-specific trends in infant mortality, not controlling for those trends is confounding with the effect of AGOA. Controlling for country time trends along with country fixed effects in (2) makes the coefficient statistically significant. The coefficient now indicates that AGOA reduces the probability of infant dying by 0.8 percentage points. Controlling for maternal fixed effects in (3) decreases the coefficient to about 0.7 percentage points.[21]

**Table 4: Effect of AGOA treatment on infant and neonatal mortality**

| | Specification 1 | | | Specification 2 | | | |
|---|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** |
| **Dependent Variable** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Neonatal Mortality** |
| **Treatment** | -0.0071<br>(0.0028) | -0.0081***<br>(0.0028) | -0.0071**<br>(0.0028) | -0.0079***<br>(0.0019) | -0.0079***<br>(0.0028) | -0.0069**<br>(0.0027) | -0.0046***<br>(0.0011) |
| **Explanatory Variables** | YES | YES | YES | YES | YES | YES | YES |
| **Country time trend** | NO | YES | YES | YES | YES | YES | YES |
| **Country FE** | YES | YES | NO | NO | YES | NO | NO |
| **Mother FE** | NO | NO | YES | NO | NO | YES | YES |
| **Cohort-year FE** | NO | NO | NO | YES | YES | YES | YES |
| **Number of countries** | 30 | 30 | 30 | 30 | 30 | 30 | 30 |
| **Number of mothers** | 212738 | 212738 | 212738 | 212738 | 212738 | 212738 | 212738 |
| **Observations** | 686093 | 686093 | 686093 | 686093 | 686093 | 686093 | 686093 |

Note: Treatment is defined as 1 for a child born after AGOA has been implemented in an AGOA affected country. The other control variables included in the specifications are sex of child, whether born in multiple birth, year of birth, mother's age at birth, birth order and birth month. Standard errors clustered at the country level are reported in brackets. Specification 2 allows for changing mother's age at birth for different year of birth of child. Hence, controls for mother's age and year of birth of child are subsumed in these specifications.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

Table 4, specification 2 (columns (4)-(7)) additionally controls for cohort-year fixed effects. In this specification, the changing time of mother's age at birth due to improvements in survival of babies over time in Africa is accounted. The fixed effect controls for fertility changes over the years due to improvement in health technology. Even after controlling for these with an interaction

[21] To be robust to the possibility of small number of clusters in the sample, I also test the significance of coefficients using method outlined in Donald & Lang (2007) and Cameron & Miller (2015). Since N within each group (country) is large, the resulting t-statistic will have a T(G-2) distribution rather than standard normal, where G is the number of groups. The t-statistic is computed using the estimated coefficient and clustered standard errors and is tested using the T(G-2) distribution critical levels. The critical values for a two-tailed test using T distribution with 28 degrees of freedom are 1.70 at 10%, 2.048 at 5% and 2.763 at 1%. The coefficients still remain significant at these levels.

of dummies for mother's birth year (cohort) with child's year of birth, the magnitude of the coefficient remains around 0.7 percentage points statistically significant.[22]

The absolute value of coefficient remains between 7.9 to 6.9 reductions in infant deaths per 1000 live births, as we move across specifications. Thus, the results are robust to various specifications. Mother fixed effects controls for factors like maternal ability to raise kids, genetic traits, household environment, parental education, place of residence etc. On carrying out mother fixed effects analysis of AGOA on neonatal mortality in column (7), a significant negative effect is found. Neonatal deaths reduce by 4.6 deaths per 1000, which is about 12% of the sample mean. Hence, about half of the reduction in infant deaths is coming via a decrease in neonatal deaths.

The magnitude of the estimated effect is economically significant as well. Exposure to AGOA reduces infant mortality by 0.7 percentage points which is 9% of the sample mean and decreases deaths before age of one month by 0.4 percentage points, which is 12% of the sample mean. For comparison, the effect of Progresa, a conditional cash transfer program in Mexico, is an 8% reduction in rural IMR (Barham, 2011). The previous literature on the link between trade and child health produces varied estimates. Levine and Rothman (2006) find that for an average country, a 15-percentage point increase in predicted trade as a share of GDP results in 4 fewer infant deaths per 1000 births. The estimated effect is higher in absolute magnitude using mother FE than in the cross-country setting, with trade openness contributing to a reduction of around 7 infant deaths per 1000 births. [23] Olper et al. (2018) find a 17 percentage point decrease in infant mortality on an average for 50% of their sample to no significant decrease in other parts of the sample, which is much different in magnitude and may point to country and region specific heterogeneity across the world.

It is crucial to note that the above effects are average treatment effects for countries that were affected by AGOA over a period of time. However, we may expect that AGOA will have dynamic effects over years and the above aggregated effect may suffer from short-run bias. To alleviate any

[22] On testing sex selection at birth, I do not find differences in probability of infant dying based on gender. Results are available upon request.

[23] Even though I do not use trade share as my main coefficient of interest due to reasons argued before, the change in trade to GDP shares for the countries in the analysis has been presented in the Appendix Table A11 for the readers to get an idea of the heterogenous effects and magnitudes of trade share and volume changes. Moreover, as a robustness check, I also check for changing percentage of trade as an alternate treatment definition in my specifications and present the results in Online Appendix B.

short run bias, the average effects should be calculated with a valid control group which never experiences treatment, is not small relative to the treatment group, and should not be on a separate time trend (Borusyak and Jaravel, 2017). Given that the sample of mothers giving birth to 2 or more children only before or after AGOA is 42% of the treated mothers and that we observe similar mean infant and neonatal mortality rates before AGOA across these groups (see Table 3), the coefficients should not suffer from short-run bias. However, we may still estimate a semi-dynamic specification which would specify the lagged effects of the policy and that holds true if there are no pre-trends (as has been shown via the event study). A regression involving lag periods for AGOA has been estimated with flexible dynamic effects:[24]

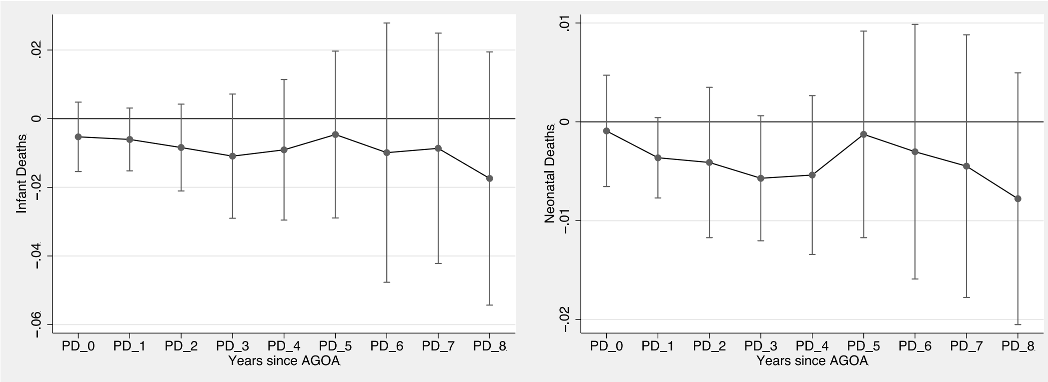
$$IMR_{imct} = \alpha_m + \beta_t + \sum_{j=0}^{infinite} \theta_j.1\{J_{c,t}=j\} + X_{imct}'\delta + \mu_c.t + \varepsilon_{imct} \quad (8)$$

Where the treatment is normalized to 0 when AGOA is implemented in a specific country and lags are calculated with reference to that. The same controls as equation (1) are used.

Figure 4 graphs the dynamics of infant mortality from the time of AGOA implementation to 8 years after it.[25] It should be noted that composition of countries and mothers is changing as we move farther along the timeline since every country is not exposed to AGOA for the same number of years. It can be seen that the effect on both infant and neonatal mortality changes as greater number of years pass since AGOA, with respect to before the implementation of AGOA and the control groups. Similarly, heterogenous effects with time can be calculated by interacting AGOA with the year dummies for each AGOA year. The results show that the impact of AGOA strengthens over time and are strongest in our sample between 2007-09. These estimates are presented in the Appendix Table A10.

[24] Point estimates are shown in Appendix Table A9.
[25] 8 years are the maximum lag years observed in this sample.

**Figure 4: Dynamics of Infant and Neonatal Mortality**




Note: The solid black line depicts the change in infant (left figure) and neonatal (right figure) mortality compared to before implementation of AGOA controlling for mother fixed effects, time fixed effects, country specific linear trends, sex of child, whether born in multiple birth, birth order, mother's age at birth, and birth month. Period 0 is the year of implementation of AGOA. The confidence interval with standard errors clustered at the country level are represented by the vertical bars. It should be noted that composition of countries and mothers is changing as we move farther along the timeline. The point estimates are provided in Appendix Table A9.

### 5.3 Robustness to Time-variant Factors

Table 5 controls for country level variables like log GDP per capita, democratic regime, openness, female education etc. in the mother FE specification with cohort-year FE. Since some benefits of AGOA were based on income threshold, especially for Apparel exports, it is imperative to control for changing GDP per capita levels for the countries since higher income countries may also display better health of children. GDP per capita data is obtained from PWT 7.0 (Heston, Summers, & Aten, 2011) and log of GDP per capita is used to run the regression with cohort year fixed effects in Table 5 (1). Infant mortality was observed to decrease with an increase in the GDP per capita (significant at 10% level), but even controlling for GDP per capita did not reduce the magnitude of the AGOA coefficient much nor remove statistical significance.

Some studies find that democracy and political regime may affect child health (Kudamatsu, 2012). Since AGOA emphasized political stability, it was the politically stable countries which acquired and retained AGOA rights.[26] It may also have served as an incentive to turn into a democratic country to acquire AGOA rights. Hence, democracy may have served as a pre-condition for getting AGOA benefits. The effect of democracy and political regime is controlled for by using the democracy-dictator data from Cheibub et al. (2009) which is an updated dataset based on

[26] For example, Cote D'Ivoire was removed as an AGOA beneficiary due to not implementing a peace plan and Eritrea was removed after failing to bring about democratic reforms.

Przeworski et al. (2000). They define democracy as: the executive is directly elected or indirectly elected via the legislature; the legislature is directly elected; there is more than one party; and the executive power alternates between different parties under the same electoral rule. If a country satisfies these conditions, the democracy indicator takes the value 1. In Table 5 (2) controlling for democracy status of the country, does not change the magnitude of the coefficient much from the results in Table 4. Democracy tends to reduce infant mortality but the coefficient is not significantly different from zero at the conventional level.

Sub-Saharan African countries have increasingly received Official Developmental Assistance (ODA) from developed countries to promote economic development. It may be the case that at the same time AGOA was introduced, the trade-related or other parts of ODA also increased. ODA is intended to provide assistance in development and hence will aid in infant mortality reduction. To ensure that the actual effects of AGOA are observed, ODA is included as a control variable in Table 5 (3). It may be argued that a country which already had trade routes open under GSP would have benefitted more from AGOA and hence its coefficient maybe capturing the effects of already increased trade flows. But controlling for openness from PWT 7.0 in Table 5 (4), it is observed that the coefficient is not significantly different from zero and the original coefficient on AGOA does not decrease in absolute value or significance.

Countries with higher growth of human capital such as average years of education of females in a country may be benefitting more than others in attracting trade flows as well as decreasing infant mortality. Thus, data for the average years of schooling of females 15 years or older is collated from Barro & Lee (2013) and there is a control for average years of female education of the country in Table 5 (5). The number of countries for which this data is available falls to 21. It is seen that the coefficient is not significantly different from zero and also the coefficient on treatment to AGOA does not change much and stays statistically significant.

Commodity price fluctuations have contributed to improved incomes and growth in Africa over the last decade (Deaton, 1999). Changes in international commodity prices can work through changes in consumption and government expenditure, which results in changes in national output. Since sub-Saharan Africa relies a lot on primary exports and these are subject to volatility in commodity prices, it is necessary to separate the effects of a commodity price boom from the effects of AGOA. Considering this finding, the commodity price index derived from PWT 8.0

(Feenstra, Inklaar, & Timmer, 2015) in Table 5 (6) is controlled for but this does not decrease the magnitude of the coefficient on AGOA much. The coefficient on commodity price index is itself significant and tends to increase infant mortality. In Table 5 (7), all the macro variables are controlled for and that also does not reduce the magnitude or significance of the variable in question. Finally, the use of country time trends captures country specific trends that may confound the analysis as well. It confirms that the coefficient on AGOA is robust to controlling for some of the important country level time variant factors.[27]

**Table 5: Country-level time varying variables**

| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** |
|---|---|---|---|---|---|---|---|
| **Dependent Variable** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** |
| **Treatment** | -0.0068** | -0.0076*** | -0.0082*** | -0.0069** | -0.0067* | -0.009*** | -0.0066** |
| | (0.0025) | (0.0026) | (0.0025) | (0.0026) | (0.0032) | (0.0025) | (0.0028) |
| **Log GDP per capita** | -0.0099*<br>(0.0054) | | | | | | -0.0175*<br>(0.0094) |
| **Democracy** | | -0.0041<br>(0.0029) | | | | | -0.0043<br>(0.0028) |
| **ODA** | | | 0.00009<br>(0.0001) | | | | -0.00003<br>(0.00007) |
| **Openness** | | | | -0.00002<br>(0.00007) | | | 0.00009<br>(0.00005) |
| **Female Education** | | | | | 0.0029<br>(0.0053) | | -0.001<br>(0.0048) |
| **Commodity Price Index** | | | | | | 0.0327***<br>(0.0067) | 0.0311***<br>(0.0066) |
| **Number of countries** | 30 | 30 | 29 | 30 | 21 | 29 | 21 |
| **Number of mothers** | 212738 | 209721 | 205420 | 212738 | 134952 | 206137 | 131959 |
| **Observation** | 686093 | 673646 | 655443 | 686093 | 410833 | 663838 | 394715 |

Note: The regressions control for sex of child, whether born in multiple birth, birth order, birth month, mother fixed effects, country specific linear trends, mother's cohort by child birth year FE. Standard errors clustered at country level are reported in brackets. Data for (1) and (4) taken from PWT 7.0, (2) is taken from Democracy-Dictatorship (DD) Data by Cheibub et al (2010), (3) Net Official Development Assistance received as a % of GNI is taken from World Development Indicators (The World Bank, 2012), (5) from Barro and Lee (2010), and (6) from PWT 8.0. Number of observations and number of mothers varies depending on availability of country level control variable from different data sources.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

[27] I also check for robustness to controlling for ODA since sub-Saharan African countries were highly dependent on foreign aid and these changed with time, the coefficient still remains significant and similar in magnitude.

## 6 Heterogeneity in Effects and Possible Pathways

Recent literature assessing the impact on child health shows that there may be a huge variation in effect for different countries (Olper et al., 2018). Moreover, there is evidence that AGOA may affect the recipient countries differentially based on their composition of exports at the country level (Thompson, 2004; Mattoo et al., 2006). Given this, we would expect there to be a variation in the treatment effect across countries, household, and time. I explore these in the following section. The heterogenous effects across space and time also point towards the possible pathways through which we observe the average effect of AGOA on infant mortality.

At the individual level, heterogeneity may exist based on characteristic of the mother and the household. Table 6 columns (1)-(4), check for heterogeneity in effects based on mother's place of residence, education, possession of assets, and employment.[28] AGOA differentially decreases infant deaths for uneducated mothers with no (or low) schooling more than educated mothers with secondary or higher education. AGOA also has a significant effect in reducing infant mortality for mothers living in rural areas, but not for those living in urban areas. AGOA seems to be effective in significantly reducing infant deaths for poor more than non-poor; negating the widely held notion that trade increases inequality. While interpreting these results, it should be kept in mind that data on asset variables is available at the time of survey. So, as long as mothers have not moved across wealth categories (moving within wealth categories does not pose a problem), these results are informative. AGOA seems to be affecting the more socially disadvantaged sections of the society, where there is a larger scope of reducing infant mortality. This is consistent with standard economic theory (Hecksher-Ohlin model) stating that gains of trade should flow to abundant factors, and in this developing country setting, unskilled labor (uneducated, rural, and poor mothers) should benefit the most.

[28] The definitions of variables are elaborated in the Data Section.

**Table 6: Heterogeneity across different types of mothers**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Dependent Variable** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** |
| **No Schooling** | -0.0082***<br>(0.0029) | | | |
| **Primary Ed.** | -0.0075*<br>(0.0039) | | | |
| **Secondary Ed.** | 0.0003<br>(0.0039) | | | |
| **Higher Ed.** | 0.0139<br>(0.0092) | | | |
| **Rural** | | -0.0085***<br>(0.0028) | | |
| **Urban** | | -0.0018<br>(0.0031) | | |
| **Poor** | | | -0.0102***<br>(0.0028) | |
| **Non-Poor** | | | -0.0044<br>(0.0029) | |
| **Employed** | | | | -0.0095***<br>(0.0028) |
| **Unemployed** | | | | -0.0057<br>(0.0038) |
| **F-Stat** | 3.08<br>(0.043) | 5.71<br>(0.021) | 7.82<br>(0.009) | 2.25<br>(0.145) |
| **Number of Countries** | 30 | 30 | 30 | 28 |
| **Number of mothers** | 212732 | 212738 | 212738 | 197632 |
| **Observations** | 686075 | 686093 | 686093 | 632951 |

Note: The control variables are sex of child, whether born in multiple birth, birth order, birth month, mother fixed effects, country specific linear trends, mother's cohort by child birth year FE. Standard errors clustered at country level are reported in brackets. The treatment is interacted with the type/characteristic of mothers to get the treatment effect on those types of mothers vis-à-vis all mothers of control group. Column (1) includes the effect on infant mortality for educated mothers where educated implies having attended either primary, secondary, or higher education and uneducated mothers, where uneducated is defined as mother did not attend any school. Column (2) assesses this heterogeneity between women living in rural areas and urban areas at the time of interview. Column (3) has effect on infant mortality for mothers having a wealth index as defined as poor or poorer vis-à-vis with mothers who are non-poor based on the wealth index being middle, richer, or richest. Column (4) specifies the effect of infant mortality for mothers who are employed, where employment has been categorized into 9 categories – Professional and managerial, clerical, sales, Agricultural self-employed, Agricultural employee, household and domestic, services, skilled manual and unskilled manual. Unemployed is defined for a mother who is not working. Data for employment status is not available for mothers in Angola and Nigeria in the DHS survey used. F-stat and the corresponding p-values for equality of coefficients are also reported.

*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

One of the main channels through which AGOA may affect women is by affecting their employment. The women who are working should benefit more than women who have no gainful employment. The results in Table 6 column (4) show this. Moreover, among the employed, women employed in agriculture and manual labor benefits the most since many of these countries were apparel, mineral, or agriculture exporting countries. This can be seen in Table 7. The infant mortality for mothers employed in the agriculture and manual labor sector, falls significantly by around 1.2 percentage points while those involved in household and services do not show a significant decline with respect to the fall in infant mortality for unemployed women. Therefore, increasing employment of women in export sectors with increasing agricultural or manual labor, leads to an increase in income, which leads to a reduction in infant mortality.[29] The income effect dominates leading to bettering of child health due to increased employment of mothers. Moreover, since rural, poor, and uneducated women are most likely to be employed in low-skilled export sectors, this section of the society benefits the most by the increase in employment.

**Table 7: Heterogeneity across different employment groupings for mothers**

| | **Treat** | **Agriculture** | **Manual Labor** | **Managerial Services** | **Household and Services** |
|---|---|---|---|---|---|
| **Infant Mortality** | 0.0063<br>(0.0040) | -0.0185***<br>(0.0035) | -0.0155***<br>(0.0043) | -0.0081***<br>(0.0026) | -0.0022<br>(0.0061) |
| **F-Stat** | 3.16<br>(0.041) | | | | |
| **Number of Countries** | 28 | | | | |
| **Number of mothers** | 148006 | | | | |
| **Observations** | 484754 | | | | |

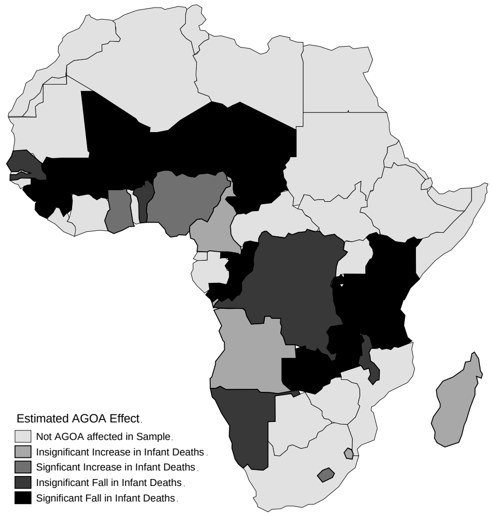
Note: The control variables are sex of child, whether born in multiple birth, birth order, birth month, mother fixed effects, country specific linear trends, mother's cohort by child birth year FE. All the coefficients are derived from the same regression. Robust standard errors clustered at country level are reported in brackets. Employment is categorized into four major sectors: (1) Agriculture - if the mother is working either as Agricultural self-employed or Agricultural employee, (2) Manual Labor - if the mother is employed as skilled manual or unskilled manual, (3) Managerial - if the mother is employed as Professional and managerial, clerical or sales, and (4) Household and services - if the mother is working in household or domestic services or the services sector. F-stat and corresponding p-value for equality of coefficients on employment categories is reported. Omitted category is the unemployed mothers. Data for employment status is not available for mothers in Angola and Nigeria in the DHS survey used.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

[29] Author's calculation using the information on assets collated across various DHS surveys suggests a decrease in probability of being poor after AGOA is implemented. Results available on request.

I also observe considerable heterogeneity across the AGOA-affected countries and across time. This is observed in Figure 5. The 12 countries experiencing the most significant fall in infant mortality are Burkina Faso, Burundi, Chad, Republic of Congo, Guinea, Kenya, Mali, Niger, Rwanda, Sierra Leone, Tanzania, and Zimbabwe. The magnitude ranges from a fall in infant deaths by 6.8 percentage points in Burkina Faso to 22.8 percentage points in Rwanda.[30] The variation in effect could in part be explained by the predominant exports from these countries. I use the trade volumes data from the Office of the United States Trade Representative, by commodity classification, to determine the main commodity of export at the 3-digit level. Table 8 (1) shows that countries producing and exporting agricultural products as well as mineral ores and petrol and metals gain the most from AGOA. These countries see a higher relative decline in infant mortality than others. On further disaggregation, the five countries with highest declines in infant mortality are Rwanda, Kenya, Mali, Tanzania, and Zambia which are also majorly agriculture, apparel or mineral exporting countries, that benefitted the most under AGOA. It has been argued that with Oil and Gas being the most valuable export to US with AGOA, it does not create long term benefits for the economy. The results are in line with this reasoning. In fact, Figure 5 shows that oil exporting countries like Angola and Nigeria see an increase in infant deaths (although statistically insignificant in case of Angola). It may well then be that resource rich countries are not able to effectively harness the developmental gains from trade.

[30] The full set of results for country effects are shown in Appendix Table A10.

**Figure 5: Heterogenous Estimated Country Effects of AGOA**




Note: The shaded areas show the different countries with different direction of estimated effects of trade on infant mortality. The coefficients and the empirical method to derive these estimates are presented in Appendix Table A10.

There are heterogeneous effects by predominant religion of the country as seen in Table 8 (2). Literature points towards the importance of ethnic groups and religion which may determine demographic factors like fertility, mortality, and child health. Predominantly Islamic countries experience a larger fall in infant mortality than predominantly Christian countries. Apart from religion playing a role in the fertility beliefs of the household (which this study cannot disentangle), at an aggregate level, many of the Islamic countries deal with mineral and ore exports, which experienced greater gains from AGOA.[31] In terms of regional heterogeneity, East Africa experience larger gains in magnitude in infant deaths as seen in Table 8 (4). Four out of the five nations in East Africa are predominantly agricultural exporters, which corroborates the previous finding. Table 8 column (3) shows that low-income countries in sub-Saharan Africa experience a significant decline in infant mortality due to AGOA vis-à-vis the middle-income countries. Even at the macro level, AGOA helps in leveling the disparities.

[31] Guinea, Niger, Sierra Leone are mineral exporters which saw a significant decline (refer Appendix Table A5)

## Table 8: Heterogeneity across different country groupings

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Dependent Variable** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** |
| **Apparel** | -0.00023<br>(0.0046) | | | |
| **Oil** | 0.00142<br>(0.0034) | | | |
| **Agricultural Products** | -0.0132***<br>(0.0031) | | | |
| **Mineral and ore** | -0.0109**<br>(0.0048) | | | |
| **Others** | -0.00764<br>(0.0057) | | | |
| **Non-Islamic** | | -0.0038<br>(0.0026) | | |
| **Islamic** | | -0.0107***<br>(0.0034) | | |
| **Low income countries** | | | -0.0094***<br>(0.0031) | |
| **Middle income countries** | | | 0.00012<br>(0.0038) | |
| **East** | | | | -0.0181***<br>(0.0031) |
| **West** | | | | -0.0064<br>(0.0038) |
| **Central** | | | | -0.0055<br>(0.0043) |
| **South** | | | | 0.0006<br>(0.0009) |
| **F-Stat** | 4.40<br>(0.0066) | 3.30<br>(0.079) | 6.02<br>(0.0204) | 20.21<br>(0.00) |
| **Number of Countries** | 30 | 30 | 30 | 30 |
| **Number of mothers** | 212738 | 212738 | 212738 | 212738 |
| **Observations** | 686093 | 686093 | 686093 | 686093 |

Note: The regressions control for sex of child, whether born in multiple birth, birth order, birth month, mother fixed effects, country specific linear trends, mother's age and child birth year FE. Standard errors clustered at country level are reported in brackets. The treatment is interacted with the different country groupings to get the treatment effect on those groups of countries. Column (1) includes separate effect of being affected by AGOA based on their predominant commodity of export. Countries with high volume of apparel exports are Kenya, Lesotho, Madagascar, Namibia, Malawi and Eswatini. Countries having majorly oil and gas exports are Angola, Congo, Cameroon, Nigeria and Democratic Republic of Congo. Countries which had highest share of agricultural products exports are Burkina Faso, Burundi, Rwanda, and Tanzania. Countries with major mineral and ore exports (includes petrol, coal, minerals and ores) were Guinea, Ghana, Niger, Sierra Leone, and Zambia. Products not being classified under these above categories have been labeled as "other exports". These include Forestry, animal and wood products, electronics chemicals etc. The countries exporting these types of products are Benin, Chad, Mali, Sao Tome and Principe, and Senegal. The data for predominant commodity of export from these countries into US has been collected from Office of the United States Trade Representative. Column (2) assesses heterogeneity in reduction in infant mortality for countries based on their predominant religion. Data for predominant religion of each country has been collected from CIA World Factbook. Column (3) divides the 30 countries based on World Bank's ranking of incomes into low- and middle-income countries. Column (4) analyzes the impact on the countries based on their geographic location in sub-Saharan Africa. F-stat and the corresponding p-values for equality of coefficients are also reported.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

I also provide evidence on the dynamics of the treatment for each of the AGOA affected countries separately in Appendix Figure A3. The analysis is conducted by constructing separate datasets for each AGOA affected country in the sample vis-à-vis the clean control countries which were not affected by AGOA. These graphs illustrate the lags that may occur in experiencing the effects of AGOA and explain the aggregate heterogenous effects we observe at the country level in Figure 5 and Appendix Table A10. The five countries with highest declines in infant mortality - Rwanda, Kenya, Sierra Leone, Tanzania, and Zambia – also show a lagged deeper effect of the policy overtime.

Although the changes in employment of women and export composition at the country level shed light on the possible mechanisms for the observed effect of AGOA, it should be pointed out that this analysis is not able to directly test changes on income, wages, hours worked, or consumption due to unavailability of such data in DHS. DHS provides an excellent opportunity to capture household level experience of child death due to the retrospective fertility histories but it does not contain data on possession of assets and other health care variables retrospectively. Moreover, any income or number of hours worked data are not a part of these questionnaires. A more case specific analysis may be required using either qualitative data collection from specific countries or other more detailed income and work surveys to carve out the different pathways for the effects.[32]

Lastly, I check for the robustness of the results using various specifications. These are presented in the online Appendix. One concern in this analysis may be that AGOA may have affected fertility differently so that mothers who gave birth in those years in AGOA countries are not the same as the mothers who gave birth in the same year in non-AGOA countries. I find no evidence of fertility selection bias (Online Appendix A). I test the relevance of the results for a balanced panel restricting the analysis to countries which received AGOA benefits in 2001 with respect to control countries, and I still find significant effects (Online Appendix B). Moreover, I relax different assumptions in the data and model specification and find the results are robust to different specifications (Online Appendix B). I also carry out an alternative specification based on assigning weights to the observations using propensity score weighting based on mother level characteristics

[32] However, no standardized income/work data is available for all the 30 countries when this analysis was conducted. Therefore, these pathways can only be looked at *within* particular countries and for a specific country at a time. This may be a useful research area for the future.

to eliminate selection bias on observables and find that the results hold with matching and weighting (Online Appendix C).

## 7 Discussion

The empirical study of the effect of trade on child mortality has been limited, and in many cases confounding. This paper uses a novel way of combining micro datasets across 30 sub-Saharan African countries to study the effect of a macroeconomic trade policy, AGOA, on infant and neonatal mortality and provides the first estimates. Using a difference-in-difference analysis with sibling fixed effects and event study analysis, this study is able to better control for confounding factors at the country and mother level like poor institutions, macroeconomic instability, geography, differential country specific trends in epidemics and health policies, time shocks like wars, inherent capability of the mother in raising a child etc. and hence is able to derive a causal estimate of trade on infant mortality. The results are robust to various specifications and time-varying controls at the country level. The reduced-form results indicate that, on average, AGOA reduces the probability of infant and neonatal deaths by 9-12% of the sample mean. Dynamics of the mechanism reveal differential effects of the policy over time.

The paper also finds evidence of differential benefits to different sections of the population. AGOA reduces infant death significantly for the uneducated, rural, and poor mothers. I also observe a greater decline in infant mortality for mothers employed in agriculture or manual labor. Together, these point towards higher benefits to women working in export-oriented agriculture or industries requiring low-skilled manual labor services, leading to better outcomes for rural and uneducated women, who are employed by these sectors. Income effect due to employment creation for women via trade seems to be a pathway through which we observe the improvement in child health. The benefits and heterogeneity we see at the micro level are reflected in the country level heterogeneity analysis. The benefits to the poor and uneducated are consistent with low income countries and countries with agricultural product exports benefitting more than countries with predominantly oil exports. Although this paper cannot investigate the change in hours worked or change in wages due to unavailability of consistent household-level data for all 30 countries, the heterogeneity analysis showcases that if trade policy is successful in creation of employment in the agricultural or manual labor sector, it may create better outcomes for socially disadvantaged groups of population via the income effect.

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## Online Appendix A

One concern in this analysis may be that AGOA may have affected fertility differently so that mothers who gave birth in those years in AGOA countries are not the same as the mothers who gave birth in the same year in non-AGOA countries. This may also differ by socio-economic status of women. To test this, I calculate the percentage of women of socio-economic status z giving birth in each country-cohort in year t by creating a panel for women aged 15 to 49 years of age.[33] The regression estimates the effect of AGOA on this fertility indicator. To account for changes in maternal age by year of child birth, differing by 'type' of woman, the regression controls for cohort-year-type fixed effects. These regressions also control for effect of birth cohort of women on fertility differing by country and 'type' using cohort-country-type fixed effects. The results, presented in Table A1, show that the coefficients are not significantly different from zero. Fertility selection bias does not seem to be a major concern in the estimations.

**Table A1: Fertility Selection Effect**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | **Fertility (All)** | **Fertility (Uneducated)** | **Fertility (Poor)** | **Fertility (Rural)** |
| **AGOA** | -1.242<br>(1.07) | -0.567<br>(0.981) | -1.261<br>(1.07) | -1.252<br>(1.135) |
| **AGOA*Woman's type** | | -0.265<br>(0.712) | 0.338<br>(0.839) | -0.101<br>(1.04) |
| **F-stat** | | 0.39<br>[0.54] | 0.5<br>[0.48] | 1.18<br>[0.29] |
| **Number of Countries** | 30 | 30 | 30 | 30 |
| **Observations** | 19250 | 38199 | 38290 | 38325 |

Note: The dependent variable is percentage of ('type' of) women giving birth. Woman's type is a dummy variable referring to if the woman is uneducated, poor or rural. For definitions of these, check notes in Table 1. (1) refers to all types of women, (2) to uneducated women, (3) to poor women and (4) to rural women. Standard errors clustered at the country level are reported in brackets. F-test reports F-statistics and its associated p-values in brackets for the null that the sum of coefficients on AGOA and on its interaction term with Woman's type is zero. All regressions control for country by woman's birth cohort fixed effects and year of giving birth by woman's birth cohort fixed effects which are also allowed to differ by woman's type.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

[33] Fertility(Z) = number of births(Z)/total number of women(Z)*100 where Z refers to socio-economic type of woman being uneducated, poor or rural. The types are segregated as there is evidence of these types of women systematically differing in their fertility/mortality behavior (Paxson and Schady, 2005).

## Online Appendix B

This section provides robustness to various specifications of the model, the data selection process, and robustness to placebo effects.

There may be concerns about whether it is the pre-conditions that are required for a sub-Saharan African country to become AGOA eligible that is bringing about the change or it is implementation of AGOA and the changes in government policies thereafter which is helping in reducing infant mortality. This has been addressed in Figure 3. If it were the pre-conditions that were making infant mortality fall then we would have seen the drop even before AGOA was implemented; which is not the case. As an additional check I divide the group of countries into two – one who got AGOA status in 2001 and in the other group those who got later. It may be argued that countries who got the AGOA status in 2001, were already "ready" while those which got later, needed to work on pre-requisites to get themselves an AGOA Beneficiary status. Hence, the estimates for the latter group should be bigger and significant if in fact it is the preconditions which lead to a fall in infant deaths. Table A2 (1) shows the results. It is observed that the group of countries which got AGOA status later does not significantly do better than those countries which got their AGOA status earlier in 2001 and in fact the group of countries which got the status earlier is more effective in decreasing infant mortality. Table A2 (1) also provides a check for a balanced panel. If the analysis is run with all the treated countries receiving the treatment in 2001, we find statistically significant and a bigger effect for these group of countries.

I run additional robustness tests to include different cut offs for dropping the sample based on various year of birth of children in Table A2 (2). The result is robust to using different years as cut offs on both ends for the mortality estimates. There may exist bunching of deaths at 12 months in the data in DHS. To account for this, I redefine the infant mortality variable to include children who died at 12 months as well. The results are presented in Table A2 (4). This does not change the effect of AGOA on infant mortality and the effect is still statistically significant with a fall in 0.8 percentage points in infant mortality.

I then re-estimate the models using different definitions for treatment to AGOA. In my first specification in Table A2 (3), a model is estimated where in instead of choosing an indicator variable to indicate the presence of AGOA policy, percentage change in trade volumes interacted with an indicator of the country becoming AGOA eligible, is used as the independent variable. Table A2 (3) indicates that the coefficient is still negative and significant. A 1% increase in percentage in trade volumes in AGOA affected countries decreases the probability of an infant dying by 0.0014 percentage points. With year-to-year increases about 100% for some countries, this means it decreases the probability of infant dying by 0.14 percentage points. But, as pointed out earlier, it should be kept in mind that trade volumes in fact embody different aspects of an economy which may correlate with infant mortality and hence may not provide the best estimate.

It could be argued that countries that got treated with AGOA earlier in 2001 are different from countries that got AGOA later and hence a treatment variable is defined such that it takes the intensity of treatment into account. In Table A2 (3), I redefine the treatment variable such that it differs by number of years exposed to AGOA. The countries that got AGOA earlier in 2001 get a value 2 for treatment while those that got AGOA after 2001, get a value of 1. The never treated countries in the sample get a value of zero. The resulting coefficient remains statistically significant at about 0.4 percentage points fall in infant deaths. This fall in magnitude could be due to the heterogeneity in fall in infant mortality for those countries that got AGOA in 2001 vis-à-vis later. Lastly, in defining the treatment variable, instead of incorporating in-utero exposure, I redefine the AGOA exposure such that if date of birth of child is greater than implementation of AGOA, that child is considered treated. I show the results in Table A2 (3). The results are unchanged from the original specification.

Placebo tests by allocating fake timing of AGOA is also carried out to rule out spurious effects of policy change.[34] I re-estimate the effect of AGOA on infant mortality by assuming that AGOA has been implemented 1 to 5 years before the actual implementation. The t-statistics for each of the regressions is given in Table A3. I expect to observe no significant effect of the fake treatment. None of the t-statistics for the previous periods are significant at the standard levels.

Table A4 regenerates the impact of AGOA (where treatment is defined as the date of birth of child being greater than AGOA implementation) with different clustering of standard errors. I cluster at the PSU level and the mother cohort level, apart from the usual country level. The results are significant in all specifications and the largest standard errors are in fact reported at the country level, indicating there could be correlations at the country level that could be ignored if we cluster at a more granular level. The most conservative standard errors are therefore reported in the main regressions.

[34] I also carry out another test where I allocate AGOA treatment and year of the treatment to the countries in the sample, randomly from a uniform distribution. I do not find any significant results. Results are available on request.

**Table A2: Effect of AGOA on different specifications**

| | (1) | | (2) | | (3) | | | (4) |
|---|---|---|---|---|---|---|---|---|
| **Dependent Variable** | **Infant Mortality** | **Infant Mortality** | **Neonatal Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** |
| **Sample** | AGOA in 2001 | AGOA after 2001 | No YOB cutoff | 1993< YOB< 2008 | % change in trade volumes | Intensity of treatment | Child date of birth> AGOA Date | Including death at 12 months |
| **Treatment** | -0.0156*** | -0.0025 | -0.005*** | -0.0082*** | -0.000014** | -0.0041** | -0.0069*** | -0.0085*** |
| | (0.0039) | (0.0064) | (0.0014) | (0.0028) | (0.0027) | (0.0019) | (0.0024) | (0.0030) |
| **Explanatory Variables** | YES | YES | YES | YES | YES | YES | YES | YES |
| **Country time trend** | YES | YES | YES | YES | YES | YES | YES | YES |
| **Mother FE** | YES | YES | YES | YES | YES | YES | YES | YES |
| **Cohort Year FE** | YES | YES | YES | NO | NO | NO | NO | NO |
| **Number of countries** | 25 | 10 | 30 | 30 | 30 | 30 | 30 | 30 |
| **Number of mothers** | 176295 | 69667 | 218303 | 197170 | 209970 | 212738 | 212738 | 212738 |
| **Observation** | 559498 | 218110 | 845484 | 536624 | 635844 | 686093 | 686093 | 686093 |

Note: YOB stands for year of birth. The covariates are sex of child, whether born in multiple birth, birth order, birth month, mother's age, birth year. Standard errors clustered at country level are reported in brackets. (1) includes effect of trade on infant mortality in AGOA affected countries vis-à-vis non-AGOA countries, where AGOA was implemented in 2001 in column 1 vis-à-vis those nations where AGOA was implemented after 2001. (2) includes robustness check for birth year cut off for children in sample. (3) redefines the independent variable to percentage change in trade volumes from previous year for AGOA vs. non-AGOA countries. (3) also checks for another definition of treatment, with treatment taking the value 2 for AGOA affected countries in 2001, 1 for countries getting AGOA after 2001 and 0 for not being AGOA affected in the sample. The last column in (3), redefines treatment by comparing the actual AGOA implementation date and the date of birth of the child to create treatment. (4) redefines the dependent variable to include deaths at 12 months as well.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

**Table A3: Placebo test – False timing of AGOA**

| | **5 years before** | **4 years before** | **3 years before** | **2 years before** | **1 year before** | **Actual** |
|---|---|---|---|---|---|---|
| **Infant** | 0.006 | 0.0030 | 0.0032 | -0.0004 | -0.0023 | -0.0071** |
| **Mortality** | (1.58) | (0.69) | (0.70) | (-0.11) | (-0.68) | (-2.53) |
| **Number of countries** | 30 | | | | | |
| **Number of mothers** | 212738 | | | | | |
| **Observations** | 686093 | | | | | |

Note: Each cell represents a different regression. The explanatory variables included in the specifications are sex of child, whether born in multiple birth, birth order, birth month, mother's age, country specific linear trends, year fixed effects and mother fixed effects. Standard errors are clustered at country level. Resulting t-statistics are reported in brackets. These are placebo test run to test if there are false effects of AGOA on infant mortality before AGOA has been actually implemented.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

**Table A4: Robustness to clustering standard errors at different levels**

| | Specification 1 | | | Specification 2 | | |
|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
| **Dependent Variable** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** |
| **Treatment** | -0.0069*** | -0.0069*** | -0.0069*** | -0.0068** | -0.0068*** | -0.0068*** |
| | (0.0024) | (0.0018) | (0.0018) | (0.0024) | (0.0018) | (0.0018) |
| **Explanatory Variables** | YES | YES | YES | YES | YES | YES |
| **Cluster Level** | Country | PSU | Mother | Country | PSU | Mother |
| **Mother FE** | YES | YES | YES | YES | YES | YES |
| **Cohort-year FE** | NO | NO | NO | YES | YES | YES |
| **Number of countries** | 30 | 30 | 30 | 30 | 30 | 30 |
| **Number of mothers** | 212738 | 212738 | 212738 | 212738 | 212738 | 212738 |
| **Observations** | 686093 | 686093 | 686093 | 686093 | 686093 | 686093 |

Note: Treatment is defined as 1 for a child born after AGOA has been implemented in an AGOA affected country. The other control variables included in the specifications are sex of child, whether born in multiple birth, year of birth, mother's age at birth, birth order, birth month, and country time trends. Standard errors clustered at the various levels are reported in brackets. PSU are primary sampling units at which the sample is clustered in DHS and is provided in the survey. Specification 2 allows for changing mother's age at birth for different year of birth of child. Hence, controls for mother's age and year of birth of child are subsumed in these specifications.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

To alleviate the concern that the results may be driven by one outlier country, Table A5 shows that the result is robust to dropping one country at a time implying that these are not driven by changes due to an outlier country. Also, since there are 30 countries and there may be country specific differences in birth order or mother's age trend, Table A6 controls additionally for country specific birth order dummy and country specific mother's age quadratic trend. The magnitude and significance of the coefficient derived in the main specification is unchanged, implying that the result is robust to differing trends and decline among countries in birth order and mother's age.

**Table A5: Robustness Check – Dropping one country at a time**

| Dependent Variable | Infant Mortality | Infant Mortality | Infant Mortality | Infant Mortality | Infant Mortality | Infant Mortality | Infant Mortality |
|---|---|---|---|---|---|---|---|
| | **(1)Angola** | **(2)Benin** | **(3) Burkina Faso** | **(4)Burundi** | **(5)Cameroon** | **(6)Chad** | **(7)Congo** |
| **Treatment** | -0.00761** (0.0029) | -0.00737** (0.0026) | -0.00732** (0.0025) | -0.00613* (0.0032) | -0.0077** (0.0031) | -0.00859*** (0.0027) | -0.0068** (0.0026) |
| | **(8)Congo, Dem.** | **(9)Cote d'Ivoire** | **(10)Ethiopia** | **(11)Ghana** | **(12)Guinea** | **(13)Kenya** | **(14)Lesotho** |
| **Treatment** | -0.00849*** (0.0026) | -0.00648** (0.0026) | -0.00686** (0.0026) | -0.00703** (0.0026) | -0.00681** (0.0026) | -0.00664** (0.0026) | -0.00677** (0.0026) |
| | **(15)Liberia** | **(16)Madag-ascar** | **(17)Malawi** | **(18) Mali** | **(19)Mozam-bique** | **(20)Namibia** | **(21)Niger** |
| **Treatment** | -0.00665** (0.0027) | -0.00733** (0.0027) | -0.00713** (0.0028) | -0.00653** (0.0026) | -0.00667** (0.0026) | -0.00682** (0.0026) | -0.00688** (0.0026) |
| | **(22)Nigeria** | **(23)Rwanda** | **(24)Sao Tome & Principe** | **(25)Senegal** | **(26)Sierra Leone** | **(27)Eswatini** | **(28)Tanz-ania** |
| **Treatment** | -0.00737*** (0.0026) | -0.00688** (0.0026) | -0.00698** (0.0026) | -0.00748*** (0.0026) | -0.00597** (0.0025) | -0.00715** (0.0026) | -0.00654** (0.0026) |
| | **(29)Zambia** | **(30)Zimba-bwe** | | | | | |
| **Treatment** | -0.00677** (0.0026) | -0.00616** (0.0025) | | | | | |

Note: The explanatory variables included in the specifications are sex of child, whether born in multiple birth, birth order, birth month, mother fixed effects, country specific linear trends, mother's cohort by child birth year FE. Standard errors clustered at country level are reported in brackets. In each of the separate regressions, one of the countries is dropped at a time in alphabetical order.

**Table A6: Robustness Check – Birth order and Mother's Age**

| | **(1)** | **(2)** |
|---|---|---|
| **Dependent Variable** | **Infant Mortality** | **Infant Mortality** |
| **Specification** | Country Specific Birth Order | Country Specific mother's age quadratic trends |
| **Treatment** | -0.00688** (0.0025) | -0.00681** (0.0026) |
| **Explanatory Variables** | YES | YES |
| **Country Specific Birth Order Dummy** | YES | YES |
| **Country specific mother's age quadratic trend** | NO | YES |
| **Mother FE** | YES | YES |
| **Cohort-year FE** | YES | YES |
| **Number of countries** | 30 | 30 |
| **Number of mothers** | 212738 | 212738 |
| **Observations** | 686093 | 686093 |

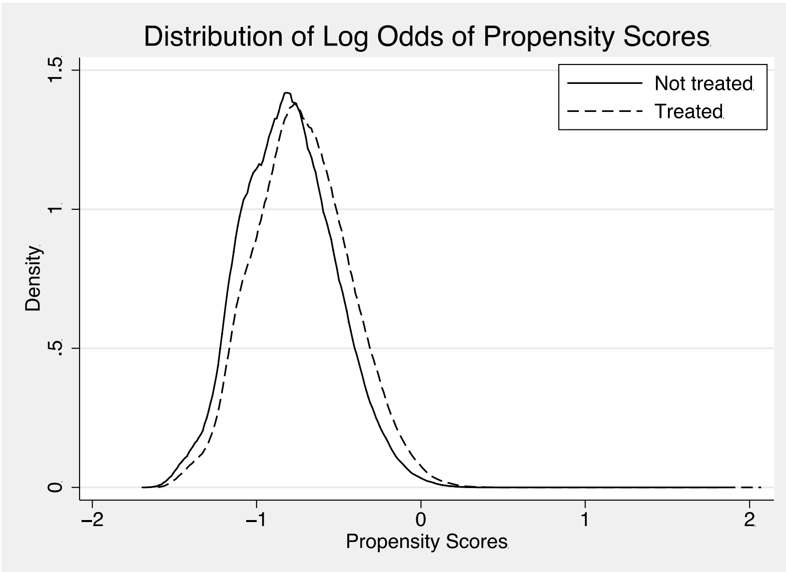
Note: The other control variables included in the specifications are sex of child, whether born in multiple birth, year of birth, mother's age at birth, birth order, birth month, and country time trends. Standard errors clustered at the country level are reported in brackets. *** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

## Online Appendix C

This section checks for selection. Difference-in-Difference analysis will be biased if there are certain kind of mothers who select into treatment. Note that since the analysis is at the household level and we are looking at mothers giving birth before and after AGOA, if this group behaves differently than the control, that may create bias. We checked in Online Appendix A that there is no change in fertility due to AGOA. Since the effects are coming from within the household, rather than at the country-level, we have a more robust control in terms of observing the same mother before and after the treatment. This is different than if we did a country-level analysis where we would be comparing across heterogenous countries which would mostly necessitate a process of creating synthetic cohorts by matching to create robust control group. However, the analysis checks if there are any differences if we supplement the DID analysis with weights generated from propensity score matching, especially as there is potential for heterogenous treatment effects.

This analysis uses mother characteristics that could guide certain kinds of mothers to select into treatment like mother's age, rural or urban residence, whether the mother has no schooling, primary, secondary, or higher schooling, and the wealth index of the mother in creating propensity weights for the main regression. Figure A1 presents the distribution of log odds of propensity scores between the treated and control groups and we find they are very close in distribution. I then create Inverse Probability of Treatment (IPT) weights and SMR weights. Inverse probability treatment weights are the inverse of the estimated Propensity Score for treated groups and the inverse of one minus the estimated Propensity Scores for control patients. SMR weighting reweights the control group to be representative of the treated group. Propensity Score weighting helps in reweighting the individuals within the original treated and control groups to create a "pseudo-population" with no association between the confounders and treatment. The results, after reweighting are presented in Table A7. The magnitude and significance of coefficients remain similar to the original estimates derived, assuaging concerns about selection effects.

**Figure A1: Kernel density of Log Odds of Propensity Scores**




Note: These are the Kernel density estimates of the Log Odds or Propensity Scores generated from a Logistic regression of the probability of treatment on mother characteristics like mother's age, rural or urban residence, whether the mother has no schooling, primary, secondary, or higher schooling, and the wealth index, for both the treated and control groups.

**Table A7: Propensity Score Reweighted Regressions**

| | **(1)** | **(2)** | **(3)** |
|---|---|---|---|
| **Dependent Variable** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** |
| **Treatment** | -0.0069** | -0.0067** | -0.0073** |
| | (0.0030) | (0.0032) | (0.0026) |
| **Explanatory Variables** | YES | YES | YES |
| **Weight** | IPT | SMR | IPT |
| **Country FE** | YES | YES | YES |
| **Cohort-year FE** | NO | NO | YES |
| **Number of countries** | 30 | 30 | 30 |
| **Observations** | 686075 | 686075 | 686075 |

Note: The other control variables included in the specifications are sex of child, whether born in multiple birth, year of birth, mother's age at birth, birth order, birth month, and country time trends. Standard errors clustered at the country level are reported in brackets. IPT and SMR are calculated weights from propensity scores generated from a logistic regression.

*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

## Online Appendix – Other Tables and Figures

**Table A8: List of 30 countries in sub-Saharan Africa used in the study, categorized by AGOA Eligibility, year made AGOA eligible, DHS survey used and sample period of births**

| Sub-Saharan Africa | AGOA Eligible | Year made AGOA Eligible | DHS used | Sample period |
|---|---|---|---|---|
| Angola | Y | December 30, 2003 | 2011 | 1990-2010 |
| Benin | Y | October 2, 2000 | 2006 | 1990-2005 |
| Burkina Faso | Y | December 10, 2004 | 2010 | 1990-2009 |
| Burundi | Y | January 1, 2006 | 2010 | 1990-2010 |
| Cameroon | Y | October 2, 2000 | 2011 | 1990-2010 |
| Chad | Y | October 2, 2000 | 2004 | 1990-2003 |
| Republic of the Congo | Y | October 2, 2000 | 2005 | 1990-2004 |
| Democratic Republic of the Congo | Y | October 31, 2003 – Suspended 2011 | 2007 | 1990-2006 |
| Cote d'Ivoire | Y | 2003 – Suspended 2005; restored 2011 | 2005 | 1990-2003 |
| Ethiopia | Y | October 2, 2000 | 2011 | 1990-2002 |
| Ghana | Y | October 2, 2000 | 2008 | 1990-2007 |
| Guinea | Y | 2000- Suspended 2009; restored 2011 | 2005 | 1990-2004 |
| Kenya | Y | October 2, 2000 | 2008-09 | 1990-2008 |
| Lesotho | Y | October 2, 2000 | 2009 | 1990-2009 |
| Liberia | Y | December 29, 2006 | 2007 | 1990-2006 |
| Madagascar | Y | 2000-Suspended 2009; restored 2014 | 2008-09 | 1990-2008 |
| Malawi | Y | October 2, 2000 | 2010 | 1990-2009 |
| Mali | Y | 2000 – Suspended 2012; restored 2014 | 2006 | 1990-2005 |
| Mozambique | Y | October 2, 2000 | 2003 | 1990-2002 |
| Namibia | Y | October 2, 2000 | 2006-07 | 1990-2006 |
| Niger | Y | 2000-Suspended 2009; restored 2011 | 2006 | 1990-2005 |
| Nigeria | Y | October 2, 2000 | 2010 | 1990-2009 |
| Rwanda | Y | October 2, 2000 | 2010 | 1990-2009 |
| Sao Tome and Principe | Y | October 2, 2000 | 2008-09 | 1990-2008 |
| Senegal | Y | October 2, 2000 | 2010-11 | 1990-2010 |
| Sierra Leone | Y | October 23, 2002 | 2008 | 1990-2007 |
| Eswatini | Y | October 2, 2000 | 2006-07 | 1990-2006 |
| Tanzania | Y | October 2, 2000 | 2010 | 1990-2009 |
| Zambia | Y | October 2, 2000 | 2007 | 1990-2006 |
| Zimbabwe | N | Non-AGOA | 2010-11 | 1990-2009 |

Note: Since Liberia has sample size till 2006 and AGOA was implemented in 2006 for the country, it effectively in the sample behaves as not being AGOA affected. Similarly, for Cote d'Ivoire, Ethiopia and Mozambique, since I merge the last year data with previous year due to few data points in the final year, these countries effectively behave as not affected by AGOA.

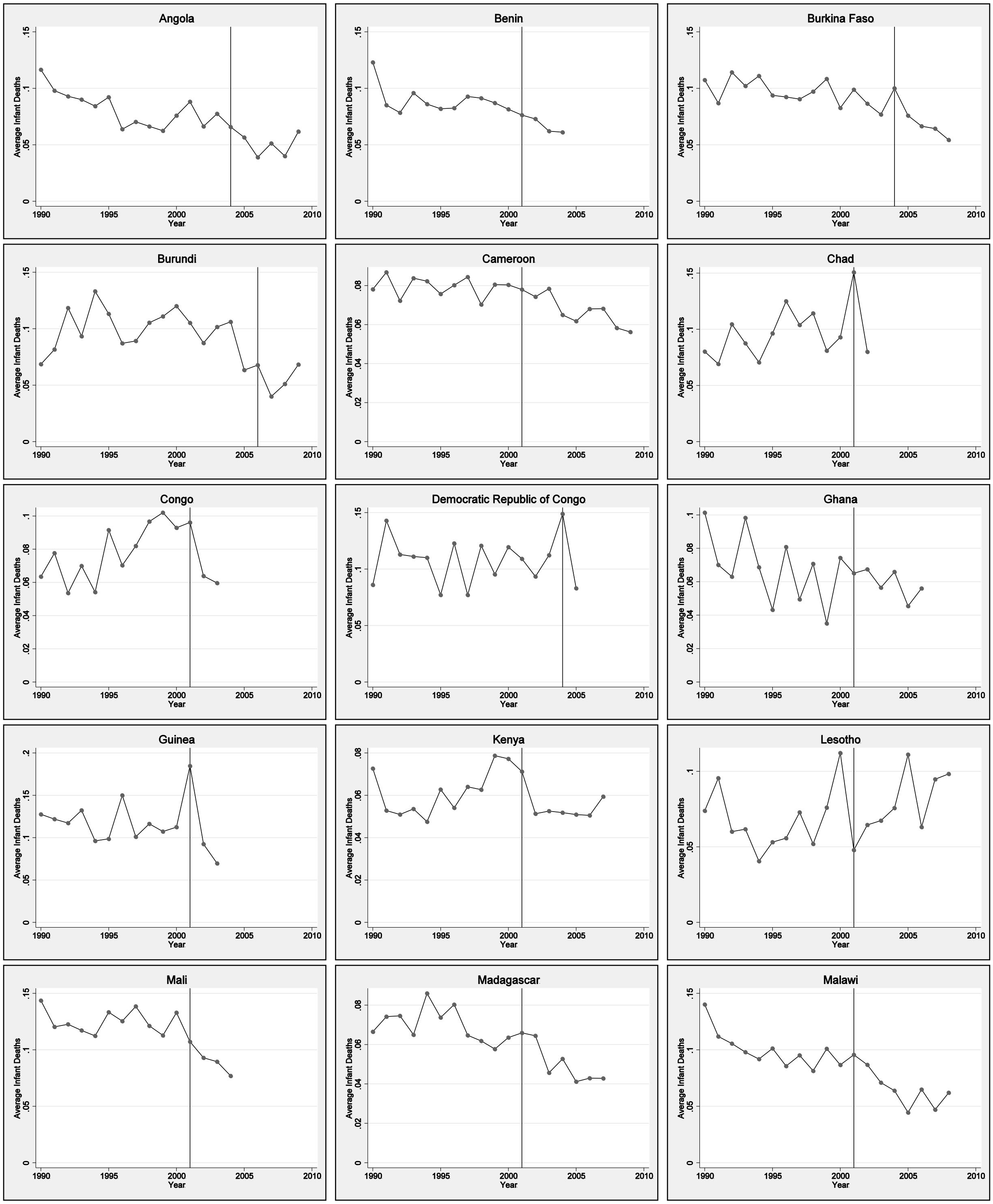



**Figure A2(a): Sample mean infant mortality rates by country for AGOA affected countries overtime, 1990 onwards**

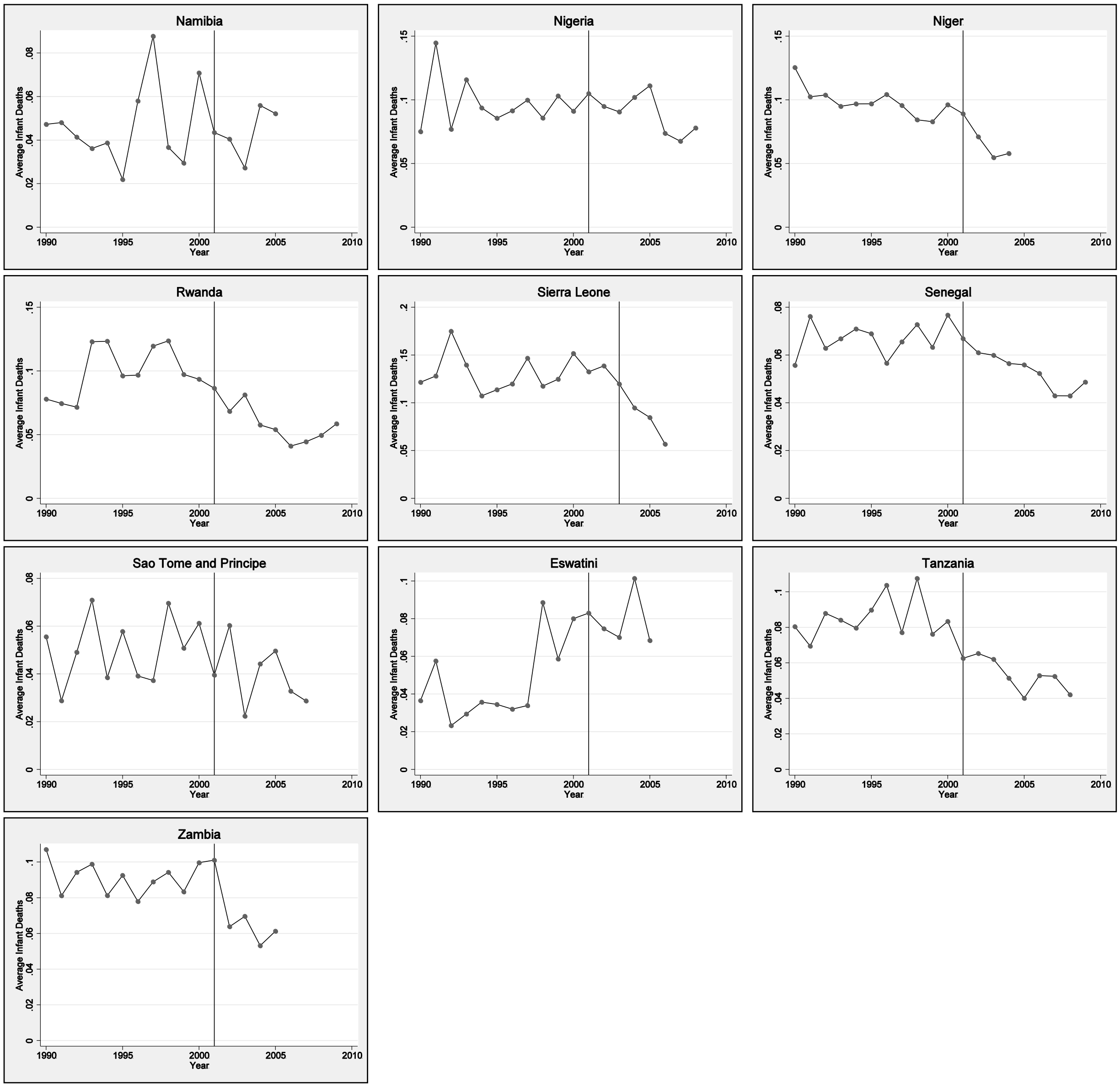



**Figure A2(b): Sample mean infant mortality rates by country for AGOA affected countries overtime, 1990 onwards**

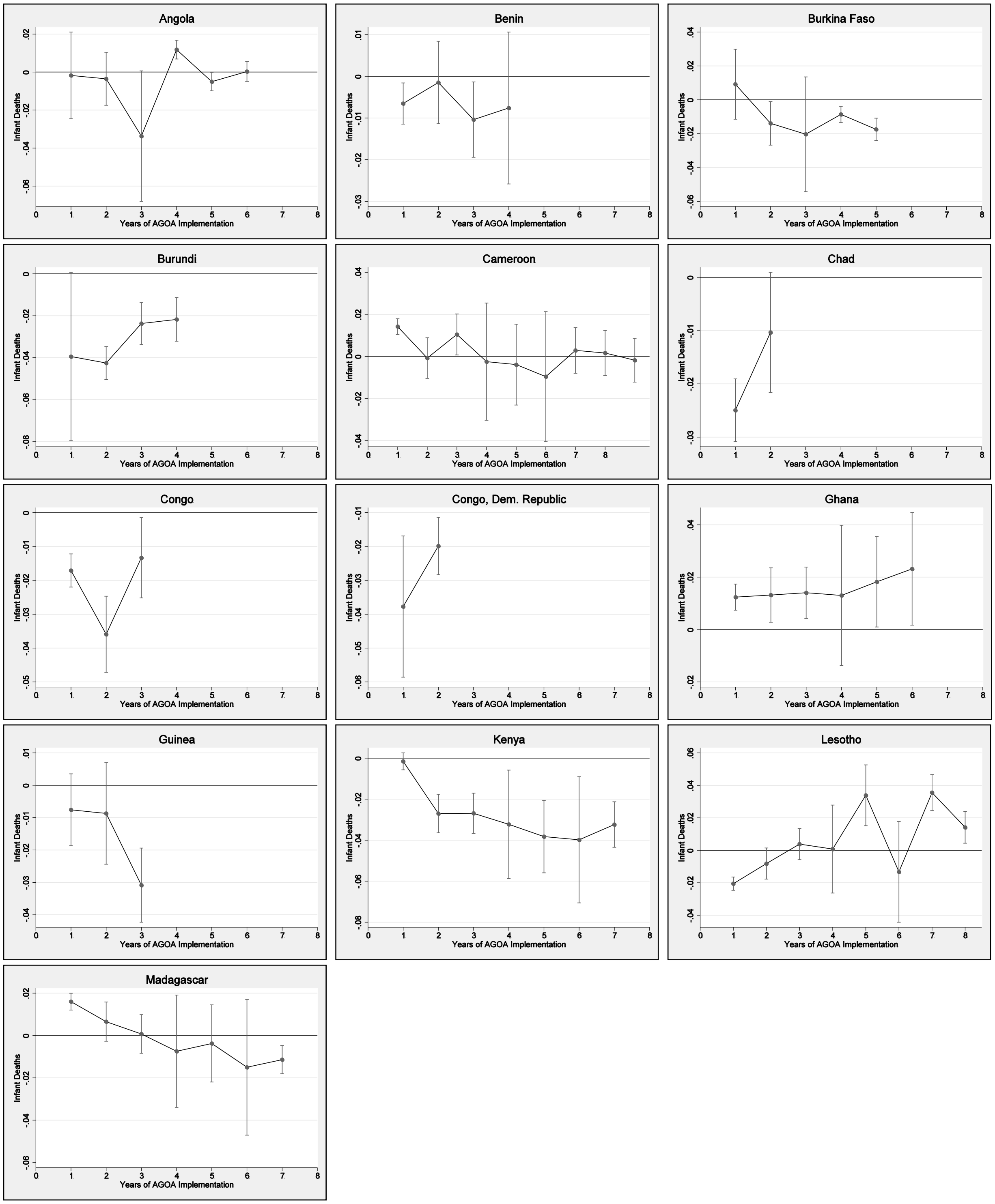



**Figure A3(a): Heterogeneity in dynamics of treatment effects for AGOA affected countries overtime, since implementation of AGOA**

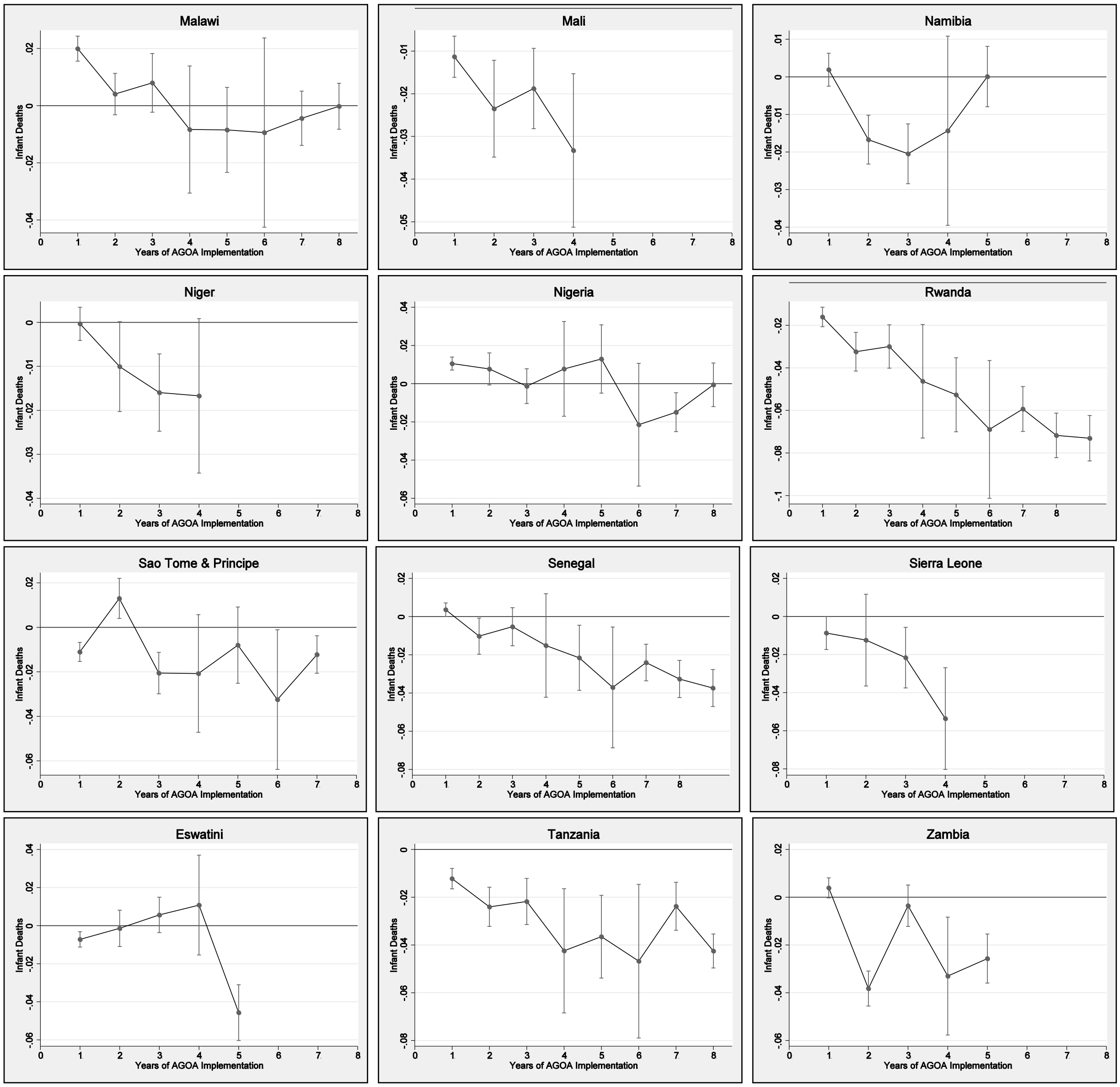



**Figure A3(b): Heterogeneity in dynamics of treatment effects for AGOA affected countries overtime, since implementation of AGOA**

**Table A9: Point estimates for dynamics of infant and neonatal mortality**

| | (1) | (2) |
|---|---|---|
| **Dependent Variable** | **Infant Mortality** | **Neonatal Mortality** |
| **T=0** | -0.0053<br>(0.0049) | -0.0009<br>(0.0028) |
| **T=1** | -0.0061<br>(0.0045) | -0.0036*<br>(0.0019) |
| **T=2** | -0.0084<br>(0.0061) | -0.0041<br>(0.0037) |
| **T=3** | -0.0109<br>(0.0088) | -0.0057*<br>(0.0031) |
| **T=4** | -0.0090<br>(0.010) | -0.0054<br>(0.0039) |
| **T=5** | -0.0046<br>(0.0118) | -0.0012<br>(0.0051) |
| **T=6** | -0.0098<br>(0.018) | -0.0030<br>(0.0062) |
| **T=7** | -0.0086<br>(0.016) | -0.0045<br>(0.0064) |
| **T=8** | -0.017<br>(0.018) | -0.0078<br>(0.0062) |
| **Number of countries** | 30 | 30 |
| **Observations** | 686093 | 686093 |

Note: This is a semi dynamic model of trade estimated from a single equation:
$IMR_{imct} = \alpha_m + \beta_t + \sum_{j=0}^{infinite} \theta_j.1\{J_{c,t}=j\} + X_{imct}'\delta + \mu_c.t + \varepsilon_{imc}$
The explanatory variables included in the specifications are sex of child, whether born in multiple birth, birth order, birth month, mother's age, country specific linear trends, year fixed effects and mother fixed effects. The end years have not been binned to allow flexibility and therefore should be interpreted with caution due to changing composition of countries. Standard errors clustered at country level are reported in brackets.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

## Table A10: Effect of AGOA on infant mortality by year and country

### Time effects:

| | 2002 | 2003 | 2004 | 2005 | 2006 | 2007 | 2008 | 2009 |
|---|---|---|---|---|---|---|---|---|
| **Infant** | -0.0059 | -0.0069** | -0.0118 | -0.0028 | 0.0028 | -0.0084** | -0.014*** | -0.0104 |
| **Mortality** | (0.0042) | (0.0030) | (0.0073) | (0.0102) | (0.0082) | (0.0034) | (0.0048) | (0.0061) |
| **Number of countries** | 30 | | | | | | | |
| **Number of mothers** | 212738 | | | | | | | |
| **Observa-tions** | 686093 | | | | | | | |

Note: These are estimates of AGOA interacted with the year dummies for each AGOA year. The explanatory variables included in the specifications are sex of child, whether born in multiple birth, birth order, birth month, mother's age at birth, country time trend and mother fixed effects. Standard errors clustered at country level are reported in brackets.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

### Country effects:

| **Dependent Variable** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** | **Infant Mortality** |
|---|---|---|---|---|---|---|
| | **(1)Angola** | **(2)Benin** | **(3)Burkina Faso** | **(4)Burundi** | **(5)Cameroon** | **(6)Chad** |
| **Treatment** | 0.0043<br>(0.0026) | -0.0022<br>(0.0043) | -0.0068**<br>(0.0027) | -0.0148***<br>(0.0024) | 0.0029<br>(0.0044) | -0.0089*<br>(0.0044) |
| | **(7)Congo** | **(8)Congo, Dem.** | **(11)Ghana** | **(12)Guinea** | **(13)Kenya** | **(14)Lesotho** |
| **Treatment** | -0.0154***<br>(0.0037) | -0.0019<br>(0.0033) | 0.0175***<br>(0.0045) | -0.0157***<br>(0.0039) | -0.0217***<br>(0.0043) | 0.0179***<br>(0.0044) |
| | **(16)Mada-gascar** | **(17)Malawi** | **(18) Mali** | **(20)Namibia** | **(21)Niger** | **(22)Nigeria** |
| **Treatment** | 0.0026<br>(0.0044) | -0.00073<br>(0.0043) | -0.0209***<br>(0.0042) | -0.00668<br>(0.0044) | -0.0104**<br>(0.0042) | 0.00858*<br>(0.0044) |
| | **(23)Rwanda** | **(24)Sao Tome & Principe** | **(25)Senegal** | **(26)Sierra Leone** | **(27)Eswatini** | **(28)Tanzania** |
| **Treatment** | -0.0223***<br>(0.0043) | 0.0057<br>(0.0045) | -0.0037<br>(0.0044) | -0.0175***<br>(0.0030) | 0.0072<br>(0.0043) | -0.0176***<br>(0.0044) |
| | **(29)Zambia** | | | | | |
| **Treatment** | -0.0186***<br>(0.0044) | | | | | |

Note: These are estimates of AGOA interacted with the country dummies for each 25 AGOA affected country in the sample. The explanatory variables included in the specifications are sex of child, whether born in multiple birth, birth order, birth month, mother's age at birth, country time trend and mother fixed effects. Standard errors clustered at country level are reported in brackets.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.

### Table A11: AGOA affected countries and trade volumes

| Country | AGOA Year | Trade/GDP Change | Percent Change in Export Volumes to US |
|---|---|---|---|
| Angola | 30-Dec-03 | 5.965 | 218.66 |
| Benin | 2-Oct-00 | -0.024 | -19.22 |
| Burkina Faso | 10-Dec-04 | 0.028 | 518.3 |
| Burundi | 1/1/06 | 0.18 | 116.09 |
| Cameroon | 2-Oct-00 | 0.341 | 113 |
| Chad | 2-Oct-00 | 29.639 | 69573.5 |
| Congo | 2-Oct-00 | 12.516 | 355.13 |
| Congo, Democratic Rep. | 31-Oct-03 | 1.055 | 196.76 |
| Cote D' Ivoire | 31-Oct-03 | 3.619 | 237.57 |
| Ethiopia | 2-Oct-00 | 0.215 | 403.9 |
| Ghana | 2-Oct-00 | 1.009 | 280.92 |
| Guinea | 2-Oct-00 | -0.636 | -8.73 |
| Kenya | 2-Oct-00 | 0.479 | 246.49 |
| Lesotho | 2-Oct-00 | 6.607 | 173.9 |
| Liberia | 29-Dec-06 | -1.866 | 13.04 |
| Madagascar | 2-Oct-00 | -1.343 | -44.77 |
| Malawi | 2-Oct-00 | -0.377 | 17.09 |
| Mali | 2-Oct-00 | -0.125 | -58.5 |
| Mozambique | 2-Oct-00 | -0.205 | 43.34 |
| Namibia | 2-Oct-00 | 3.054 | 869.37 |
| Niger | 2-Oct-00 | 4.743 | 4007.98 |
| Nigeria | 2-Oct-00 | 2.619 | 221.27 |
| Rwanda | 2-Oct-00 | 0.302 | 509.41 |
| Sao Tome & Principe | 2-Oct-00 | 0.476 | 91.29 |
| Senegal | 2-Oct-00 | 0.002 | 59.92 |
| Sierra Leone | 23-Oct-02 | 0.737 | 477.43 |
| Eswatini | 2-Oct-00 | 0.29 | 69.82 |
| Tanzania | 2-Oct-00 | -0.018 | 83.08 |
| Zambia | 2-Oct-00 | 0.042 | 166.95 |

Note: These are the percent volume changes in exports to US and Trade/GDP changes after AGOA for all the AGOA affected countries. The last year in the sample is 2001. Trade/GDP change is calculated as the difference between the two ratios in 2011 vis-à-vis the year before AGOA was implemented in the country, multiplied by 100. Percent change in export volumes are calculated as the difference between export volumes in 2011 vis-à-vis the year before AGOA, with reference to the year before AGOA. I use the trade volumes data from the Office of the United States Trade Representative and GDP at Constant 2010 prices from the World Bank dataset.

## Table A12: Effect on the likelihood of child death

| | (1) | (2) |
|---|---|---|
| **Dependent Variable** | **Infant Mortality** | **Neonatal Mortality** |
| **T-5** | -0.000008<br>(0.0078) | -0.0004<br>(0.0029) |
| **T-4** | -0.00017<br>(0.0049) | -0.00009<br>(0.0023) |
| **T-3** | -0.00028<br>(0.0034) | -0.0034*<br>(0.0019) |
| **T-2** | -0.0036<br>(0.0046) | -0.0005<br>(0.0025) |
| **F-stat**<br>**PRE** | 0.39<br>(0.8135) | 0.77<br>(0.553) |
| **T** | -0.0078<br>(0.0068) | -0.0049<br>(0.0037) |
| **T+1** | -0.0071*<br>(0.0036) | -0.0057**<br>(0.0022) |
| **T+2** | -0.0072<br>(0.0078) | -0.0066<br>(0.0053) |
| **T+3** | -0.0139*<br>(0.0076) | -0.0103**<br>(0.0040) |
| **T+4** | -0.0052<br>(0.0088) | -0.0072<br>(0.0046) |
| **T+5** | -0.0004<br>(0.0112) | -0.0047<br>(0.0068) |
| **F-stat**<br>**POST** | 3.46<br>(0.0131) | 3.35<br>(0.0153) |
| **Number of countries** | 25 | 25 |
| **Observations** | 594560 | 594560 |

Note: These are the θj estimates derived from estimating this equation:
$Death_{imct} = \alpha_m + \beta_t + \sum_{j=-5}^{5} \theta_j T_{c,t+j} + X_i'\delta + \varepsilon_{imct}$. All coefficients are from the same regression. The sample is restricted to treated countries. The standard errors are clustered at the country level and reported in brackets. The control variables are whether born in multiple birth, birth order, birth month, mother's age at birth, mother's education, place of residence, and asset index. Both the specifications control for year and mother fixed effects. F-statistics and the corresponding p-values are reported for the joint significance of pre-treatment years and post-treatment years.
*** Significant at 1% level, ** significant at 5% level, * significant at 10% level.