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Evidence from Firm-Level Data

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Résumé

Cet article analyse l'impact de l'aide au développement sur la croissance des entreprises pour un panel de 4342 entreprises de 29 pays en développement, dont 11 situés en Afrique. Avec des données issues des Enquêtes Entreprises de la Banque mondiale et en tenant compte des effets spécifiques à chaque firme, nous trouvons un impact positif de l'aide au développement sur la croissance des entreprises. Ce résultat apparait robuste à plusieurs tests, notamment à l'instrumentation de l'aide et aux estimations sur des sous-échantillons variés. Nous montrons ensuite que l'effet positif de l'aide est particulièrement élevé pour les entreprises opérant dans des secteurs avec des besoins intensifs en infrastructure et en financements externes. Ce résultat suggère que l'aide au développement améliore la performance des entreprises en atténuant les contraintes d'infrastructure et de financement existantes dans les pays en développement.

Mots-clés: Aide publique au développement. Croissance des entreprises. Contrainte d'infrastructures. Contrainte de financement.

Codes *JEL*: F35, O16, O50

Abstract

This paper explores the impact of foreign aid on firm growth for a panel of 4,342 firms in 29 developing countries, 11 of which are in Africa. Using the World Bank Enterprise Surveys data and controlling for firm fixed effects, we find a positive impact of foreign aid on firms' sales growth. This result is robust to several checks, notably to the instrumentation of aid and to estimations on various sub-samples. We then provide evidence that the positive effect of aid is especially strong for firms operating in sectors that are intensive in infrastructure and external finance, suggesting that aid may improve firm performance through the alleviation of infrastructure and financing constraints in developing countries.

Keywords: Foreign aid. Firm growth. Infrastructure constraint. Financing constraint.

***JEL* classification**: F35, O16, O50

Non-technical summary

Foreign aid effectiveness has been harshly debated over the last decade. While some authors recently found a small positive effect of aid on growth (Clemens et al., 2011; Galiani et al., 2014), others found no impact (Rajan and Subramanian, 2008), or even an adverse Dutch disease effect of aid on growth (Rajan and Subramanian, 2011). These studies all suffer from methodological weaknesses, the main one being the poor treatment of the endogeneity of aid with respect to aggregate growth outcomes.

In this article, we provide a new assessment of aid effectiveness looking at a disaggregated outcome, namely firm growth. The database comprises more than 4,300 firms in 29 developing countries, 11 of which are in Africa. We stacked World Bank Enterprise Surveys (WBES) datasets, and focused on the datasets for which two points in time are available for each firm, so that we can control for a firm fixed effect.

We find that aid has a positive impact on firm performance. This result is robust to specification tests, sample dependence, and attrition. It is also robust to the instrumentation of aid using instruments in the tradition of Tavares (2003). The coefficients indicate that a 10% increase in aid would increase firm growth by between 5 and 7.5%.

There are two ways through which aid may influence firm growth: demand (increased demand financed by aid), or supply (aid affects the productive capacity of firms). We exploit sector structural characteristics to analyse the various channels through which aid may affect firm growth. We find that the positive impact of aid on growth is at play for firms belonging to sectors which tend to depend on external finance and on infrastructure, suggesting that aid improves firm performance through the alleviation of infrastructure and financing constraints in developing countries. This set of results suggests that aid contributes to increasing the productive capacity of firms in developing countries, through relaxing the constraints they face. We cannot rule out that the demand-side mechanism is at play. However, large, state-owned, and foreign-owned firms would be more likely to benefit from an increase in demand due to aid, but we find no difference in the impact of aid according to firm characteristics.

1 Introduction

After a few years of disgrace, the debate over the new Sustainable Development Goals has brought the issue of aid effectiveness back to the fore. In the last decade, the fierce debate on aid effectiveness mainly led to the conclusion that, if any, aid has a heterogeneous influence on growth, which depends on how much aid receiving countries can absorb before displaying negative returns. The absorptive capacity of aid has been shown to depend on the quality of macroeconomic policy (Burnside and Dollar, 2000), exposure to external shocks (Guillaumont and Chauvet, 2001; Collier and Dehn, 2001), structural handicaps (Dalgaard et al., 2004), and the interests of the elite (Angeles and Neanidis, 2009), among others. Other authors have argued that the impact of aid also depends on donors' practices (Djankov et al., 2009) or motivation (Kilby and Dreher, 2010; Dreher et al., 2014), or on the type of aid (Clemens et al., 2011). In an influential article, Rajan and Subramanian (2008) also found that aid has no impact on growth.

These studies all explore the impact of aid on aggregate growth rates at the country level and share similar methodological weaknesses, the main issue being the poor treatment of the endogeneity of aid (Deaton, 2010; Temple, 2010). In this article, we build on the existing literature on aid effectiveness, but rely on more disaggregated data to assess the impact of aid on growth. We examine how aid affects firms' sales growth for a panel of 29 developing countries, using the World Bank Enterprise Surveys (WBES) panel datasets. This approach has one major methodological advantage: it largely attenuates the endogeneity at play in the aid-growth relationship.

The endogeneity of aid in aggregate growth regressions stems both from reverse causality and confounding factors. The resulting endogeneity bias could go in either direction. Reverse causality may induce a downward bias if donors wish to compensate receiving countries for their poor growth performance. Inversely, it may induce an upward bias if donors tend to reward and support receiving countries when they show promising growth performance. Brückner (2013) explores the reverse causality bias in the aid-growth relationship for a panel of 47 least developed countries. He finds a large negative effect of growth - instrumented using the international commodity price index and rainfall variables - on aid. Once this negative effect is accounted for, the impact of aid on growth becomes significantly positive, though rather small.

There are also many examples of cases in which omitted variables may bias the least-squares estimates of the impact of aid on growth downward. Back in the seventies, Papanek (1972) presented the many factors that could simultaneously induce greater aid flows and lower growth rates, such as wars and political instability, terms of trade deterioration, climatic shocks, and natural disasters. This of course assumes that aid is mostly counter-cyclical, which has been debated in the literature (Bulir and Hamann, 2001; Pallage and

Robe, 2001). When aid is pro-cyclical, omitted shocks may as well induce an upward bias in the least-squares estimates of the aid-growth relationship.

The identification strategies dealing with the endogeneity of aid at the macroeconomic level have evolved and improved over time. The first attempts to instrument aid relied on recipients' characteristics, which are highly correlated with aid allocation, mostly the size of the receiving countries. Both Deaton (2010) and Bazzi and Clemens (2013) provide a detailed discussion of the reasons why this instrument violates the exclusion restriction. The first improvement to this identification strategy came from Tavares (2003) who resorted to supply-side instruments for aid in a corruption equation. The idea is to exploit the exogenous variation in the geographical allocation of aid that stems from donor countries' economic situations. Tavares (2003) uses the weighted average of bilateral donors' total aid budget, where the weights are alternatively various cultural and geographic distances. This approach was then refined by Rajan and Subramanian (2008). They run a gravity model that explains bilateral aid flows using structural variables (relative size and colonial past) and use the predicted value of bilateral aid flows, re-aggregated at the recipient level, as an instrument for aid. However, as discussed in Bazzi and Clemens (2013), their instrument is highly correlated with the recipients' size, which largely weakens its validity.

A new strand is currently emerging in this aid effectiveness literature which exploits quasi-experiments, that is specific situations which allow to identify the impact of aid on growth. The first attempts have used shocks affecting donor countries. Werker et al. (2009) exploit the exogenous variation in oil prices as an instrument for aid from Arab countries. Similarly, Nunn and Qian (2014) use fluctuations in US wheat production, weighted by a proxy for a country's tendency to receive food aid, as an instrument for US food aid in a conflict estimation. Shocks affecting receiving countries have also recently been exploited as an exogenous source of variation in aid. Galiani et al. (2014) use graduation from International Development Association (IDA), and the large cut in aid in the following years, as a source of exogenous variation in aid. They find that once aid is instrumented by a dummy variable indicating whether the country has crossed the IDA threshold, aid has a positive impact on growth.

In this article, we explore the impact of aid on growth, but using firm-level performance as the outcome. Looking at the impact of aid on disaggregated growth outcomes is an alternative approach to dealing with the endogeneity of aid. First, looking at the impact of aid on disaggregated growth outcomes largely attenuates reverse causality: it is highly unlikely that a firm's performance would influence the allocation of aid by donors. Our econometric framework also largely reduces the omitted variable bias. We focus on the WBES for which panel data are available. This allows us to include firm fixed effects, hence controlling for time-

invariant heterogeneity. To account for time-varying heterogeneity, we include a large set of firm-level and country-level variables, as well as industry x year dummies. The last source of omitted bias could stem from time-varying unobservable heterogeneity, which may be correlated with both firm performance and the amount of aid received. We therefore instrument aid using changes in donors' situations as a source of exogenous variation in aid, in the tradition of Tavares (2003). More specifically, we find an exogenous source of variation in aid in the change of total fiscal revenue of donor countries, weighted by the historical distance (colonial ties) between pairs of donor-recipient countries. We find that once instrumented, the impact of aid is not significantly different than when using the fixed-effect estimator. Our results also suggest that the endogeneity of aid does not plague our analysis and that aid can be assumed to be exogenous to firm growth in our framework.

Our results suggest a significantly positive effect of aid on firm growth. A one percentage point increase in aid increases firm growth by around two percentage points, which represents a 20-percent increase in average growth - average growth is around 8.5 percent in our sample of firms. Despite the apparently very strong effect of aid in our analysis, we provide evidence that the magnitude of the effect of aid is very similar to what was found at the aggregate level by Clemens et al. (2011), who conclude that a one percentage point increase in aid would translate into a 0.1 to 0.3 percentage point increase in growth, which given the countries' average growth rates (around 1.3 percent) would also represent a 20 percent increase in growth.

The second main methodological advantage of our approach is that it allows exploring the channels through which aid influences firm growth. In this respect, our study closely follows Rajan and Subramanian (2007, 2011). They examine the impact of aid on the growth rate of manufacturing industries' value added. They explore two channels through which aid may adversely impact industry growth: Dutch disease and the deterioration of institutions. Following Rajan and Zingales (1998), they show that aid adversely impacts industries that rely relatively more on institutions and that are more outward looking. We implement the same strategy and explore how firms are impacted by aid given the structural characteristics of the industries they belong to. We explore four channels: the Dutch disease and institution channels, the financing channel, and the infrastructure channel (electricity and transport). We find evidence that the positive impact of aid on firm growth mainly goes through relaxing the financing and infrastructure constraints.

Despite all the methodological advantages, looking at the effect of aid on firm growth also raises new methodological challenges that we discuss in the article. The main challenges are sample dependence and attrition. We provide a set of robustness checks suggesting that our results are not driven either by specific country and firm characteristics, or by a selection bias.

The article is structured as follows. After having presented the various mechanisms through which aid may influence firm growth in Section 2, we describe the model and data in Section 3. The baseline results are presented in Section 4. Section 5 provides a large set of robustness checks, while the channels of the effect of aid on firm growth are discussed in Section 6. Finally, Section 7 concludes.

2 Aid and Firm Growth

Aid is not directly provided to firms. In this study, the aid we focus on is Official Development Assistance, which is provided by governments and multilateral institutions to developing countries' governments mostly.[1] There are two ways through which aid may influence firm performance: demand (increased demand financed by aid is met by firms' production), or supply (aid affects the productive capacity of firms).

The demand mechanism refers to the fact that aid, and notably aid to large infrastructure projects, may partly finance firms' activities if firms benefit from public contracts directly, or indirectly if they are suppliers to large firms which have won public procurements on infrastructure. This is a typical Keynesian mechanism, in which firms' activities benefit from the public financing of large infrastructure construction. This mechanism should rather benefit large firms, whether they are state-owned and politically connected in the receiving country, or foreign-owned and politically connected in the donor country since donors' procurement systems are often designed to provide their own domestic firms, or affiliates, with the aid contracts.

The supply-side mechanism refers to the way aid changes the productive capacity of firms. Aid may notably influence the constraints which impede firm growth in developing countries. The literature on firm performance emphasizes three main kinds of constraints to firm growth in developing countries. The financing constraint is by far the most documented. Harrison et al. (2004) and Beck et al. (2005) show that individual financing obstacles such as credit access, collateral requirements, or bank bureaucracies do constrain firm growth. Weak access to finance also reduces the probability that firms enter the export market (Berman and Héricourt, 2010) and prevents them from importing the required capital goods (Bas and Berthou, 2012). Aid may relax the financing constraint faced by firms. In a credit constrained environment, aid may provide the government with financing, hence making more credit available for private firms. This point was first made by Rajan and Subramanian (2011), who however found no evidence of this mechanism in their data.

A second constraint highlighted in the literature on firm performance in developing countries is infras-

[1] There is a small share of aid which is directly provided to government agencies or state-owned firms. Whether state-owned firms drive our results is explored in Section 6.

tructure. The critical role of the provision of infrastructure for economic development has been evidenced in its various dimensions: transport, energy, telecommunications, and water (see among others Calderon and Serven (2008), Rud (2012), and Straub (2008)). Infrastructure has been shown to be quantitatively important in determining transport costs (Limao and Venables, 2001), and in ensuring access to inputs and to markets. At a more disaggregated level, several studies have also found that a lack of infrastructure significantly undermines firm growth. Using firm-level data on Bangladesh, China, India, and Pakistan, Dollar et al. (2005) find that the factor returns, accumulation, and growth of firms are higher when there are fewer bottlenecks, such as the number of days to clear goods through customs, to get a telephone line, or the sales lost to power outages. Harrison et al. (2014) underline that the lack of well-functioning infrastructures, proxied by telecommunications, is one of the key explanations to Africa's disadvantage in firm performance (along with the financing constraint), compared to other regions. In these countries, indirect costs, related to infrastructure and services, represent a large burden on the competitiveness of firms (Eifert et al., 2008). In India, Mitra et al. (2002) and Datta (2012) also evidenced that infrastructure endowment substantially fosters the performance of the industrial sector. As for the financing constraint, aid may contribute to relaxing the infrastructure constraint weighing on firm performance. Aid invested in factors which are complementary to firms' activities may crowd in private investment. This is particularly true for the basic transport and energy infrastructure. For example, Selaya and Sunesen (2012) find evidence that aid to these sectors has a strong positive impact on foreign direct investment flowing in.

Last, a country's global macroeconomic and institutional environment also significantly affects the way firms can profitably develop their activities. In particular, Fisman and Svensson (2007) and Chong and Gradstein (2009) respectively show that corruption and the volatility of economic policies tend to reduce firm growth. Again, aid may affect both the institutional and macroeconomic environment. Aid may induce an institution curse and weaken economic institutions (Knack, 2001; Bräutigam and Knack, 2004; Busse and Gröning, 2009). Using industry-level data, Rajan and Subramanian (2007) provide evidence that aid reduces the growth rate of industries which structurally rely more on institutions. From a macroeconomic point of view, aid may also dampen manufacturing firm growth if it induces Dutch disease, that is an appreciation of the real exchange rate detrimental to outward-looking manufacturing firms. Rajan and Subramanian (2011) have indeed recently provided evidence that aid has an adverse impact on industries which tend to be more outward looking.

To summarize, it seems that aid may benefit firms by increasing their demand (notably through access to infrastructure contracts), or by relaxing some constraints: the infrastructure and financing constraints. From

a negative point of view, aid may impede firm growth if it induces an institution curse or Dutch disease. The mechanisms sketched out above are not exhaustive, but we argue that they are the most important when looking at the short-run impact of aid. In the longer run, aid may also influence firm performance through human capital (increasing education and enhancing health in the developing world for example), but given the time span of our data, these mechanisms are not examined in this study.

3 Model and Data

We investigate the impact of foreign aid on firm growth using the general following specification:

$$GROWTH_{i,k,j,(t,t-3)} = \alpha + \beta X_{i,k,j,t} + \gamma Y_{j,(t,t-3)} + \mu_i + \tau_{k,t} + \varepsilon_{i,k,j,t} \quad (1)$$

where $\text{GROWTH}_{i,k,j,(t,t-3)}$ is the average annual growth rate of the sales of firm i in industry k, and country j. The average annual growth rate is computed over three years, between year t and *t-3*. $\text{X}_{i,k,j,t}$ is a set of time-varying firm-level characteristics, while $\text{Y}_{j,(t,t-3)}$ is a set of country-level variables including foreign aid, measured on average on the three years for which firm growth is measured. We include firm fixed effects, μ_i, as well as industry x year dummies, $\tau_{k,t}$.

3.1 Firm-Level Panel Data

The dataset at the firm level combines all the World Bank Enterprise Surveys (WBES) with two periods of observation for each firm available in September 2013. These surveys cover a representative sample of an economy's manufacturing and service sectors. In each country, data were gathered through an extensive questionnaire answered during a face-to-face interview by business owners and top managers. They represent a comprehensive and comparable source of firm-level data since the survey questions are the same across all countries and years. The sample of countries and years is presented in Appendix 1. It is composed of 16 Latin American countries (which account for 64.7% of the sample of firms observed twice), 11 African countries (22.6% of the firms observed twice), and two South Asian countries (Bangladesh and Pakistan, 12.7% of the firms observed twice).

Data in local currencies have been deflated using the same base year (100 = 2005), and converted into US dollars. GDP deflators and exchange rates are obtained from the IMF International Financial Statistics (IFS). After harmonization across countries, the panel dataset comprises more than 4,300 firms observed twice in time from 29 developing countries (details in Appendix 1). We did not consider surveys for Angola

(2006 and 2010), the Democratic Republic of Congo (2006 and 2010), and Afghanistan (2005 and 2009) since these three countries experienced violent events and benefited from higher than normal growth rates and/or aid amounts, driving our results on the effect of aid on growth artificially upwards.[2]

Each WBES includes information on sales in the year preceding the survey, as well as three years before. This allows us to compute the growth rate of sales over three years for each available survey, $\text{GROWTH}_{i,k,j,(t,t-3)}$. For some countries, the time span is slightly different, depending on the years for which the questions were asked.[3] Equation 1 is run controlling for the lagged value of sales, $\text{SALES}_{i,k,j,t-3}$, in logarithm, which is most of the time measured in t-3, with some exceptions.[4] We also control for the characteristics of firm ownership using two variables, $\text{STATE}_{i,k,j,t}$ and $\text{FOREIGN}_{i,k,j,t}$. $\text{STATE}_{i,k,j,t}$ is a dummy variable which is equal to one when part of (or all) the firm is owned by the state. $\text{FOREIGN}_{i,k,j,t}$ is a dummy variable which is equal to one when part of (or all) the firm is owned by a foreign individual or company. Firm-level controls also include information on whether the firm is outward looking using $\text{EXPORTS}_{i,k,j,t}$, which is a dummy variable equal to one when the firm exports part of or all its sales, either directly or indirectly (as a supplier to exporting firms). Finally, we control for the size of the firm, $\text{SIZE}_{i,k,j,t}$ which takes the value one for firms with fewer than 20 employees, the value two for firms with between 20 and 100 employees, and three for firms with more than 100 employees.

Table 1 presents basic summary statistics for our sample of firms for which we have panel data. To avoid extremely fast-growing firms driving the results, we excluded the top one percent of the growth distribution from the sample. Table 1 illustrates clearly that we are working on a panel of rather large formal firms: 36% are outward looking (exporting either directly or indirectly), 12% are owned or partly owned by a foreign entity, and the average size is 1.9. Around one-third of the sample is composed of firms with fewer than 20 employees, another third are firms with between 20 and 100 employees, and a third are firms with more than 100 employees.

3.2 Country-Level Variables

At the country level, we follow Beck et al. (2005) and Harrison et al. (2014), and control for the level of development using the logarithm of income per capita, $\text{INCOME}_{j,(t-3,t-6)}$.[5] We also control for the

[2]Collier and Hoeffler (2004) illustrate the higher than normal effectiveness of aid in post-conflict societies.

[3]The growth rate of sales covers four years for Botswana and Mali in period 1, and for Brazil, Pakistan, Senegal, South Africa, and Zambia in period 2. It is calculated over two years for Niger in period 1.

[4]It is measured in t-4 for Botswana and Mali in period 1, and for Brazil, Pakistan, Senegal, South Africa, and Zambia in period 2. It is measured in t-2 for Niger in period 1.

[5]We use GDP per capita in current local currency - World Development Indicators - and deflate it using the same deflator as for the firm-level variables $\text{GROWTH}_{i,k,j,(t,t-3)}$ and $\text{SALES}_{i,k,j,t-3}$ (base year = 2005).

macroeconomic dynamism of the economy using the country's growth rate, GDP GROWTH$_{j,(t-3,t-6)}$. Both variables are averaged over a three-year period, but lagged one period to avoid endogeneity concerns (between t-3 and t-6). We also control for the size of the country using the logarithm of the population, POPULATION$_{j,(t,t-3)}$ averaged over t and t-3. We use the World Development Indicators for INCOME$_{j,(t-3,t-6)}$, GDP GROWTH$_{j,(t-3,t-6)}$, and POPULATION$_{j,(t,t-3)}$. We also control for the quality of economic institutions using an indicator of control of corruption, CORRUPTION$_{j,(t,t-3)}$ (from the Worldwide Governance Indicators, Kaufmann et al. (2011)). It is averaged over t and t-3 and ranges from weak (-2.5) to strong (+2.5) control of corruption.

Table 1: Summary statistics.

Variables		**N**	**mean**	**median**	**sd**	**min**	**max**
			Firm characteristics				
GROWTH$_{i,k,j,(t,t-3)}$		8,684	8.26	3.33	35.26	-99.65	445.36
SALES$_{i,k,j,t-3}$	logarithm	8,684	13.74	13.52	2.69	5.23	28.81
STATE$_{i,k,j,t}$	dummy	8,684	0.01	0	0.08	0	1
FOREIGN$_{i,k,j,t}$	dummy	8,684	0.12	0	0.32	0	1
EXPORTS$_{i,k,j,t}$	dummy	8,684	0.36	0	0.48	0	1
SIZE$_{i,k,j,t}$		8,684	1.92	2	0.78	1	3
			Country variables[a]				
GDP GROWTH$_{j,(t-3,t-6)}$		58	-1.28	1.80	8.41	-34.74	10.08
INCOME$_{j,(t-3,t-6)}$	logarithm	58	7.40	7.46	1.15	5.27	9.51
POPULATION$_{j,(t,t-3)}$	logarithm	58	16.53	16.43	1.32	13.05	19.05
CORRUPTION$_{j,(t,t-3)}$		58	-0.32	-0.44	0.66	-1.44	1.38
AID$_{j,(t,t-3)}$	%GDP	58	5.17	1.24	6.60	-0.10	21.73
INSTRUMENT$_{j,(t,t-3)}$	%GDP	58	35.45	34.85	12.44	0	74.06

[a] Number of observations at the country level. Firm-level variables are from the World Bank Enterprise Surveys (various years). Data at the country level are from the World Development Indicators, except for CORRUPTION (Worldwide Governance Indicators) and AID (OECD-DAC).

Finally, we include Official Development Assistance, AID$_{j,(t,t-3)}$, in our estimations. Aid data are from the OECD-DAC. Aid is measured as net disbursement flows of ODA in percent of GDP. It is averaged over the three years for which firm growth is measured, between t and *t-3*. As shown in Table 1, AID$_{j,(t,t-3)}$ is on average 5.2% of GDP, but the amounts vary significantly from one country to the other. Some upper-middle-income Latin American countries such as, Argentina, Brazil, and Mexico receive less than 1% of their GDP in aid. Other African countries, such as Niger, Malawi, and Zambia receive more than 15% of their GDP in aid. The summary statistics of the country averages presented in Table 1 are unweighted. Each

country is given the same weight, independently of the number of firms in each country. Given the fact that the firm surveys in Latin American countries include on average twice as many firms as the African surveys, while receiving on average five times less aid, the mean aid on the sample of firms for which we have panel data is at a much lower level than 5.2%, around 3.1%.

3.3 Identification Strategy for the Impact of Aid

Equation 1 is estimated using the fixed-effect estimator. This allows us to control for firm-level time-invariant heterogeneity. To this fixed-effect setting, we add industry x time dummies in order to also control for industry time-varying heterogeneity. Finally, following Moulton (1990), the standard errors are clustered at the country level, the level of aggregation of the variable of interest, $\text{AID}_{j,(t,t-3)}$.

Reverse causality concerns are largely attenuated by the fact that foreign aid is measured at the country level while the outcome, sales growth, is measured at the firm level. Moreover, our framework allows us to account for part of the observable heterogeneity - using a large set of control variables both at the firm and country level - and for the unobservable heterogeneity - using firm fixed effects and industry x year dummies.

However, the estimated correlation between foreign aid and firm growth could still be biased mainly through one remaining endogeneity channel: the existence of time-varying unobservable heterogeneity. To account for this issue, we rely on an instrumentation procedure based on Tavares (2003). More specifically, we find a source of exogenous variation of aid in changes in donors' economic situation, weighted by historic proximity between donors and receiving countries. A more aid-prone donor environment is captured using the total amount of fiscal revenue (as a share of donors' GDP), $\text{FISCAL}_{d,(t,t-3)}$, where d indicates the donor country. Our instrument is then the weighted average of $\text{FISCAL}_{d,(t,t-3)}$ over the 22 DAC donors. We use a dummy for whether the receiving country is a former colony of the donor country, $\text{COLONY}_{j,d}$ - historical distance between the pair of donor d and receiving country j - to calculate the weighted sum of $\text{FISCAL}_{d,(t,t-3)}$ over the 22 donors. Our instrument for aid is thus:

$$INSTRUMENT_{j,(t,t-3)} = \Sigma_{d=1}^{22} FISCAL_{d,(t,t-3)} \times COLONY_{j,d} \tag{2}$$

The fact that we use colonial links as a weight to construct the instrument has some implications regarding the interpretation of the effect of aid once instrumented. Indeed, the instrument is likely to be a good predictor of aid to Africa (11 countries in the sample), but quite poor for Latin American countries for

which aid from the major donors (US, Japan, Germany, UK, and France) will be weighted by zero, and aid from Spain and Portugal, more marginal donors, will be weighted by one. To the extent that most of the identification relies on the sub-sample of Africa, the results of the IV estimation should be interpreted in terms of local average treatment effect (LATE), that is as an estimate of the effect of aid on the sub-sample of Africa mostly. For the exclusion restriction to be valid, we need to assume that fiscal revenues in donor countries are not correlated to the error term in Equation 1, notably when the countries have close historical links. If the fiscal cycles in donor countries are correlated with an omitted variable - trade for example - which might be itself directly correlated with firm performance, then the exclusion restriction may be violated. In the robustness checks, we will therefore provide specification tests which account for factors that may induce the violation of this hypothesis.

4 The Impact of Aid on Firm Growth

4.1 Baseline Results

Before turning to our core fixed-effect results, we look at the results when the OLS estimator is used. In this case, Equation 1 is estimated without the firm fixed effects (μ_i), but including country dummies and industry x year dummies. Since the equation includes country fixed effects, the standard errors are clustered at the firm level. The results are presented in Table 2.

Using the OLS estimator, we do not need to restrict ourselves to the 4,342 firms for which we have panel data. Column (1) shows the results when all 21,331 firms are used. Then column (2) shows the same estimation on the sample of firms for which we have two points in time. Finally, column (3) shows the results when aid is instrumented. In all three estimations, the coefficient of $\text{SALES}_{i,k,j,t-3}$ suggests a catching-up effect: firms with lower sales levels in t-3 tend to have higher growth rates in t than firms that already had high sales. $\text{STATE}_{i,k,j,t}$ is never significant, suggesting that being owned or partly owned by the state does not significantly affect the performance. $\text{FOREIGN}_{i,k,j,t}$ and $\text{EXPORTS}_{i,k,j,t}$ both have positive and significant coefficients suggesting that outward-looking firms and firms which are foreign-owned tend to have higher growth rates. The coefficient associated with $\text{SIZE}_{i,k,j,t}$ is positive and significant suggesting that larger firms also tend to have a higher growth rate. Turning to the country-level variables, Table 2 shows that the level of development ($\text{INCOME}_{j,(t-3,t-6)}$) and economic dynamism ($\text{GDP GROWTH}_{j,(t-3,t-6)}$) are both positively correlated with firm growth: firms face higher demand and a better business environment in higher income, growing countries. The size of the population also displays a positive correlation with firm

growth, which reflects the fact that the market is larger in bigger countries. Finally, countries with a better control of corruption tend to have more performing firms.

Turning to the correlation between foreign aid and firm growth, regressions (1) and (2) show a positive and significant coefficient for $\text{AID}_{j,(t,t-3)}$, suggesting that a one percentage point increase in the share of aid in GDP may induce an increase in firms' sales growth of 1.4-1.8 percentage points. Once aid is instrumented, in regression (3), its coefficient is slightly higher (2.2), but not significantly so given the size of the standard errors and the overlap in the confidence intervals. The instrument used for aid in regression (3) seems to perform fairly well. It has a significant coefficient in the first-step regression, with the expected sign. The first-step Kleibergen-Paap LM and F tests are satisfactory.

Columns (4) and (5) display the results when firm fixed effects are accounted for. Country dummies are now dropped and the standard errors are clustered at the country level. When firm fixed effects are introduced, some of the firm-level variables lose their significance. This is the case of $\text{STATE}_{i,k,j,t}$ and $\text{FOREIGN}_{i,k,j,t}$ which do not sufficiently vary over time.[6, 7] The results of regression (4) are otherwise very similar to those obtained with OLS. The coefficient for $\text{AID}_{j,(t,t-3)}$ is slightly higher than in the OLS estimation, and a bit smaller than the IV coefficient for aid in regression (3). Again, given the overlap in the confidence intervals, these differences are not statistically significant. The coefficient of $\text{AID}_{j,(t,t-3)}$ implies that firms in countries where aid has been increased by one percentage point would experience a growth increase of almost two percentage points. While this may seem considerable, we compare below our results with those of the aid literature at the country level. In column (5), the IV estimation when fixed effects are accounted for is also very similar to the previous result. The first-step tests all provide satisfactory results. The coefficient for aid is a bit higher when it is instrumented in column (5) than it is in column (4), but again the difference in the coefficients is not statistically significant.

Overall, columns (1), (2), and (4) of Table 2 suggest a significantly positive effect of aid on firm growth: for a one percentage point increase in aid, firm growth would increase by around 1.4-2 percentage points. The impact is a bit greater in regressions (3) and (5). Since the IV estimator may be interpreted as a local average treatment effect (LATE), it may suggest that the impact of aid is potentially higher in countries which have former colonial ties with the biggest donors. In our sample, these countries are mostly African countries, since the former colonial ties for Latin American countries links them to marginal donor countries (Spain and Portugal). Overall, one interpretation of the highest coefficient for $\text{AID}_{j,(t,t-3)}$ in column (5) of

[6] Only 49 firms have a switch in $\text{STATE}_{i,k,j,t}$ (1.13% of the firms for which we have panel data) from period one to period two; and 678 firms have a switch in $\text{FOREIGN}_{i,k,j,t}$ (15.6% of the observations).

[7] All the tables have also been estimated without $\text{STATE}_{i,k,j,t}$ and $\text{FOREIGN}_{i,k,j,t}$. Dropping these two variables does not affect the results.

Table 2 would be that aid has a bigger effect on low income African countries than on the Latin American countries in the sample. More discussion on the differential impact of aid in Latin America and in Africa is provided in the robustness checks.

4.2 Magnitude of the Effect

While the magnitude of the effect of aid in Table 2 seems large compared to what is usually found in the literature, it needs to be related to the average values of aid and growth in our sample. Panel A of Table 3 compares the percentage increase in growth that would stem from a one percentage point increase in aid found in different articles in the literature. We compare the effect of aid found in columns (1), (2), and (4) of Table 2 with those presented by Clemens et al. (2011) who reproduce Burnside and Dollar (2000)'s and Rajan and Subramanian (2008)'s results using extended datasets.[8] They find that overall a one percentage point increase in aid may increase GDP growth rate by around 0.1-0.3 percentage points in the following years.[9] Because these studies look at country-level income per capita growth rate, the average value of growth in their sample is much lower (around 1.34-1.62 annual growth rate) than the average value of firms' sales growth. This implies that an economy growing at 1.34% per year would see its growth rate increased by 19.8% if aid was increased by one percentage point, and assuming that a one percentage point increase in aid increases growth by 0.265 percentage points (column (5), Panel A of Table 3).

Panel A of Table 3 suggests that controlling for fixed effects (columns (3), (5), and (7)) leads to estimates of the impact of a one percentage point increase in aid in the range of 11% to 23% of increase in average growth. The difference between our fixed-effect estimate and Clemens et al. (2011)'s estimation of Burnside and Dollar's model is quite small. The difference is greater when comparing our estimates with the coefficients obtained by Clemens et al. (2011) for Rajan and Subramanian's model. These latter coefficients are however systematically lower than what has recently been found in the literature.[10]

[8] They also lag the aid variable.

[9] The calculations are presented in footnote 27 page 609 of Clemens et al. (2011). The coefficients for aid and aid squared corresponding to the effect of aid in column (4) of Table 3 are respectively 0.165 and -0.004. Mean aid is 5.52 in their sample. A one percentage point increase in aid therefore increases growth by [0.165 x 6.52 - (-0.004) x 6.52^2]-[0.165 x 5.52 - (-0.004) x 5.52^2] = 0.117. In column (5) of Table 3, the coefficients for aid and aid squared on which the calculations are based are respectively 0.361 and -0.008. In column (6), the coefficients for aid and aid squared on which the calculations are based are 0.106 and -0.003; they are 0.247 and -0.005 in column (7). The coefficients for aid and aid squared estimated by Clemens et al. (2011) when reproducing Rajan and Subramanian (2008)'s results are not significant. However the corresponding turning points are positive and significantly different from zero.

[10] See Galiani et al. (2014) for example.

Table 2: Baseline estimations of the impact of aid on firm growth.

Dependent: $GROWTH_{i,k,j,(t,t-3)}$	(1) OLS	(2) OLS	(3) IV	(4) FE	(5) IV-FE
$SALES_{i,k,j,t-3}$	-7.128***	-7.776***	-7.777***	-11.797***	-11.806***
	(0.222)	(0.406)	(0.403)	(1.791)	(1.747)
$STATE_{i,k,j,t}$	3.871	7.288	7.293	17.076	17.163
	(3.408)	(6.930)	(6.885)	(11.136)	(10.963)
$FOREIGN_{i,k,j,t}$	5.430***	5.415***	5.408***	0.995	0.846
	(0.658)	(1.112)	(1.102)	(1.843)	(1.770)
$EXPORTS_{i,k,j,t}$	4.775***	4.427***	4.433***	5.284**	5.339***
	(0.489)	(0.840)	(0.834)	(2.139)	(2.067)
$SIZE_{i,k,j,t}$	13.407***	13.782***	13.784***	6.988***	6.987***
	(0.506)	(0.891)	(0.885)	(1.473)	(1.441)
$INCOME_{j,(t-3,t-6)}$	40.389***	43.946***	43.946***	56.765***	56.832***
	(2.324)	(3.780)	(3.762)	(14.887)	(14.604)
GDP $GROWTH_{j,(t-3,t-6)}$	0.468***	0.674***	0.660***	1.003*	0.970**
	(0.069)	(0.132)	(0.129)	(0.508)	(0.476)
$POPULATION_{j,(t,t-3)}$	237.43***	281.14***	273.54***	289.03***	272.68***
	(26.021)	(41.501)	(42.931)	(97.345)	(81.841)
$CORRUPTION_{j,(t,t-3)}$	62.63***	62.03***	63.02***	66.89***	70.12***
	(4.115)	(6.423)	(6.484)	(12.935)	(14.161)
$AID_{j,(t,t-3)}$	1.388***	1.815***	2.175**	1.980**	2.986*
	(0.448)	(0.638)	(0.991)	(0.721)	(1.730)
First-step results					
$INSTRUMENT_{j,(t,t-3)}$			1.975***		1.965***
			(0.049)		(0.432)
Observations	25,673	8,684	8,684	8,684	8,684
Number of firms	21,331	4,342	4,342	4,342	4,342
R-squared	0.173	0.189	0.189	0.257	0.256
Firm fixed effects	no	no	no	yes	yes
Industry x Year dummies	yes	yes	yes	yes	yes
Level of se clustering	firm	firm	firm	country	country
Country dummies	yes	yes	yes	no	no
Kleibergen-Paap LM stat. (p-value)			0.000		0.079
Kleibergen-Paap F-stat.			1593		20.6
Durbin-Wu-Hausman (p-value)			0.696		0.581

Columns (1) and (2) are estimated using the OLS estimator, with country and industry x year dummies and robust standard errors clustered at the firm level. Column (3) is estimated using the IV estimator, with country and industry x year dummies and robust standard errors clustered at the firm level. Column (4) is estimated using the within estimator, with firm fixed effects, industry x year dummies and robust clustered standard errors at the country level. Column (5) is estimated using the IV estimator with firm fixed effects, industry x year dummies and robust clustered standard errors at the country level. The underidentification stems from the Kleibergen-Paap rk LM statistic. The weak identification test stems from the Kleibergen-Paap rk Wald F-statistic. The Stock-Yogo weak identification test critical value at 10% is 16.4. The Durbin-Wu-Hausman test checks whether the OLS and FE estimators are consistent (aid can be treated as exogenous). ***p<0.01, **p<0.05, *p<0.1.

If we want to fully be able to compare the impact of aid across studies, not only do we need to account for different mean values of growth rate, but also to account for different mean values of aid. In our sample of firms, aid is on average two percentage points lower than in Burnside and Dollar (2000) and Rajan and Subramanian (2008). A 10% increase in aid would therefore lead to a 6 to 11 % increase in growth in these studies (when accounting for country fixed effects, columns (5) and (7) of Panel B in Table 3).[11] In our sample, a 10% increase in aid would lead to a 7.5% increase in average growth, which is in a similar range.

Table 3 also allows to compare the impact of aid obtained from OLS and fixed-effect estimations: it seems that the OLS estimator tends to under-estimate the impact of aid, the difference between OLS and fixed-effect estimations being larger on aggregate data (columns (4) and (5), and columns (6) and (7)) than on firm data (columns (2) and (3), on the same sample of firms). This suggests that estimations on aggregate outcome are more prone to the omitted variable bias when estimating the aid-growth relationship. It also suggests that the omitted variable bias tends to bias the coefficient of aid in OLS downward, as already discussed by Papanek (1972).

Table 3: Magnitude of the effect and comparison with other studies.

	Firm's sample			Clemens et al. (2011)			
		Table 2		Burnside and Dollar Table 7		Rajan and Subramanian Table 9	
	Col(1) OLS (1)	Col(2) OLS (2)	Col(4) FE (3)	Col(6) OLS (4)	Col(7) FD (5)	Col(6) OLS (6)	Col(7) FD (7)
Average aid	2.83	3.13	3.13	5.52	5.52	5.12	5.12
Average growth rate	8.13	8.26	8.26	1.34	1.34	1.62	1.62
Panel A							
Impact of one p.p. increase in aid	1.388	1.815	1.980	0.117	0.265	0.073	0.187
Increase in average growth (%)	17.07	21.97	23.96	8.72	19.75	4.46	11.53
Panel B							
Impact of a 10% increase in aid	0.394	0.572	0.618	0.066	0.148	0.038	0.099
Increase in average growth (%)	4.84	6.88	7.50	4.89	11.05	2.33	6.11

In columns (4) to (7), aid is lagged (Clemens et al., 2011).
In column (3), FE refers to the within estimator; in columns (5) and (7) FD refers to the estimation after first-differencing the equation.

[11] In column (5), for example, a 10% increase in average aid would lead to a [0.165 x (5.52 + 0.552) - (-0.004) x (5.52 + 0.552)^2]-[0.165 x 5.52 - (-0.004) x 5.52^2] = 0.066 percentage point increase in growth, which is equal to a 4.89% increase in average growth (which is 1.34 at the average).

5 Robustness Checks

In what follows, we present various robustness checks for the baseline results. First, we explore alternative specifications and discuss the validity of the exclusion restriction. Then, we examine the sample-dependence of our result. Finally, we check whether the attrition of firms which are not observed twice biases our results.

5.1 Specification Tests

We first provide specification tests on the firm-level dimension for our preferred model: column (4) of Table 2. We focus on firm-level variables that may relate to the aid-growth relationship, namely the financing, institution, and infrastructure constraints. The WBES questionnaire includes questions on the constraints perceived by managers as significantly impeding their activity. These obstacles are rated on a 0 (no obstacle) to 4 (very severe obstacle) scale. We transform these variables into dummies which take the value one whenever the obstacle is perceived as moderate, major, or very severe.[12] In Table 4, the first three columns display the results when we introduce $\text{FINPB}_{i,k,j,t}$ (whether credit access is considered as an obstacle), $\text{CORRPB}_{i,k,j,t}$ (whether corruption is considered as an obstacle), and $\text{INFRAPB}_{i,k,j,t}$ (whether transport or electricity are considered as an obstacle). The coefficient of aid is not altered by the introduction of these three variables, and none of them significantly affects firm growth. These variables are poor measures of the real constraints faced by firms, since they only reflect the perception of the constraints by the managers of the firms. They are thus likely to be endogenous to firm performance, and the absence of effect should be considered with caution.

In the remainder of Table 4, we address the issue of the potential endogeneity of the firm-level control variables. One obvious endogeneity issue stems from the presence of the logarithm of lagged sales, $\text{sales}_{i,k,j,t-3}$, on the right-hand side of equation 1. As shown by Nickell (1981), the within estimator for dynamic models leads to inconsistent estimates. There is no easy solution to solve the Nickell bias in our framework since we cannot use lagged variables as instruments (we only have two points in time). Columns (1) and (2) of Table 2 show the results when lagged sales are introduced without firm fixed effects. Column (4) of Table 2 displays the results when both firm fixed effects and lagged sales are accounted for. In column (4) of Table 4, we therefore also show the results when only firm fixed effects are accounted for, that is dropping lagged sales from the model. The estimated impact of aid on growth is unaltered by this change in specification which, without solving the issue, is reassuring regarding the robustness of the results.

[12] Focusing on major and very severe does not change the results.

Table 4: Specification tests, fixed-effect estimations, firm-level variables.

Dependent: $GROWTH_{i,k,j,(t,t-3)}$	FINPB (1)	CORRPB (2)	INFRAPB (3)	W/O SALES (4)	CELL (5)
$AID_{j,(t,t-3)}$	1.862***	1.981***	1.953**	1.739**	1.636**
	(0.497)	(0.709)	(0.712)	(0.633)	(0.656)
$VARIABLE_{i,k,j,t}$	-1.778	0.866	1.954		
	(1.306)	(1.204)	(1.249)		
$MEAN\_SALES_{cell,j,t-3}$					1.459
					(0.867)
$SH\_STATE_{cell,j,t}$					2.438
					(17.704)
$SH\_FOREIGN_{cell,j,t}$					7.684
					(6.113)
$SH\_EXPORT_{cell,j,t}$					-4.922
					(5.490)
$MEAN\_SIZE_{cell,j,t}$					-2.269
					(2.327)
Observations	7,234	8,390	8682	8,684	8,684
Firm fixed effects	yes	yes	yes	yes	yes
Industry x Year dummies	yes	yes	yes	yes	yes
Level of se clustering	country	country	country	country	country
Firm-level controls	yes	yes	yes	yes	no
Country-level controls	yes	yes	yes	yes	yes

In column 4, lagged sales, $SALES_{i,k,j,t-3}$, are not included. Estimations using the within estimator, with firm fixed effects, industry x year dummies and robust clustered standard errors at the country level. All estimations include country-level control variables. In columns (1) to (3), firm-level controls are included. In column (4), firm-level controls are replaced with cell-level controls. $FINPB_{i,k,j,t}$ is a dummy equal to one if the firm declares access to credit to be a major obstacle to its activity. $CORRPB_{i,k,j,t}$ is equal to one if corruption is a major obstacle to its activity. $INFRAPB_{i,k,j,t}$ is equal to one if electricity or transport is a major obstacle to its activity. CELL indicates that the variable has been averaged on industry-region-size cells, and on industry-region cells if the cell is empty or if it includes fewer than 5 firms. $MEAN\_SALES_{cell,j,t-3}$ is the level of sales in t-3 for firms that belong to the same industry-region-size cell. $MEAN\_SIZE_{cell,j,t}$ is the average size of the firms belonging to the same cell. $SH\_STATE_{cell,j,t}$, $SH\_FOREIGN_{cell,j,t}$, and $SH\_EXPORT_{cell,j,t}$ are the percentage of firms, at the industry-region-size level, which respectively are owned by the state, owned by a foreign entity, and export. All the cell averages exclude observations for the firm itself. ***p<0.01, **p<0.05, *p<0.1.

Other firm-level control variables may be endogenous to firm growth. We follow Harrison et al. (2014) and re-aggregate all the firm-level controls included in the baseline regression on cells at the industry-region-size level in each country. When re-aggregating the firm-level variables on cells at the industry-region-size level, we exclude the observation for the firm itself. When the cell is too small (fewer than five firms) or when it is empty (the only observation is for the firm itself), we use the average computed on an industry-region cell. As argued by Harrison et al. (2014), these averaged variables capture the local business environment faced by firms and are less likely to be endogenous to firm performance. We apply this method to all the firm-level controls included in the baseline regression: $SALES_{i,k,j,t-3}$, $EXPORTS_{i,k,j,t}$, $FOREIGN_{i,k,j,t}$, $STATE_{i,k,j,t}$,

and $\text{SIZE}_{i,k,j,t}$. The results are shown in column (5) of Table 4. Replacing the firm-level controls by their mean values on industry-region-size cells does not affect the coefficient of aid, but drastically modifies the results for the variables themselves, since none of them is significant any more. This suggests that the endogeneity of the firm-level variables may well influence the estimated coefficients of the firm-level controls, but does not seem to interfere with the estimation of the coefficient of aid.

In our framework, a more concerning source of omitted variable bias stems from country-level variables that would be correlated with both aid and firm growth. Panel A of Table 5 therefore explores some potential country-level factors that may interfere in the aid-growth relationship. The usual suspects in the aid literature are alternative sources of development finance, as well as proxies for the quality of institutions. We introduce these additional country-level variables in our baseline regression in column (4) of Table 2.[13] This baseline estimation is likely to be the most conservative one, since it includes the firm fixed effects, along with the industry x year dummies, and the country-level and firm-level control variables.

The first three variables that we introduce are $\text{TRADE}_{j,(t,t-3)}$ (exports plus imports in percentage of GDP), $\text{FDI}_{j,(t,t-3)}$ (net foreign direct investment in percentage of GDP), and $\text{REMIT}_{j,(t,t-3)}$ (workers' remittances in percentage of GDP).[14] These variables are alternative sources of development finance which sometimes complement or substitute for aid. None of them is significant in columns (1) to (3) of Table 5, and their inclusion does not significantly affect the coefficient of aid. Our baseline regression already controls for the quality of economic institutions captured by $\text{CORRUPTION}_{j,(t,t-3)}$. In column (4) of Panel A, we further control for the quality of *de jure* political institutions, and include $\text{POLITY}_{j,(t,t-3)}$ (the polity score provided by POLITY IV, Marshall et al. (2013)). It measures the type of political institution on a scale from -10 (autocracy) to +10 (democracy). This variable is not significant and its inclusion does not affect the coefficient of the aid variable. Finally, in column (5) of Table 5, we explore the potential non-linearity that may exist in the aid-growth relationship (Hansen and Tarp, 2000). Aid squared turns out to be non-significant. The coefficient of aid loses its significance (p-value = 0.139) when we include the squared term. The last column of Table 5 tends to suggest that, in our sample of firms, there are no marginal diminishing returns to aid.

[13] All the additional variables are measured as three-year averages between t and t-3.

[14] Data for both $\text{TRADE}_{j,(t,t-3)}$ and $\text{REMIT}_{j,(t,t-3)}$ are from the World Development Indicators. Remittance data for Zambia in period one are not available, hence the smaller size of the sample. Data for $\text{FDI}_{j,(t,t-3)}$ are from UNCTAD. We compute net FDI as FDI inflows minus FDI outflows, and then divide it by GDP.

Table 5: Specification tests, country-level variables.

Dependent: $GROWTH_{i,k,j,(t,t-3)}$	TRADE (1)	FDI (2)	REMIT (3)	POLITY (4)	AID2 (5)
Panel A - Fixed effect estimations					
$AID_{j,(t,t-3)}$	1.985***	2.698**	2.424**	2.027**	3.204
	(0.706)	(1.008)	(0.915)	(0.795)	(2.107)
$VARIABLE_{j,(t,t-3)}$	0.047	1.925	0.147	-0.778	-0.057
	(0.322)	(1.964)	(1.714)	(3.797)	(0.087)
Panel B - Fixed effect IV estimations					
$AID_{j,(t,t-3)}$	2.959*	3.345*	3.076**	2.942*	
	(1.715)	(1.957)	(1.503)	(1.647)	
$VARIABLE_{j,(t,t-3)}$	0.060	2.296	0.492	-1.101	
	(0.325)	(2.220)	(1.938)	(3.856)	
First-step results					
$INSTRUMENT_{j,(t,t-3)}$	1.988***	1.806***	2.162***	1.997***	
	(0.437)	(0.419)	(0.418)	(0.434)	
Observations	8684	8684	8590	8684	8684
Firm fixed effects	yes	yes	yes	yes	yes
Industry x Year dummies	yes	yes	yes	yes	yes
Level of se clustering	country	country	country	country	country
Firm-level controls	yes	yes	yes	yes	yes
Country-level controls	yes	yes	yes	yes	yes
Kleibergen-Paap LM stat. (p-value)	0.079	0.050	0.046	0.073	
Kleibergen-Paap F-stat.	20.75	18.63	26.70	21.24	

Estimations of Panel A using the within estimator, with firm fixed effects, industry x year dummies and robust clustered standard errors at the country level. Estimations of Panel B using the IV estimator, with firm fixed effects, industry x year dummies and robust clustered standard errors at the country level. All estimations include country and firm-level control variables. $TRADE_{j,(t,t-3)}$ is the ratio of exports plus imports over GDP. $FDI_{j,(t,t-3)}$ is the ratio of net foreign direct investment (inflows minus outflows) in GDP. $REMIT_{j,(t,t-3)}$ is the ratio of workers' remittances in GDP. $POLITY_{j,(t,t-3)}$ is the POLITY IV indicator (-10, +10). $AID2_{j,(t,t-3)}$ is the aid squared term. All the additional country-level variables are measured on three-year averages between t and t-3. The underidentification stems from the Kleibergen-Paap rk LM statistic. The weak identification test stems from the Kleibergen-Paap rk Wald F-statistic. The Stock-Yogo weak identification test critical value at 10% is 16.4. ***p<0.01, **p<0.05, *p<0.1.

5.2 Exclusion Restriction

One caveat to the validity of the instrument for aid used in Table 2 would be if, conditional on the controls, the fiscal revenues of each recipient's proximate donors do not affect firm growth only through their impact

on aid. This could notably be the case if donors' budget cycles are correlated with the recipient countries' trade or FDI, which in turn could be correlated with firm performance. In Panel B of Table 5, we check the validity of the instrument when additional controls are added to the more conservative estimation, including firm fixed effects (column (5) of Table 2). The most worrying factors with respect to the exclusion restriction are the alternative sources of development finance - trade, FDI, and remittances - which may be correlated with budget cycles in donor countries. Including these variables does not significantly alter the estimated coefficient of $\text{AID}_{j,(t,t-3)}$, nor does it modify the strength of the instrument as suggested by the Kleibergen-Paap statistics.

5.3 Sample Dependence

Sample dependence is an issue that is particularly acute in the aid effectiveness literature. Clemens et al. (2011) show that sample restrictions (both in terms of countries and period coverage) affects the results of three influential aid effectiveness studies (Boone, 1996; Burnside and Dollar, 2000; Rajan and Subramanian, 2008). Sample dependence may be particularly problematic in our framework since aid is measured at the country level and our sample only comprises 29 countries. In what follows, we provide three sets of robustness checks for sample dependence. First, we check whether our baseline results are not driven by one specific country. Table 15 in Appendix 2 presents the results obtained when the OLS estimation on 25,671 observations, as well as for the fixed-effect estimation on 8,684 observation (columns (1) and (4) of Table 2), are run when each country in the sample is excluded one at a time. Table 15 in Appendix 2 suggests that the coefficient of aid obtained in both the OLS estimation on the full sample of firms and the fixed-effect estimation on the sample of firms for which we have panel data is unaffected by the exclusion of one country at a time.

Second, we explore how the heterogeneity of our sample may affect the results. Table 6 presents the results obtained on different sub-samples of countries. We run these sub-sample tests on the OLS estimation on the full sample of firms, as well as on the fixed-effect estimation on the sample of firms for which we have panel data (columns (1) and (4) of Table 2). In Panel A, the coefficients of $\text{AID}_{j,(t,t-3)}$ obtained in Table 2 are reproduced. In Panel B, the estimations are run on the sub-sample of firms located in Latin American countries. In these 16 countries, the effect of aid on firm growth is positive and significant. The coefficient of $\text{AID}_{j,(t,t-3)}$ turns out to be slightly higher than when estimated on the entire sample. However, a 10% increase in aid would lead to a 5.6% increase in average growth, which is slightly lower than when all the countries are included in the estimations.

Table 6: Sample dependence.

Dependent: $GROWTH_{i,k,j,(t,t-3)}$	OLS	FE
Coefficient for $AID_{j,(t,t-3)}$	(1)	(2)
Panel A: All sample	1.388***	1.980**
	(0.448)	(0.721)
Observations	25,673	8,684
Impact of aid (+10%)	4.8	7.5
Panel B: Latin America	1.850***	2.718***
	(0.698)	(0.804)
Observations	17,797	5,622
Impact of aid (+10%)	3.1	5.6
Panel C: Africa	1.843 (p = 0.108)	3.945***
	(1.148)	(0.634)
Observations	5,756	1,958
Impact of aid (+10%)	15.9	36.3
Panel D: Without ARG, BRA, MEX	1.231***	1.975**
	(0.454)	(0.933)
Observations	18,992	6,864
Impact of aid (+10%)	6.1	10.8
Panel E: Without MWI, ZMB, NER	1.627***	1.981**
	(0.540)	(0.947)
Observations	24,725	8,408
Impact of aid (+10%)	4.5	6.2
Panel F: Manufacturing only	1.851***	2.830**
	(0.539)	(1.107)
Observations	19,056	6,782
Impact of aid (+10%)	6.6	12.4
Firm fixed effects	no	yes
Country fixed effects	yes	no
Industry x Year dummies	yes	yes
Level of se clustering	firm	country
Firm-level controls	yes	yes
Country-level controls	yes	yes

In column 1, estimations use the OLS estimator, with country fixed effects, industry x year dummies and robust clustered standard errors at the firm level. In column 2, estimations use the within estimator, with firm fixed effects, industry x year dummies and robust clustered standard errors at the country level. All estimations include country and firm-level control variables. Impact of aid displays the percentage increase in firms' average growth for a 10% increase in aid.***p<0.01, **p<0.05, *p<0.1.

Panel C of Table 6 displays the results when the baseline estimations are run on the sample of African countries only. In these 11 countries, the impact of aid loses significance when firm fixed effects are not

accounted for (p-value = 0.108). However, in our preferred specification with firm fixed effects, aid has a positive and significant impact on firm growth, which is quite large: a 10% increase in aid would lead to a 36% increase in average growth. Given the lower average growth rate of African firms (7.8%) compared to Latin American firms (9.7%) in our sample, it seems that more aid can effectively be absorbed in Africa than in Latin America, on average. This result is consistent with the higher impact of aid which is found in the IV estimations presented in Table 2, since the instrument used for aid is likely to predict aid to Africa much better than aid to Latin America.

In Panel D, we drop the countries which receive the least aid from the sample: Argentina, Brazil, and Mexico. These countries receive less than 0.6% of their GDP in aid. The coefficient for aid is virtually unaltered, but the impact of a 10% increase in aid slightly increases compared to Panel A. We then drop the three countries in the sample which receive the most aid: Malawi, Zambia, and Niger. These three countries receive more than 15% of their GDP in aid. Again, the results displayed in Panel E suggest that dropping these three countries does not modify the coefficient of aid, but slightly decreases the impact of a 10% increase in aid.

Finally, in Panel F, the benchmark estimations are run on the sample of manufacturing firms only. Around one-fourth of the firms operate in services. Dropping these firms from the sample does not significantly modify the results: the coefficient of aid remains positive and significant. The magnitude of the effect of aid is found to be slightly higher for manufacturing firms than on the whole sample, notably in column (2): a 10% increase in aid would increase firm growth by around 12%, compared to 7.5% on the whole sample.

As presented in Appendix 1, different samples of firms have been surveyed in the countries of our sample. When estimated on the whole sample of firms, the number of observations varies from 156 (Niger) to 2,643 (Brazil). For the firms for which we have panel data, the number of observations varies from 62 (Niger) to 782 (Brazil). As our variable of interest, aid, is measured at the country level, this implies that some countries, those where a higher number of firms were surveyed, are over-represented in the sample. In Table 7, we display the results when each country is given the same weight, by randomly drawing the same number of enterprises from each survey.

In columns (1) to (3), we run this test on the OLS baseline results of column (1) on Table 2. We first randomly draw 200 observations for each country, and then expand the number of observations to 250 (column (2)) and to 300 (column (3)). Each random draw is replicated 500 times, and we then compute the mean value of the coefficient $\text{AID}_{j,(t,t-3)}$, as well as its standard deviation and the percentage of estimations in which this coefficient is not significantly different from zero. Columns (1) to (3) of Table 7 show an

average coefficient for aid that is very close to the one estimated on the full sample, and between 8 to 11% of estimations in which it was not significantly different from zero.

Table 7: Random draw of firms.

Dependent: $GROWTH_{i,k,j,(t,t-3)}$	OLS			FE		
Nb of firms randomly drawn	200	250	300	40	70	100
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient of $AID_{j,(t,t-3)}$, 500 replications						
Mean	1.628	1.536	1.448	2.413	2.184	2.122
Standard deviation	0.457	0.383	0.339	0.560	0.235	0.190
Percent not significant	11.0	8.8	8.2	5.2	0	0
Observations	5,753	7,103	8,327	2,302	3,752	4,758
Firm fixed effects	no	no	no	yes	yes	yes
Country fixed effects	yes	yes	yes	no	no	no
Industry x Year dummies	yes	yes	yes	yes	yes	yes
Level of se clustering	firm	firm	firm	country	country	country
Firm-level controls	yes	yes	yes	yes	yes	yes
Country-level controls	yes	yes	yes	yes	yes	yes

In columns 1 to 3, estimations are run using the OLS estimator, with country fixed effects, industry x year dummies and robust standard errors clustered at the firm level. In columns 4 to 6, estimations are run using the within estimator, with firm fixed effects, industry x year dummies and robust standard errors clustered at the country level. All estimations include country and firm-level control variables. ***p<0.01, **p<0.05, *p<0.1.

In columns (4) to (6) of Table 7, we perform the same test on the firms for which we have panel data, and first randomly draw 40 firms (column (4)), then 70 (column (5)), and 100 (column (6)). The coefficients for aid on these randomly drawn samples are again very close to the coefficients for aid in column (4) of Table 2, and the percentage of times for which they were not significantly different from zero varies from 0 to 5.2. Overall, Table 7 suggests that the estimated coefficient for aid in the baseline results does not stem from a handful of over-represented countries.

5.4 Attrition of Firms in the Fixed-Effect Estimations

World Bank Enterprise Surveys are sampled in so as to be representative at the country level, with three levels of stratification: industry, region, and size. However, firms are representative for each survey round; but firms which were interviewed twice (two rounds) only represent one fourth of the total initial sample of firms. Obviously, there is no reason to believe that firms that were interviewed twice are representative at the national level.

One related issue is that of selection bias. Clearly, the fact that the results in OLS on 25,673 observations

and using the within estimator on the panel of firms on 8,684 observations (columns (1) and (4) of Table 2) lead to a very similar impact of aid suggests that attrition may not be that much of an issue in our framework. However, if firms were interviewed twice because they were more likely to survive (better performance, specific activity, etc.), and if the probability of surviving is somewhat related to how much aid is received in the country, then the estimated effect of aid would be biased, and most probably biased upward.

In Table 16 in Appendix 3, we present simple mean-comparison tests of the growth rate of firms, comparing those which survive period two in our dataset with those which do not. We compare the initial performance of these two groups in period one. The results suggest that the initial performances of the two groups of firms are not significantly different. We also compare the firms that appear in period two to the firms that were already in the sample in period one. The growth rates of the two groups of firms do not seem to be significantly different in the second period. These simple mean-comparison tests suggest that on average the firms for which we have two points in time did not grow significantly differently from the firms which did not survive period two in our dataset or which appear for the first time in the dataset in period two.

In Table 8, we further explore the difference in growth performance for firms for which we have panel data and firms which disappear from the sample between period one and two, or appear in the sample in period two. We create a dummy which is equal to one if the firm belongs to the panel dataset, $\text{PANEL}_{i,k,j,t}$. We then estimate the difference in growth performance for the two sets of firms (those which are observed twice and those which are not) controlling for firm-level and country-level variables, country fixed effects, and industry x year dummies. Of course, we cannot include the firm fixed effects in these regressions, so we use both the OLS and IV estimators to make sure that endogeneity due to omitted variables does not bias the results. Columns (1) and (2) of Table 8 show that the coefficient of $\text{PANEL}_{i,k,j,t}$ is not significantly different from zero, suggesting that the firms for which we have panel data do not display a significantly different growth performance when a large set of controls is accounted for. The impact of aid remains significantly positive when $\text{PANEL}_{i,k,j,t}$ is included in the baseline estimation, whether it is estimated using the OLS or the IV estimator. In column (2), only $\text{AID}_{j,(t,t-3)}$ is instrumented, that is $\text{PANEL}_{i,k,j,t}$ is assumed to be exogenous.

If aid adversely impacts firms which do not survive period two, we could over-estimate the impact of aid when we drop these firms from the sample in the fixed-effect estimations. In columns (3) and (4) of Table 8, we therefore examine whether the impact of aid on firm growth is different for the two sets of firms, by interacting $\text{AID}_{j,(t,t-3)}$ with $\text{PANEL}_{i,k,j,t}$. Indeed, column (3) suggests that the impact of aid is significantly different for firms for which we have panel data and for firms for which we do not. But the difference goes in

the sense of an under-estimation of the effect of aid on the sample of firms for which we have panel data. The interaction term is significant and negative, suggesting that on average, the impact of aid on firm growth is 0.353 percentage points lower when firms survive in period two. The interaction term is no longer significant in column (4) when aid is instrumented (p-value = 0.128).[15] However, its sign remains negative.

Table 8: Exploring whether the results are driven by attrition.

Dependent: $GROWTH_{i,k,j,(t,t-3)}$	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)
$AID_{j,(t,t-3)}$	1.394**	3.068***	1.594***	3.808***	1.520*	3.691***
	(0.447)	(0.735)	(0.452)	(0.888)	(0.691)	(1.109)
$PANEL_{i,k,j,t}$ (dummy)	-0.495	-0.527	0.527	3.532		
	(0.448)	(0.446)	(0.490)	(2.766)		
$AID_{j,(t,t-3)}$ x $PANEL_{i,k,j,t}$			-0.353***	-1.403		
			(0.101)	(0.921)		
First-step: $AID_{j,(t,t-3)}$						
$INSTRUMENT_{j,(t,t-3)}$		1.919***		1.919***		1.804***
		(0.029)		(0.029)		(0.038)
$INSTRUMENT_{j,(t,t-3)}$ x $PANEL_{i,k,j,t}$				0.0005*		
				(0.0002)		
First-step: $AID_{j,(t,t-3)}$ x $PANEL_{i,k,j,t}$						
$INSTRUMENT_{j,(t,t-3)}$				0.996***		
				(0.077)		
$INSTRUMENT_{j,(t,t-3)}$ x $PANEL_{i,k,j,t}$				0.033***		
				(0.003)		
Observations	25,673	25,673	25,673	25,673	16,989	16,989
Firm fixed effects	no	no	no	no	no	no
Country fixed effects	yes	yes	yes	yes	yes	yes
Industry x Year dummies	yes	yes	yes	yes	yes	yes
Level of se clustering	firm	firm	firm	firm	firm	firm
Firm-level controls	yes	yes	yes	yes	yes	yes
Country-level controls	yes	yes	yes	yes	yes	yes
Kleibergen-Paap LM stat. (p-value)		0.000		0.000		0.000
Kleibergen-Paap F-stat		4452		53.5		2285

Columns (1) to (4) are estimated on the full sample of firms. Columns (5) and (6) are estimated on the sample of firms for which we do not have panel data. Columns (1), (3), and (5) are estimated using the OLS estimator, with country and industry x year dummies and robust standard errors clustered at the firm level. Columns (2), (4), and (6) are estimated using the IV estimator, with country and industry x year dummies and robust standard errors clustered at the firm level. In column (4), the instrument is instrumented with the dummy $PANEL_{i,k,j,t}$. The underidentification stems from the Kleibergen-Paap rk LM statistic. The weak identification test stems from the Kleibergen-Paap rk Wald F-statistic. The Stock-Yogo weak identification test critical value at 10% is 16.4 for columns (2) and (6), and 7.03 for column (4). ***p<0.01, **p<0.05, *p<0.1.

[15] In this case, $AID_{j,(t,t-3)}$ x $PANEL_{i,k,j,t}$ is also instrumented, and we use $INSTRUMENT_{j,(t,t-3)}$ x $PANEL_{i,k,j,t}$ as an additional instrument.

In columns (5) and (6) of Table 8, we directly look at the impact of aid on firms for which we only have one observation instead of two. If aid were to adversely impact these firms, then dropping them in the panel estimation would bias the coefficient of $\text{AID}_{j,(t,t-3)}$ upward. In the last two columns of Table 8, the number of observations drops to 16,989. The coefficient of $\text{AID}_{j,(t,t-3)}$ remains significantly positive, and very similar to the results of column (1) in Table 2. This suggests that firms which do not survive period two or appear in period two are not adversely impacted by aid.

In Table 9, we further investigate whether firm selection may bias our results. Using all firms in the sample, we re-aggregate the data on various levels of aggregation. The larger the level of aggregation of the cells, the smaller the percentage of attrited cells should be. The cells are first aggregated at the industry-region-size level, which for some cells (those with few firms) is very close to the firm-level observations.[16] The attrition issue is not fully solved by this approach since some cells are represented in only one of the two periods. However, while at the firm level the firm fixed effects induce a loss of two-thirds of the sample, at the industry-region-size level, 'only' thirty percent of the cells are lost due to the cell fixed effects.

We then further aggregate the data on cells at the industry-region level. In this case, 16% of the cells only include one firm. In order to avoid that the performance of one specific firm drives the results, we drop the cells which are only composed of one firm.[17] At the industry-region level, the attrition of cells due to the panel dimension is further reduced: 24% of the cells only have one observation in time.

Finally, we aggregate the data on cells at the industry level. In this case, 7% of the cells rely on only one firm, 16% of them on fewer than four firms. Again, to avoid a small set of firms driving the results, we drop the cells that are based on fewer than four firms. Re-aggregation at the industry level further reduces the degree of attrition of cells: 13% of them are lost because one time-period is missing.

Table 9 presents the results when the data is re-aggregated at these various levels. We present the results both when aid is not instrumented (Panel A) and instrumented (Panel B), with and without the cell fixed effects. In all specifications, we drop the industry x year dummies which are too demanding for higher levels of aggregation. Table 9 suggests that the impact of aid on average cell growth is very similar to the one found in Table 2. This is particularly true when aid is not instrumented: in Panel A, the estimated coefficient of aid is very similar to the one found in our baseline results. In Panel B, when $\text{AID}_{j,(t,t-3)}$ is instrumented, the IV estimations lead to a jump in the coefficient of $\text{AID}_{j,(t,t-3)}$, whatever the level of aggregation. This may reflect the fact that the endogeneity of aid is more acute when it is estimated on more aggregated cells. It

[16] Around one-fourth of the cells aggregated at the industry-region-size level only include one firm.

[17] We cannot do the same at the industry-region-size level since we would lose too many cells. Given that one-fourth of the sample is composed of cells with only one firm, the results at the industry-region-size are expected to be very similar to those at the firm level.

may also reflect the violation of the exclusion restriction. The more aggregated the outcome, the more likely omitted variables are correlated with both the instrument (fiscal situation in the donor country) and the outcome. The instrument, despite the stability of its coefficient across columns (1)-(6), also seems to perform more poorly when cells are aggregated at higher levels than firms (as suggested by the Kleibergen-Paap LM and F-statistics, notably in the fixed-effect estimations).

Table 9: Re-aggregating at the industry-region-size, industry-region, and industry level.

Level of cells	Indus-region-size		Indus-region		Industry	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	OLS	FE	OLS	FE	OLS	FE
$AID_{j,(t,t-3)}$	1.556**	1.588**	1.527**	1.548*	1.731**	1.627*
	(0.669)	(0.713)	(0.747)	(0.792)	(0.706)	(0.872)
Panel B	IV	FE-IV	IV	FE-IV	IV	FE-IV
$AID_{j,(t,t-3)}$	7.502***	7.659*	7.241***	6.506*	6.229***	5.998*
	(1.502)	(4.005)	(1.574)	(3.378)	(1.650)	(3.496)
First-step results						
$INSTRUMENT_{j,(t,t-3)}$	1.099***	1.149**	1.113***	1.165**	1.133***	1.156**
	(0.066)	(0.418)	(0.105)	(0.434)	(0.187)	(0.476)
Observations	3,040	3,040	1,354	1,354	468	468
Number of cells	1,520	1,520	677	677	234	234
Percent of attrited cells	29.7	29.7	23.6	23.6	12.7	12.7
Country fixed effects	yes	no	yes	no	yes	no
Industry x Year dummies	no	no	no	no	no	no
Cell FE	no	yes	no	yes	no	yes
Level of se clustering	cells	country	cells	country	cells	country
Cell-level controls	yes	yes	yes	yes	yes	yes
Country-level controls	yes	yes	yes	yes	yes	yes
Kleibergen-Paap LM stat. (p-value)	0.000	0.049	0.000	0.061	0.000	0.089
Kleibergen-Paap F-stat	278.2	7.57	113.1	7.21	36.7	5.90

In the upper part (Panel A) of columns 1, 3 and 5, OLS estimations with country dummies and robust clustered standard errors at the firm level. In the upper part (Panel A) of columns 2, 4 and 6, estimations use the within estimator, with firm fixed effects, and robust clustered standard errors at the country level. In the lower part (Panel B) of columns 1, 3 and 5, IV estimations with country, and robust clustered standard errors at the firm level. In the lower part (Panel B) of columns 2, 4 and 6, estimations use the IV estimator, with firm fixed effects and robust clustered standard errors at the country level. All estimations include country and cell-level control variables. The underidentification stems from the Kleibergen-Paap rk LM statistic. The weak identification test stems from the Kleibergen-Paap rk Wald F-statistic. The Stock-Yogo weak identification test critical value at 10% is 16.4; it is 6.66 at 20% and 5.53 at 25%. ***p<0.01, **p<0.05, *p<0.1.

6 Mechanisms

6.1 Through Which Channels Does Aid Affect Firm Growth?

As discussed in Section 2, many mechanisms could explain the fact that aid influences firm growth in a positive way. From a demand-side perspective, aid may provide firms with contracts, for example in the infrastructure sector. Large firms, firms owned by the state or by foreign investors are more likely to benefit from these infrastructure contracts. From a supply-side perspective, aid invested in factors which are complementary to firms' activities may crowd in private investment. This may particularly be the case when firms' activities are constrained by poor-quality infrastructure. Aid may also relax the financing constraint faced by firms. In a credit-constrained environment, aid may provide the government with financing, hence making more credit available for private firms.

The literature has pointed out that aid may also have adverse effects on growth outcomes through the Dutch disease mechanism or through the deterioration of institutions, as documented by Rajan and Subramanian (2007, 2011). To explore these mechanisms, Rajan and Subramanian (2007, 2011) look at the effect of aid on the manufacturing industry's growth rate. They interact aid with indicators meant to capture whether the industries are more prone to exporting or rely more on institutions (using Levchenko (2007)'s data of dependence on institutions).[18] This strategy was first implemented by Rajan and Zingales (1998). The idea of this approach is to filter the impact of a country-level variable on industry-level outcomes using an exogenous industry characteristic, reflecting the United States' situation. The specification includes country-year dummies, which account for time-varying heterogeneity at the country level and largely reduce the omitted variable bias. It also includes the country-level variable of interest in interaction with sector-specific structural characteristics, (*e.g.* dependence on external finance, on institutions, or proneness to export). This interaction term allows to explore the channels through which the country-level variable may affect the outcome.

Given the positive effect of aid found so far on firms' sales growth, we rather expect aid to affect firm growth either by increasing demand or by providing complementary factors and relaxing the financing constraint. However, aid may also have adverse effects that are compensated for by positive ones, and when exploring the channels, we do not rule out these negative effects *a priori*. We therefore investigate four channels through which aid may influence firm growth: Dutch disease, institution curse, finance, and infrastructure. Each of them is captured by a different industry-level characteristic, INTENSITY_k, which reflects

[18] Levchenko (2007) himself uses the data on institutional dependence computed by Cowan and Neut (2007).

the structural dependence of each industry on either exports, institutions, external finance, or infrastructure. We use the following general specification:

$$GROWTH_{i,k,j,(t,t-3)} = \alpha + \beta X_{i,k,j,t} + \delta AID_{j,(t,t-3)} * INTENSITY_k + \mu_i + \gamma_{j,t} + \tau_{k,t} + \varepsilon_{i,k,j,t} \qquad (3)$$

where we replace the country-level variables in Equation 1 with country x year dummies, $\gamma_{j,t}$, and include an interaction term of aid with various industry-level intensities, INTENSITY$_k$. Since for most measures of INTENSITY$_k$, the data is only available for manufacturing industries, we estimate Equation 3 on the sample of manufacturing firms only (see Panel F of Table 6 for the estimation of the benchmark model on this sub-sample of firms).

First, we follow Rajan and Subramanian (2011) and examine whether aid differentially impacts growth when firms are more prone to exporting (the Dutch disease mechanism). In Rajan and Subramanian (2011), the proneness to export of industry k, EXPORT$_k$, is measured using a dummy variable which takes the value one for industries with a ratio of exports to value-added greater than the median across industries in their sample of countries. The WBES data only allows to distinguish between eight manufacturing industries, which is a far more aggregated level than Rajan and Subramanian (2011). For some firms, the WBES provide the ISIC revision 3 code of the main product. When it is not available, the WBES provides a description of the main product sold by the firm, which we used to determine its ISIC code.[19] Using these codes, we matched Rajan and Subramanian (2011)'s exportability indicator presented in Appendix Table 1C of their article with our own data.[20] We then re-aggregated these codes for the eight industries of the WBES. Table 10 presents the corresponding EXPORT$_k$ variable. It suggests that chemicals, electronics, and non-metallic mineral industries are less intensive in exports than the five other sectors.

In order to examine the governance curse channel, we use INSTITUTION$_k$, the institutional dependence of industry k provided by Levchenko (2007) on his website. Levchenko (2007) uses a measure of product complexity computed as the Herfindahl index of intermediate input use, using the U.S. Input-Output Use Table for 1992. The baseline idea of this indicator is to measure whether institutions are more important to specific industries. This should be the case when their activity is based on other industries' inputs, since this requires more contract and better functioning institutions. The Herfindahl index is multiplied by minus one so that a higher fragmentation captures a higher institutional dependence.[21] Table 10 displays the mean

[19] For 2.23% of the manufacturing firms for which we have panel data (151 firms), we were not able to find the ISIC code for the firm's main product.

[20] Rajan and Subramanian (2011)'s exportability indicator is presented for ISIC revision 2 codes

[21] Again, the data provided by Levchenko (2007) are far more disaggregated than our eight industries. He uses the 4-digit 1987 US SIC classification. Using the ISIC revision 3 codes for the firms' main product, we matched Levchenko (2007)'s data

value of institutional dependence for each industry. It is higher for machinery and equipment, non-metallic minerals, electronics, and chemicals than for food, textiles, and garments.

In order to examine whether aid is differentially effective when firms face a financing constraint, we use Kroszner et al. (2007)'s computation of external finance dependence in the 80s (see Table 12 of their article) for U.S. industries, FINANCE_k.[22] We matched these data with our eight industries in the same way as for EXPORT_k. As shown in Table 10, garments and food seem relatively less intensive in external finance than other industries.

The last channel explored is the infrastructure channel. We use two different kinds of infrastructure intensity, ELECTRICITY_k and TRANSPORT_k. We compute the intensity in electricity using U.S. data on electricity consumption by industry (per dollar of value-added) provided by the U.S. Energy Information Administration in the Manufacturing Energy Consumption Survey (MECS). The aggregation into eight industries follows the same principle as before. The electricity data is categorised following the NAICS codes which we matched to the ISIC revision 3 codes, and averaged for the eight industries in the sample. Table 10 shows that the most electricity-intensive industries are non-metallic minerals, chemicals, and textile industries. Unfortunately, we do not have U.S. industry data on transport intensity. We therefore use the WBES information on the percentage of material inputs and supplies of foreign origin in the last fiscal year. We compute the average of this variable for industry k across countries and time. Table 10 suggests that intensity in transport is higher for chemicals, electronics, and textile.

Table 10: Industry intensities.

Industry	EXPORT_k	INSTITUTION_k	FINANCE_k	ELECTRICITY_k	TRANSPORT_k
Food	0.899	-0.166	0.130	0.039	0.202
Garments	1.000	-0.143	0.009	0.009	0.330
Textile and leather	1.000	-0.145	0.400	0.048	0.367
Machinery and equipment	0.390	-0.098	0.388	0.015	0.309
Chemicals	0.037	-0.121	0.667	0.057	0.479
Electronics	0.252	-0.108	0.757	0.012	0.396
Non-metallic minerals	0.000	-0.081	0.221	0.157	0.205
Other manufacturing	0.417	-0.137	0.280	0.053	0.289

with ours and computed averages over the eight industries in the dataset.

[22]"External dependence is the fraction of capital expenditures not financed with cash flow from operations. Cash flow from operations is defined as in Rajan and Zingales (1998)" (Kroszner et al., 2007, p.222).

Equation 3 is estimated in Panel A of Table 11. Unlike Rajan and Subramanian (2011) and Rajan and Subramanian (2007), we find no significant impact of the interaction term of aid with EXPORT_k and INSTITUTION_k. Turning to the interaction term of aid with external finance dependence, FINANCE_k, we find a positive and significant impact, suggesting that aid is more effective in spurring the growth of firms in sectors which rely more on external finance. This suggests that aid tends to relax the financing constraint faced by firms. This result is also in opposition with what was found by Rajan and Subramanian (2011). The discrepancy between our results and those of Rajan and Subramanian (2007, 2011) may stem from various factors. First, we do not work at the same level of aggregation, since our data is at the firm level, while theirs are aggregated at the industry level. Second, the dependent variable is different in our study - sales growth rate - and in theirs - industry value added growth rate. Finally, the sample of countries we work on is very different from theirs, since their core sample is composed of low-income countries receiving more than 1% of their GDP in aid. In order to be able to fully compare our results with theirs, we should compute the value-added - which is not possible due to poor data on intermediate consumption goods -, and drop all the middle-income countries from our sample - which is not possible since we would then end up with too few countries. Turning to the infrastructure channel, we find that aid manages to increase the growth rate of firms which belong to industries which are highly intensive in electricity and transport, given the significant and positive coefficients of the interaction terms of aid with both ELECTRICITY_k and TRANSPORT_k. Aid therefore seems to be relatively more effective in increasing the sales growth of firms belonging to sectors whose activity is more dependent on infrastructure. This is consistent with the idea that aid may relax the infrastructure constraint hampering firms' activities in developing countries.

In the last row of Panel A in Table 11, we follow Rajan and Zingales (1998) and present the difference in growth rates between firms that belong to highly intensive industries (75th percentile) and firms that belong to low-intensity industries (25th percentile), in countries receiving high amounts of aid (75th percentile) and those receiving low amounts of aid (25th percentile).[23] Column (3) of Table 11 suggests that firms which belong to an industry whose activity is structurally more dependent on external finance tend to have a growth rate that is 1.454 percent higher when they are located in a country receiving high amounts of aid rather than in one receiving low amounts of aid.

[23] We compute the difference in predicted growth rate of a switch from the 25th to the 75th percentile of INTENSITY_k, in both high- and low-aid countries. We then compute the difference in predicted growth between high- and low-aid countries.

Table 11: Exploring the channels.

Intensity in:	$EXPORT_k$ (1)	$INSTITUTION_k$ (2)	$FINANCE_k$ (3)	$ELECTRICITY_k$ (4)	$TRANSPORT_k$ (5)
Panel A					
$AID_{j,(t,t-3)}$ x $INTENSITY_k$ss	-0.709	6.798	2.500***	17.916**	5.867**
	(0.557)	(11.942)	(0.788)	(6.854)	(2.742)
Differential in growth	-0.959	0.343	1.454	1.461	0.973
Panel B - Alternative measures of intensity					
$AID_{j,(t,t-3)}$ x $INTENSITY_k$	1.446*	-3.627	2.527**	94.877**	7.751**
	(0.821)	(47.067)	(1.013)	(43.974)	(3.049)
Differential in growth	3.117	-0.049	1.458	1.831	1.939
Observations	6,782	6,782	6,782	6,782	6782
Number of firms	3,391	3,391	3,391	3,391	3,391
Firm fixed effects	yes	yes	yes	yes	yes
Country x Year dummies	yes	yes	yes	yes	yes
Industry x Year dummies	yes	yes	yes	yes	yes
Level of se clustering	country	country	country	country	country
Firm-level controls	yes	yes	yes	yes	yes
Country-level controls	no	no	no	no	no

The dependent variable is $GROWTH_{i,k,j,(t,t-3)}$. Estimations using the within estimator, with firms fixed effects, industry x year dummies, country x year dummies, and robust clustered standard errors at the country level. All estimations include firm-level control variables. To compute the differential in growth we first calculate the difference in predicted growth rate of a switch from the 25th to the 75th percentile of $INTENSITY_k$, in both high and low aid countries. We then compute the difference in predicted growth between high (75th percentile) and low (25th percentile) aid receiving countries. ***p<0.01, **p<0.05, *p<0.1.

In Panel B of Table 11, we provide a robustness check using alternative measures of $INTENSITY_k$. The alternative measure of $EXPORT_k$ is provided by Rajan and Subramanian (2011), and considers textile and garments as the only exporting sectors. $INSTITUTION_k$ is provided by Levchenko (2007) as the Gini coefficient of intermediate use shares. $FINANCE_k$ is provided by Kroszner et al. (2007) as external dependence over 1980-1999. The alternative measure of $ELECTRICITY_k$ stems form the WBES which provides information on the cost of electricity incurred by firms. We measure this cost as a percentage of firms' sales. In order to get a measure which makes sense for countries of various development levels, we computed the average cost of electricity for firms belonging to the same sector and same continent. We did not find any obvious alternative measure for $TRANSPORT_k$ and therefore only tested whether the results are consistent when $TRANSPORT_k$ varies across continents, *i.e.* computed the average of imported inputs across sectors and continents. These intensity variables are presented in Table 17 in Appendix 4. Panel B of Table 11 tends to confirm the results obtained in Panel A. Except for $EXPORT_k$, the impact of the

interaction terms is similar, as is the magnitude of the effect presented as the differential in growth. The interaction term of $\text{AID}_{j,(t,t-3)}$ with EXPORT_k turns out to be significantly positive when only textile and garments are considered as exporting sectors. This is the opposite of what the Dutch disease mechanism predicts and suggests that on our sample of firms, aid tends to complement the activity of exporting firms from the textile and garment sectors.

6.2 Do Some Firms Benefit More from Aid than Others?

So far, our results tend to suggest that aid has a positive impact on firm growth that could be channelled through relieving both the financing and infrastructure constraints. This mainly favors the supply-side interpretation of the effect of aid: by providing the complementary factors required for firms' activities, aid could improve their productive capacity.

The demand-side mechanism cannot be ruled out however. One question that arises from the previous analysis is whether the impact of aid is driven by a sub-sample of firms with specific characteristics: large firms, with political connections, or better access to the markets financed by aid (infrastructure construction notably). One way to examine this question is to run our baseline regressions (columns (1) and (4) of Table 2) on various sub-samples of firms, depending on characteristics such as size and ownership. Because some of the sub-samples of interest are quite small,[24] we do not restrict the samples to these categories, but rather drop them from the full sample to see whether these firms drive the results.

Panel A of Table 12 reproduces the baseline results on manufacturing firms. Then, in Panels B and C, we examine whether large firms (more than 100 employees) or small ones (fewer than 20 employees) drive the results. Dropping these two categories of firms does not substantially modify our results. In Panel D, state-owned firms are dropped from the sample. State-owned firms is the closest variable for political connections, but it is a very rough proxy. Given the very small number of observations that are dropped, it is not a surprise that the results are unaffected.

Aid procurements are sometimes designed by donor countries so as to favor its own domestic firms or their affiliates. Fully or partly foreign-owned firms may therefore benefit more from aid contracts than other domestic firms. To explore whether the positive effect of aid on firm growth is driven by foreign-owned firms, we drop them from the sample. The results are presented in Panel E of Table 12. The impact of aid is very similar to what is found in Panel A: a 10% increase in aid could increase growth by between 6 and 13%. Finally, Panels F and G of Table 12 examine whether exporting and non-exporting firms are differentially

[24] For the panel data, 48 observations belong to the state-owned group, 761 observations to the foreign-owned group, 1,965 are large firms, and 2,193 are small firms.

affected by aid. The coefficient for aid remains significantly positive on both sub-samples, with a slightly higher impact of aid on exporting firms.

Our framework allows us to explore whether the four channels - Dutch disease, institution curse, financing constraint, and infrastructure constraint - play out differentially for the various sub-categories of firms highlighted in the previous section. In Table 13, we therefore re-estimate Equation 3 for the different sub-samples of firms examined in Table 12. Panels A, C, and D suggest that aid, by relaxing the financing and infrastructure constraints, complements the activity of small and medium-size firms, which are not owned by the state or by foreign entities.

In Panel B of Table 13, we drop the smallest firms of the sample (fewer than 20 employees). This leads the interaction term of aid with the intensity in infrastructure proxies to lose their significance. Comparing this result with Panel A of Table 11 suggests that the impact of aid that goes through the infrastructure channel is mainly driven by the smallest firms in the sample. The distinction between exporting and non-exporting firms is striking. Exporting firms seem to benefit from better functioning infrastructures, as well as from an increased access to finance (Berman and Héricourt, 2010). On the contrary, despite the positive effect of aid on non-exporting firms found in Table 12, none of the four channels analyzed seem to match this sub-set of firms. This suggests that the impact of aid on non-exporting firms follows different routes (education, for example). In Table 18 in Appendix 5, we provide a robustness check of this analysis using the alternative measures of INTENSITY_k. The results are broadly consistent with what is found in Table 13.

A key question is whether the effect of aid, while benefiting some firms, is crowding out others. Tables 12 and 13 overall suggest that the positive effect of aid is robust to various sub-categories of firms. This suggests that the demand-side mechanism - aid providing contracts to specific categories of firms - is not that much at play on our sample of firms. However, this does not rule out that aid could adversely impact some other firms and crowd them out. Our sample of firms has very specific characteristics: relatively large (almost no individual businesses) firms from the formal sector. A large part of developing countries' economic activity stems from the informal sector. The positive effect of aid on the formal firms in our sample may hide an adverse impact of aid on informal firms. This may explain why some studies at the aggregate level find no effect of aid on GDP growth, since GDP includes an estimation of the size of the production by the informal sector.

Table 12: Impact of aid for different kinds of manufacturing firms.

	OLS	FE
	(1)	(2)
Panel A: All manufacturing	1.851***	2.830**
	(0.539)	(1.107)
Observations	19,056	6,782
Impact of aid (+10%)	6.6	12.4
Panel B: Under 100 employees	1.477**	1.690*
	(0.650)	(0.886)
Observations	13,780	4,817
Impact of aid (+10%)	6.0	9.1
Panel C: More than 20 employees	2.287***	3.129**
	(0.633)	(1.322)
Observations	12,400	4,589
Impact of aid (+10%)	6.3	10.5
Panel D: Not owned by the state	1.882***	2.910**
	(0.541)	(1.093)
Observations	18,930	6,734
Impact of aid (+10%)	6.7	12.7
Panel E: Not foreign-owned	1.711***	3.016**
	(0.622)	(1.135)
Observations	16,913	6,021
Impact of aid (+10%)	6.1	13.1
Panel F: Not exporting	1.372**	3.389***
	(0.641)	(0.962)
Observations	12,011	3,975
Impact of aid (+10%)	5.9	18.4
Panel G: Exporting	2.870***	6.145***
	(1.100)	(1.530)
Observations	7,045	2,807
Impact of aid (+10%)	7.3	18.6
Firm fixed effects	no	yes
Country fixed effects	yes	no
Industry x Year dummies	yes	yes
Level of se clustering	firm	country
Firm-level controls	yes	yes
Country-level controls	yes	yes

In column 1, estimations use the OLS estimator, with country fixed effects, industry x year dummies and robust clustered standard errors at the firm level. In column 2, estimations use the within estimator, with firm fixed effects, industry x year dummies and robust clustered standard errors at the country level. All estimations include country and firm-level control variables. ***p<0.01, **p<0.05, *p<0.1.

Table 13: Channels depending on the characteristics of the firms.

Intensity in:	$EXPORT_k$	$INSTITUTION_k$	$FINANCE_k$	$ELECTRICITY_k$	$TRANSPORT_k$
	(1)	(2)	(3)	(4)	(5)
Panel A: Under 100 employees	-0.924	12.979	1.988*	16.931**	5.926*
	(0.587)	(12.084)	(1.051)	(7.513)	(3.288)
Observations	4,817	4,817	4,817	4,817	4,817
Panel B: More than 20 employees	-1.296	20.462	3.697*	13.281	6.684
	(1.193)	(16.577)	(1.980)	(11.515)	(5.913)
Observations	4,589	4,589	4,589	4,589	4,589
Panel C: Not owned by the state	-0.890	8.269	2.778***	18.491**	6.788**
	(0.574)	(12.031)	(0.867)	(6.968)	(2.705)
Observations	6,734	6,734	6,734	6,734	6,734
Panel D: Not foreign-owned	-1.023	2.371	2.835**	23.926**	8.076*
	(0.942)	(15.968)	(1.031)	(10.706)	(4.636)
Observations	6,021	6,021	6,021	6,021	6,021
Panel E: Not exporting	-0.045	1.249	-0.951	7.503	1.900
	(1.079)	(24.559)	(1.768)	(6.666)	(3.181)
Observations	3,975	3,975	3,975	3,975	3,975
Panel F: Exporting	-0.961	-17.334	8.020***	72.303***	10.400*
	(1.729)	(31.595)	(1.626)	(12.073)	(5.937)
Observations	2,807	2,807	2,807	2,807	2,807
Firm fixed effects	yes	yes	yes	yes	yes
Country x Year dummies	yes	yes	yes	yes	yes
Industry x Year dummies	yes	yes	yes	yes	yes
Level of se clustering	country	country	country	country	country
Firm-level controls	yes	yes	yes	yes	yes
Country-level controls	no	no	no	no	no

Estimations using the within estimator, with firm fixed effects, industry x year dummies, country x year dummies, and robust clustered standard errors at the country level. All estimations include firm-level control variables. ***p<0.01, **p<0.05, *p<0.1.

7 Conclusion

Given the narrow access to international capital markets of low- and lower-middle-income countries, foreign aid remains one of their main sources of development finance. Yet, aid effectiveness has been harshly debated, and its adverse effects, notably the Dutch disease and the institution curse, highlighted in the literature. Using a disaggregated outcome measured at the firm level in 29 developing countries, we find that aid has a substantial positive effect on firm performance, the magnitude of which is in line with recent studies at the aggregate level finding a positive effect of aid: a 10% increase in aid could increase firm growth by around five to seven percent.

The macroeconomic literature on the impact of aid has been plagued by endogeneity issues. We show that assessing the effect of aid on disaggregated outcomes such as firm growth has one main methodological advantage: it largely dampens the endogeneity bias. From an analytical point of view, working on disaggregated outcomes also allows us to explore the mechanisms behind the impact. We find evidence that the positive effect of aid is not driven by specific firm characteristics, and that it mainly goes through relaxing the financing and infrastructure constraints.

Our approach contributes to understanding the so-called micro-macro paradox which refers to the fact that aid is found to be effective at the micro level, but not at the macro level (Mosley, 1986). We find that aggregate aid is effective in spurring growth at the firm level. Weak evidence at the macro level may then stem from two factors. First, as evidenced in our article, there is a quite large downward endogeneity bias in the estimation of the effect of aid on aggregate outcomes. Second, the positive effect of aid on manufacturing firms could be compensated for by adverse effects either on firms in the informal sector or on the agriculture sector. There is still much to answer regarding the economic impact of aid, and further research is needed to fully understand the mechanisms at play.

Our approach could be extended towards other promising avenues of research. First, despite the great expectations of the international community, there is still little evidence of the catalytic role that aid may play with respect to foreign investment. Firm-level analysis of whether aid contributes to attracting foreign investment by enhancing the infrastructure and business environment may provide substantial policy advice. Second, the sectoral priorities of aid could also be assessed using disaggregated outcomes. New datasets have been compiled which geo-localize parts of aid projects and could be combined with geo-localized outcomes. This approach could help further disentangle the micro-macro paradox in aid effectiveness.

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Table 14: Appendix 1. List of countries, survey years, and number of observations.

Latin America			Africa			Asia		
Country	Survey	N	Country	Survey	N	Country	Survey	N
Argentina	(2006, 2010)	726	Burkina Faso	(2006, 2009)	144	Bangladesh	(2007, 2011)	416
Bolivia	(2006, 2010)	144	Botswana	(2006, 2010)	180	Pakistan	(2002, 2007)	688
Brazil	(2003, 2009)	782	Cameroon	(2006, 2009)	136			
Chile	(2006, 2010)	628	Cape Verde	(2006, 2009)	100			
Colombia	(2006, 2010)	486	Mali	(2007, 2010)	218			
Ecuador	(2006, 2010)	222	Malawi	(2005, 2009)	120			
El Salvador	(2006, 2010)	136	Morocco	(2004, 2007)	500			
Guatemala	(2006, 2010)	178	Niger	(2005, 2009)	62			
Honduras	(2003, 2006)	378	Senegal	(2003, 2007)	108			
Mexico	(2006, 2010)	312	South Africa	(2003, 2007)	296			
Nicaragua	(2003, 2006)	432	Zambia	(2002, 2007)	94			
Panama	(2006, 2010)	88						
Peru	(2006, 2010)	508						
Paraguay	(2006, 2010)	160						
Uruguay	(2006, 2010)	334						
Venezuela	(2006, 2010)	108						
Total	5,622 (64.7%)		1,958 (22.6%)			1,104 (12.7%)		

N refers to the number of observations in the panel sample.

Table 15: Appendix 2. Stability of the results when each country is omitted.

	(1) OLS		(2) FE	
Full sample	1.388***	(0.448)	1.980**	(0.721)
Observations	25673		8684	
ARG	1.411***	(0.455)	2.108**	(0.804)
Observations	24021		7958	
BOL	2.225***	(0.507)	2.675***	(0.870)
Observations	25107		8540	
BRA	1.399***	(0.447)	2.007**	(0.768)
Observations	23030		7902	
CHL	1.421***	(0.448)	2.151**	(0.888)
Observations	24046		8056	
COL	1.294***	(0.452)	1.822**	(0.703)
Observations	24242		8198	
ECU	1.443***	(0.448)	1.994**	(0.732)
Observations	24921		8462	
GTM	1.454***	(0.446)	1.982***	(0.702)
Observations	24845		8506	
HND	1.202***	(0.453)	1.835**	(0.677)
Observations	24891		8306	
MEX	1.186***	(0.449)	1.738**	(0.832)
Observations	23287		8372	
NIC	1.422***	(0.472)	2.285***	(0.740)
Observations	24821		8252	
PAN	1.395***	(0.447)	1.972**	(0.726)
Observations	25221		8596	
PER	1.301***	(0.449)	1.932***	(0.659)
Observations	24300		8176	
PRY	0.924**	(0.457)	1.909**	(0.782)
Observations	25064		8524	
SLV	1.242***	(0.453)	1.842**	(0.755)
Observations	24882		8548	
URY	1.427***	(0.453)	1.827**	(0.798)
Observations	24871		8350	
VEN	1.356***	(0.448)	1.853**	(0.737)
Observations	25422		8576	
BGD	1.433***	(0.448)	1.983**	(0.722)
Observations	24291		8268	
PAK	1.804***	(0.468)	2.413***	(0.860)
Observations	24935		7996	

Table 15: Appendix 2. continued.

	(1) OLS		(2) FE	
Full sample	1.388***	(0.448)	1.980**	(0.721)
Observations	25673		8684	
BFA	1.562***	(0.447)	2.349***	(0.708)
Observations	25225		8540	
BWA	1.411***	(0.488)	2.012**	(0.920)
Observations	25209		8504	
CMR	1.034**	(0.467)	1.529**	(0.652)
Observations	25407		8548	
CPV	1.297***	(0.463)	1.902**	(0.792)
Observations	25476		8584	
MAR	1.610***	(0.446)	2.222***	(0.558)
Observations	24528		8184	
MLI	1.460***	(0.451)	1.968**	(0.772)
Observations	25076		8466	
MWI	1.263**	(0.524)	1.669*	(0.876)
Observations	25416		8564	
NER	1.247**	(0.524)	1.641*	(0.871)
Observations	25517		8622	
SEN	1.449***	(0.450)	1.984**	(0.789)
Observations	25232		8576	
ZAF	1.145**	(0.449)	1.802**	(0.669)
Observations	24423		8388	
ZMB	1.803***	(0.468)	2.383***	(0.846)
Observations	25138		8590	

Column 1 is estimated using the OLS estimator with country dummies, industry x year dummies and robust clustered standard errors at the firm level. Column 2 is estimated using the within estimator, with firm fixed effects, industry x year dummies and robust clustered standard errors at the country level. All estimations include country and firm-level control variables. ***p<0.01, **p<0.05, *p<0.1.

Table 16: Appendix 3. Mean-comparison tests, by period.

	Group 1 Firms which do not survive	Group 2 Firms which survive	Difference	p-value
$\text{GROWTH}_{i,k,j,(t,t-3)}$	10.45	10.85	-0.395	0.514
Observations	8,910	4,342		
	Group 1 Firms which appear in period 2	Group 2 Firms which survive	Difference	p-value
$\text{GROWTH}_{i,k,j,(t,t-3)}$	5.44	5.68	-0.239	0.712
Observations	8,079	4,342		

In the upper part of the table, firms in groups 1 and 2 are compared for period one only. In the lower part of the table, firms are compared in period 2 only.

Table 17: Appendix 4. Alternative measures of industry intensities.

Industry	$EXPORT_k$	$INSTITUTION_k$	$FINANCE_k$	$ELECTRICITY_k$			$TRANSPORT_k$		
				Lat.Am.	Africa	Asia	Lat.Am.	Africa	Asia
Food	0.000	-0.964	-0.140	0.030	0.027	0.041	0.197	0.250	0.094
Garments	1.000	-0.974	-0.306	0.021	0.028	0.027	0.282	0.482	0.286
Textile and leather	1.000	-0.968	0.010	0.031	0.041	0.041	0.367	0.509	0.267
Machinery and equipment	0.000	-0.953	-0.069	0.019	0.025	0.046	0.317	0.287	0.235
Chemicals	0.000	-0.962	0.284	0.026	0.024	0.023	0.469	0.529	0.457
Electronics	0.000	-0.956	0.286	0.016	0.017	0.029	0.375	0.463	0.380
Non-metallic minerals	0.000	-0.958	-0.258	0.033	0.027	0.096	0.189	0.304	0.000
Other manufacturing	0.000	-0.965	-0.146	0.026	0.021	0.035	0.260	0.359	0.224

Table 18: Appendix 5. Channels depending on the characteristics of the firms.

Intensity in:	$EXPORT_k$	$INSTITUTION_k$	$FINANCE_k$	$ELECTRICITY_k$	$TRANSPORT_k$
	(1)	(2)	(3)	(4)	(5)
Panel A: Under 100 employees	1.544**	-1.388	1.914	75.061**	7.210*
	(0.729)	(46.860)	(1.167)	(36.470)	(3.712)
Observations	4,817	4,817	4,817	4,817	4,817
			p=0.113		
Panel B: More than 20 employees	0.215	107.652**	3.945*	79.473	5.944
	(1.329)	(45.787)	(2.144)	(100.892)	(5.363)
Observations	4,589	4,589	4,589	4,589	4,589
Panel C: Not owned by the state	1.379	0.420	2.821**	94.449**	8.414***
	(0.816)	(47.568)	(1.083)	(44.174)	(3.015)
Observations	6,734	6,734	6,734	6,734	6,734
Panel D: Not foreign-owned	2.076**	-41.411	2.699**	124.023**	9.045**
	(0.939)	(40.262)	(1.306)	(54.286)	(4.365)
Observations	6,021	6,021	6,021	6,021	6,021
Panel E: Not exporting firms	1.656**	-64.326	-0.882	-61.964	6.641*
	(0.700)	(95.305)	(1.764)	(49.446)	(3.574)
Observations	3,975	3,975	3,975	3,975	3,975
Panel F: Exporting firms	1.117	33.691	9.074***	317.6**	12.2**
	(1.373)	(78.757)	(2.329)	(144.658)	(5.052)
Observations	2,807	2,807	2,807	2,807	2,807
Firm fixed effects	yes	yes	yes	yes	yes
Country x Year dummies	yes	yes	yes	yes	yes
Industry x Year dummies	yes	yes	yes	yes	yes
Level of se clustering	country	country	country	country	country
Firm-level controls	yes	yes	yes	yes	yes
Country-level controls	no	no	no	no	no

Estimations using the within estimator, with firm fixed effects, industry x year dummies, country x year dummies, and robust clustered standard errors at the country level. All estimations include firm-level control variables. ***p<0.01, **p<0.05, *p<0.1.

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