Do capital grants improve microenterprise productivity?*

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Abstract

Do capital grants improve microenterprise productivity? We use the lens of a production function to re-examine two previous randomised controlled trials that allocated capital to microenterprises. We find that productivity is higher for treated firms, and accounts for about 20-30 percent of the revenue effects of capital grants. Although long-run estimates are noisy, point estimates indicate that these productivity effects are sustained six years after the grants. We explore possible mechanisms for this finding, and show that treatment tilts the asset composition towards durables with a higher technology component: a result consistent with a important role for capital-embodied technology. Mediation analysis confirms that virtually all of the effect of treatment on productivity can be explained by the adoption of higher-technology durables.

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1 Introduction

A large fraction of firms in developing countries are microenterprises with very low productivity. While these firms are an important source of income to their owners, they can drag down aggregate productivity and growth (Hsieh and Klenow, 2009). Making microenterprises more productive and competitive is therefore a key element of many policies that promote private sector development. However, this has turned out to be a major challenge (Bruhn, Karlan, and Schoar, 2018; Atkin, Khandelwal, and Osman, 2017; McKenzie and Woodruff, 2014). Other interventions that take a different angle – easing capital constraints – can have large and lasting effects on revenues and profits of microenterprises. However, little is known about the channels by which such effects occur. Do capital constraints only restrict capital, or do they also hold back productivity?

In this paper, we use the lens of a production function to look at the alleviation of capital constraints to microenterprises. This enables us to study directly how capital grants affect microenterprise productivity – a relationship that is not directly observable in survey data. In doing so, we conduct a secondary analysis of data from two related randomised control trials of capital grants to microenterprises: de Mel, McKenzie, and Woodruff (2008) in Sri Lanka (DMW henceforth) and Fafchamps, McKenzie, Quinn, and Woodruff (2014) in Ghana (FMQW henceforth). The experimental setup, combined with our estimate of total factor productivity (TFP), allows us to structurally disentangle the channels through which alleviating capital constraints increases revenues and profits. We estimate microenterprise production functions as well as TFP using the standard methods in the literature: a linear panel estimator (Blundell and Bond, 1998) and an estimator exploiting the firm's first-order condition (Gandhi, Navarro, and Rivers, 2020).

We find that the effects of capital grants cannot be fully rationalised either by adjustments of capital, intermediate inputs, or other production factors alone. Capital grants also have a sizable and significant effect on TFP, in particular by shifting TFP outward at the top of the distribution. They increase TFP of the median firm by up to seven percent; and by about seven to nine percent at the 80th percentile. We use the structure of the production function to perform a decomposition of treatment effects into factor adjustments and productivity. Up to 37 percent of the increase in revenue caused by capital grants can be attributed directly to an increase in productivity in Sri Lanka, and up to 34 percent in Ghana – over and above adjustments of production factors. For Sri Lanka, where follow-up data are available to us for up to six years after the treatment, long-run point estimates – even though very noisy – suggest that productivity increases are sustained in the long term, putting firms on a different growth path.

Building on this first result, we examine the mechanisms through which capital grants affect TFP. One plausible mechanism is that treatment introduced advanced equipment and thus more efficient means of production to the firm. We exploit the richness of the asset data collected by DMW in Sri Lanka to test for this mechanism. We find that treated firms invest their grants unevenly: they particularly acquire assets that are not essential to the core activities of a business, but rather assets that can be used to run such activities more efficiently. Assets acquired by treated firms also have a relatively higher technology component. In contrast, treatment does not increase ownership or value of capital that most firms already used at baseline – such as tools, machinery and furniture – and has only a small effect on low-technology assets. Beyond a short-lived initial hoarding of inventories immediately upon receiving the grants, treatment also does not sustainably affect the stock of materials and goods held by firms. The change in the asset composition further changes the way firms do business. Treated businesses expand their customer base, and reach wider market segments through new and different products, facilitated by the acquisition of assets to produce or handle those products.

Finally, we formally test whether the adoption of different types of capital is the mechanism that explains the productivity effects of capital grants. We perform a formal mediation analysis and estimate the 'Average Controlled Direct Effect' (ACDE) proposed by Acharya, Blackwell, and Sen (2016). We find that close to 100% of the treatment effect on productivity is driven by the tilt in the capital composition towards assets with a higher technology component, and assets that were less essential to core business activities at baseline. This suggests that the increase in overall productivity due to capital grants is embodied in certain types of capital that firms adopted.

Our paper contributes in two ways to understanding of the productive structure of microenterprises. First, to our knowledge, this is the first paper to consider and test the hypothesis that an increase in capital can enhance microenterprise productivity; our resulting estimates are therefore the first quantification of this channel for microenterprise growth. A large literature has documented that low-productivity, mostly informal, microenterprises dominate this firm size distribution in developing countries, with adverse consequences for aggregate productivity (Hsieh and Klenow, 2009). It has proved difficult, in practice, either to reallocate economic activities out of this sector (Koelle, 2019; Ulyssea, 2018; La Porta and Shleifer, 2014; de Andrade, Bruhn, and McKenzie, 2014), or to improve directly the productivity of microenterprises (Bruhn et al., 2018; Atkin et al., 2017; Karlan, Knight, and Udry, 2015; McKenzie and Woodruff, 2014).

We show that capital grants – a policy not targeted at or thought to improve productivity – can have such an effect, if they succeed in introducing more advanced and productive capital equipment to firms. In order to show this, and because productivity cannot be measured directly in the data, we apply standard methods for productivity estimation (Blundell and Bond, 1998; Bloom, Brynjolfsson, Foster, Jarmin, Patnaik, Saporta-Eksten, and Van Reenen, 2019; Gandhi et al., 2020) which have previously only been applied to large firms with detailed and sophisticated accounting practices and financial records.[1] We show that, with high-quality panel data, standard production function estimators can be usefully applied to informal microenterprises, given the consistency across all tested estimators. To implement these production function estimators in a standard way, we pre-partial all elements of the production function to remove time, fixed and treatment effects. We then estimate TFP using the recovered production function coefficient and regress it on a treatment dummy. We provide a formal proof that this three-step method, which is very easy and intuitive to implement, provides consistent estimates of both production function coefficients and treatment effects as long as the standard assumptions hold. This enables us to test how an intervention affects microenterprise productivity.

Second, we show that capital-embodied technology is a key mechanism behind the productivity increase that we document. The idea of capital-embodied technology dates to the early models of capital vintage by Johansen (1959) and Solow (1959). Griliches (1979) demonstrates the specific process of rent spillover, in which firms purchasing capital goods with embodied technology accrue some of the economic rent of this technology, if the supplier cannot perfectly price discriminate and the value of the technology is therefore not fully reflected in the price of the capital good. This channel has been shown to explain significant differences in cross-country productivity levels in agriculture (Caunedo and Keller, 2020), but has received almost no attention in the literature on microenterprises – or, indeed, in the applied microeconomic literature on firms.[2]

Our evidence for this mechanism comes from a set of firms where the production technology and the capital stock are very simple – and therefore transparent and easy to understand. We observe the name and value of each individual capital asset, allowing us to distinguish between assets of different technology content and functional role in the firm. Our results suggest that, even among some of the smallest firms in developing countries, differences in sales and productivity are at least partly driven by differences in basic technology adoption. More generally, our findings resonate with a wider literature on adoption of new technology and business practices (Atkin et al., 2017; Karlan et al., 2015; Bloom, Eifert, Mahajan, McKenzie, and Roberts, 2013; Conley and Udry, 2010).

Together, our results have several key implications for understanding microenterprises

[1] The only exceptions we are aware of are Atkin et al. (2017), Atkin, Khandelwal, and Osman (2019) and Keniston (2011), who estimate microenterprise production functions using control function methods (Levinsohn and Petrin, 2003; Ackerberg, Caves, and Frazer, 2015).

[2] Several earlier qualitative studies report that owners of small firms identify technology as an important constraint of productivity and expansion (Aftab and Rahim, 1989; Kabecha, 1998). By contrast, access to better intermediate inputs in production has been recognised in the economic literature as a channel for productivity gains from trade (Amiti and Konings, 2007; Halpern, Koren, and Szeidl, 2015). See also Kabir and Mansouri (2021), finding that relaxing of financing constraints increases the quality of physical capital and, through that, firm productivity.

in low-income settings. If capital grants impact such enterprises exclusively through a capital channel, this tends to imply (i) that there are diminishing marginal returns to scale in the provision of such grants (implied directly by the diminishing marginal return to capital in standard firm production functions), and (ii) that capital grants are likely to have transitory impacts (with recipient microenterprises converging back to their steady state, as in the model in FMQW). Further, this would tend to imply (iii) that there should be a sharp distinction between policies designed to encourage technological upgrading in small firms (including, for example, through mentoring or management training) and policies designed to support capital accumulation (such as grants and loans). In contrast, our results – that capital transfers facilitate a TFP effect, through upgrading to higher-technology durables – challenge each of these implications. Specifically, our results imply (i) that there is likely to be a local convexity in returns to capital transfers (in the sense that lumpy grants, of the kind studied in Sri Lanka and in Ghana, facilitate a discrete shift in capital type) and (ii) the effects of lumpy capital grants are likely to be persistent (de Mel, McKenzie, and Woodruff, 2012). In policy terms, the result implies (iii) that policy should think about capital transfers – whether through grants or through loans – as itself encouraging technological upgrading and growth in small firms. We expand upon these implications in the conclusion to this paper.

Our paper proceeds as follows. Section 2 describes the experiments and data. We outline our identification strategy for TFP estimates in section 3, and present results on productivity in section 4. Section 5 provides evidence on mechanisms, and section 6 concludes.

2 Data and Experiments

We conduct our analysis using the experimental sample and survey data from two randomised control trials that allocated cash and in-kind grants to microenterprises in Sri Lanka (DMW) and in Ghana (FMQW).[3]

The Sri Lanka Microenterprise Survey was collected for the seminal work of de Mel et al. (2008). It spans a representative sample of 383 microenterprises, with a capital stock of less than 100,000 LKR (about $1000), in the manufacturing, retail and service sectors. Firms with a capital stock up to this value were chosen to ensure that the grant would represent a significant shock relative to their existing stock. The sample was taken in three districts, which were chosen for a high share of own-account workers and modest education levels. Numerous firms in the baseline survey were affected by the 2004 tsunami and were subsequently excluded from the sample. About 30% of the sample are engaged in artisanal food and clothing manufacturing, another 30% are retail shops,

[3] We summarise the data and experiments briefly, and refer the reader to de Mel et al. (2008) and Fafchamps et al. (2014) for further details. Datasets are available as de Mel, McKenzie, and Woodruff (2013) and McKenzie, Fafchamps, Quinn, and Woodruff (2015).

15% work in services (mostly repairs) and the remainder are engaged in a variety of specialised trade and manufacturing activities. Owners are self-employed and have no paid employees. We mainly use the first nine waves of the data; these are equally spaced, three months apart. The first wave started in April 2005.

After the first wave, half of the eligible firms were randomly assigned a cash or in-kind grant of either LKR 10,000 or LKR 20,000. The smaller LKR 10,000 grants correspond to around three months of median profits and around 55% of the median capital stock in the base period. In the baseline survey, firm owners were asked about which item would increase profits the most (independent of cost). These average LKR 25,000 and 43% were below LKR20,000 indicating that the treatment amounts were economically significant. A total of 124 firms received treatment after wave 1, and another 104 after wave 3. The probability of treatment was equal in each district. The grants were framed as a random prize draw to compensate for participation in the survey, and were only announced to firms in the wave in which it was received. The in-kind grants were purchased by the enumerators according to the free choice of the firm owners and could be spent on either or both of inventory and fixed assets. Only a few firms spent less than the treatment amount, while two-thirds of owners contributed (mostly trivial) additional funds to the purchase. Approximately 57% of the goods purchased were inventories or raw materials, 39% machinery or equipment, and 4% were construction materials for buildings. Cash grants were explicitly given without restrictions and enumerators noted that owners could purchase anything they want. Approximately 58% of grants were invested in the firm, while 12% was saved and the remainder used on loan repayments, household expenditures, house repairs and other items. Relevant for the issue of technological upgrading, even the cash grants were used to purchase new materials or equipment, suggesting that owners expected positive returns to these items. On average, about 40% and 17% of the cash grants were spent on the purchase of inventories and equipment respectively.

The Ghana Microenterprise Survey was collected for the work of Fafchamps et al. (2014). FMQW surveyed 793 microenterprises (479 with female owners and 214 with male), without paid employees or a motorised vehicle, in Accra and the neighbouring port town of Tema. These firms operate in similar sectors as those in Sri Lanka and were small enough so that the treatment would be economically significant. About 40% are traders, about a third are engaged in artisanal food an clothing manufacturing, and the remainder work in service occupations such as repairs or beauty salons. A significant difference in the Ghana is the much higher labour-force participation rate of women. As in Sri Lanka, survey waves were conducted every three months. The first wave started in November 2008, and the survey lasted for six waves.[4]

The experimental design in FMQW mostly replicated that of Sri Lanka; however, the

[4] The authors also collected a later long-term follow-up wave, which we do not use.

design used a more detailed stratification, to improve power and balance over simple randomisation. The sample is stratified by sector, gender, baseline capital stock, and a binary variable measuring potential capture of cash or firm profits by family members. Within each strata, four firms with similar firm profits were grouped. Within each quadruplet, two firms were allocated grants, and the other two remained control firms. Capital grants were randomly allocated after the second and the third wave; and for a small group, after the fourth wave. Grants were again framed as randomly drawn prices to compensate for participation in the survey. Two treatment groups of 198 firms each received cash and in-kind grants respectively, leaving a control group of 396 firms. The grant size was GHC 150, or about $120 – and, and unlike in Sri Lanka, there was no variation in the grant size. The grants are comparable in size to the smaller grants in Sri Lanka. They amount to two months of median baseline profits (median baseline profits were 68 GHC). The grant size was small enough that both purchased inventories and equipment could be liquidated easily. Since the firms in Ghana are less capital-intensive than in Sri Lanka, grants constituted a relatively larger shock to the capital stock, and almost doubled median baseline capital of GHC 170. The majority of in-kind grants were chosen in the form of inventories and materials. Only 24% of participants chose to buy physical equipment (including sewing machines, hair dryers, and carpentry tools).

Several features of the data make them particularly suitable to estimate the effects of capital grants on productivity, and to test the mechanism of capital-embedded productivity growth in a micro setting. First, the details of the production process, the nature of capital, and the boundaries of the firm are well understood. In comparison to large, often transnational enterprises in advanced economies, the difficulties arising from multi-product and multi-establishment firms, the role of intangible capital or strategic accounting practices, and price mark-ups created by product market power, are much reduced (Atkin et al., 2019). Second, both surveys advanced the measurement of business concepts for microenterprises, which were thought to be very challenging to enumerate given the absence of formal accounting systems or often even written records (De Mel, McKenzie, and Woodruff, 2009). Further, in the later of the two experiments – in Ghana – data was refined further through automated data consistency checks (Fafchamps, McKenzie, Quinn, and Woodruff, 2012). We use self-reported headline profits and sales, which give the most accurate measurement (De Mel et al., 2009). Capital is directly reported item-by-item at baseline, and additions, improvements, damages and sales are recorded at each follow-up wave. Unlike many empirical studies of large firms, imputation of capital is therefore not required. Third, the coverage of inputs and outputs (capital, labour, intermediate goods stocks and flows, sales, and profits) is comprehensive. The rate of missing data on inputs is low. Most frequently missing is capital, for

7% of firms in each wave in Sri Lanka and 10% in Ghana, on average.[5] This compares favourably with ORBIS and similar databases on large enterprises in developed countries.[6] Fourth, the survey instruments as well as the main experimental design are very similar across the two contexts; allowing us to test our hypotheses in two very different yet comparable contexts. We discuss further details on the construction of variables for our analysis in Appendix A.

3 Microenterprise production functions

3.1 Methods for estimating production functions

The first step of our analysis consists of estimating a production function for microenterprises. We define TFP – as is very standard in empirical literature – as the residual from a Cobb-Douglas production function. In this section, we discuss the two methods that we use for estimating such production functions in the context of microenterprises, and our preferred method for recovering treatment effects on TFP. We keep this discussion at a general level, but provide a more technical review in Appendix B.

We postulate a standard Cobb-Douglas production function of the form:

$$Y_{it} = A_{it} \cdot K_{it}^{\beta_k} \cdot L_{it}^{\beta_l} \cdot M_{it}^{\beta_m}, \tag{1}$$

where output Y_{it} of firm i in period t is determined by capital (K_{it}), labour (L_{it}) and materials (M_{it}); A_{it} is a Hicks-neutral technology term. Empirically, we know that firms in both experiments used a substantial share of their grants for the purchase of material inputs; in order to capture this fact in our analysis, we specify Y_{it} in terms of gross output.[7] Further, we specify Y_{it} in revenue terms.[8] In our setting, time t corresponds to a

[5] Appendix Table A.1 tests for differential attrition as well as for differential non-response on the production function variables (output and inputs). Besides a standard test for differential attrition by treatment status, we additionally test whether attrition differs along the firm productivity distribution. For example, high productivity firm might be less likely to drop out, which could lead us to overstate the true treatment effect. Our results indicate that overall, non-response and attrition do not systematically relate to treatment status and firm productivity. However, there is some weak evidence (marginally significant and quantitatively small) that in Ghana treated firms were slightly less likely to have missing data. Because of this, we perform a Lee (2009) bounding exercise as part of our robustness checks (Appendix Table A.22).

[6] See, for example, Table 9 in Maffini and Mokkas (2011), discussing missing data problems in ORBIS.

[7] The alternative would be to denote Y_{it} as value added. In a value-added production function, the contribution of the intermediate inputs is netted out and the production of value added is specified in terms of capital and labour only. This transformation can be theoretically justified in the special case where the production function is Leontieff in materials (**?**); however, we do not view that as a reasonable restriction for this context.

[8] That is, we estimate TFPR rather than TFPQ. As Atkin et al. (2019) explain, "if a firm's capabilities come from its ability to produce both quality and quantity, TFPR may be closer to the object of interest even though it confounds forces unrelated to productivity."

survey wave.

We then take logs (which we denote in lower case) and write out our assumptions on the components that make up TFP. Part of these TFP components capture the fact that our empirical setting encompasses firms in different sectors s and countries c:

$$y_{isct} = \rho \cdot y_{it-1} + \beta_k \cdot k_{it} + \beta_l \cdot l_{it} + \beta_m \cdot m_{it} + \beta_T \cdot T_{it} + \mu_{sc} + \gamma_{st} + \omega_{it} + v_{it}, \quad (2)$$

where we define $\log(TFP)$ as:

$$\log(A_{it}) \equiv \beta_T \cdot T_{it} + \mu_{sc} + \gamma_{st} + \omega_{it} + v_{it}. \quad (3)$$

In this way, we allow TFP to depend upon both observed and unobserved productivity shifters. Observed productivity shifters are treatment (through T_{it}, a treatment indicator that turns one after a microenterprise has received a grant), as well as sector-country and country-time fixed effects. These capture, for example, permanent differences in productivity or its measurement across sectors or countries; as well as possible effects of business cycles or seasonality on productivity.

We further allow TFP to depend upon two different types of unobserved productivity shifters: (i) ω_{it}, a time-variant, firm-specific shock that, we assume, follows a first-order Markov process; and (ii) v_{it}, a firm-specific measurement error. With the exception of the treatment dummy, this is a very standard specification in the empirical analysis of firm production functions (see, for example, Eberhardt and Helmers (2010); Gandhi et al. (2020)).[9]

The main challenge for identification of the parameters β_k, β_l and β_m is the fact that firms choose inputs as a function of their firm-specific productivity shocks ω_{it}, which are unobservable to the researcher. This endogeneity is conventionally referred to as 'transmission bias' (see, for example, Gandhi et al. (2020)). Two standard approaches to overcome transmission bias in gross output production functions are (i) to estimate the production function from equation 2 in a dynamic linear panel framework and (ii) to exploit the first-order condition implied by the firm's optimisation problem. We use both approaches in this paper.[10]

[9] The dynamic linear panel approach discussed below – but not the control function methods – additionally accommodates firm-level fixed effects by applying first-differencing to the the data. Gandhi et al. (2020)'s preferred implementation of their estimator, which we follow here, does not include firm fixed effects.

[10] A third approach is a class of estimators that introduce a control function term into equation 2: most commonly, a lagged polynomial of flexible inputs and capital. The resulting GMM moment conditions are then implied by structural assumptions about input choices (Olley and Pakes, 1996; Levinsohn and Petrin, 2003; Wooldridge, 2009), which allows to invert the production function with respect to unobserved productivity. However, Gandhi et al. (2020) note that applying these methods to gross output requires additional sources of variation in the demand for flexible inputs (e.g., prices). Without this, flexible inputs (materials, electricity, etc.) are not adequately identified in structural estimators, because the invertibility assumption may not hold. We do not have price data available in our context, so we do not apply this approach in this paper.

Dynamic linear panel methods exploit lags of output and input variables as instruments for endogenous inputs in a GMM framework. The main assumption of this class of estimator is that suitably lagged past input choices are independent of ω_{it}, but informative of current input choices due to adjustment costs, factor constraints, and other dynamic channels (Arellano and Bond, 1991; Blundell and Bond, 1998). As in the standard linear panel estimation of production functions, we begin by taking first differences, to remove firm fixed effects (see, for example, Blundell and Bond (2000)). It is worth noting that such estimators do not demand any particular assumption about firm optimisation (beyond the assumption that the firm faces adjustment costs or other optimisation frictions: Bond and Söderbom (2005); Gandhi et al. (2020); Shenoy (2021)).[11]

Gandhi et al. (2020) (hereafter GNR) develop an alternative empirical strategy that, relying on the first order conditions of the firm, non-parametrically identifies the flexible input elasticity. This solves for the missing source of identification for the production function within a proxy variable structure. Crucially, the GNR method requires that firms choose optimally their level of intermediate inputs (Shenoy, 2021). One might doubt this assumption on conceptual grounds – and especially in context of microenterprises receiving cash or in-kind grants. In particular, if microenterprises value intermediates as a store of wealth (in addition to their productive value) – and/or if microenterprises respond to treatment initially by 'hoarding' intermediates – then the GNR approach will overestimate the elasticity of output with respect to such intermediates. In our context, we do observe that (i) microenterprises have very high materials shares (above 100% of measured revenue for 23% of Sri Lankan firms and 30% of Ghanaian firms), and (ii) as noted earlier, we observe a short-term 'hoarding' of intermediate inputs in response to treatment. For this reason, the dynamic linear panel method is our preferred approach in this microenterprise context. Nonetheless, we apply the GNR estimator, using our Cobb-Douglas specification, as a robustness check. To foreshadow our results, both approaches give broadly similar estimates of the productivity effects of capital grants (and particularly in Ghana).

In principle – and with a sufficiently large dataset – one could recover the coefficient on the treatment term in equation 2 as the estimate of the treatment effect on productivity. However, in practice, we find that the collinearity between T_{it} and the lagged output term generates problems with precision using this approach, given our sample sizes. We therefore use a three-step approach. In a first step, we partial out observable time and fixed effects within the production function to ensure the remaining error term maintains a Markov structure. In the second step, we estimate the production function coefficients β_k, β_l and β_m. In the third step, we obtain estimates of the effect of treatment on TFP. We discuss the first two steps in Online Appendix B, and we discuss the third

[11] So, for example, if the experimental treatments augment capital by easing a credit constraint, this does not pose any threat to our identification strategy.

step in section 4.

In the context of experimental data from microenterprises in developing countries, we make a number of additional small technical adjustments. First, for power reasons, we pool data across all industries; but we estimate equation A.7 separately by country.[12] Second, to reduce the influence of outliers that are due to measurement error, we winsorize each input at the top and bottom 1%. Third, we restrict the sample to firms with strictly positive amounts of all inputs, including capital. Fourth, we deflate monetary values with the CPI in each country.

3.2 Production function estimates

We present the main estimates for gross output production functions of microenterprises in Table 1. We estimate separately for Sri Lanka and Ghana. In columns 1 and 3, we report the estimates from the Blundell and Bond (1998) estimator, in which lagged variables serve as instruments for endogenous inputs in both levels and difference equations. The dynamic nature of productivity leads to the inclusion of the lagged dependent variable in the estimating equation. Various specification tests are informative about how to specify the lag structure, as well as to which degree lagged inputs are relevant instruments. Appendix B discusses these in more detail. In columns 2 and 4, we report results from the estimator developed by GNR,[13] which estimates the flexible input elasticity in a first stage and subsequently the coefficients on labour and capital.

For Sri Lanka (column 1), we estimate a coefficient on capital β_k of 0.12, a labour coefficient β_l of 0.13, and a materials coefficient β_m of 0.42. For Ghana (column 4), we estimate a capital coefficient of 0.23, a labour coefficient of 0.20, and a materials coefficient of 0.39. We note that, in both columns 1 and 3, the estimated models comfortably pass the relevant specification tests: the Hansen (1982) test of over-identifying restrictions, and the Windmeijer (2024) test of instrument informativeness. The inclusion of lagged output, and the partialling out of most elements of TFP discussed previously, addresses autocorellation in the model as confirmed by the respective Arellano and Bond (1991) test. This gives us confidence that the two step procedure ensures that ω follows a first order Markov process as required. The GNR results in columns 2 and 4 are broadly similar to the results in columns 1 and 3. In Ghana, the estimates are almost identical. In Sri Lanka, the GNR estimates are broadly similar, but imply substantially higher returns to capital and to intermediates than do the linear panel estimates. For the reasons noted earlier, we prefer the Blundell and Bond (1998) estimates for our context.

[12] Note that, when we allow the production function coefficients to differ by sector – in column 1 of Appendix Table A.3 and column 1 of Appendix Table A.6 – we do not find large or significant differences.
[13] We use Stata code provided by the authors for this.

3.3 Robustness of production function estimates

Before we turn to the TFP analysis, we summarise a comprehensive set of tests of the robustness of our results. This includes (i) utilising all classes of production function estimators suitable for gross ouput functions; (ii) testing for internal consistency in production functions across various sub-samples (industry and treatment status); and (iii) assessing the external validity of the results across the two samples and against common results of production functions in the literature, including formal firms.

First, we already note that the production function estimates obtained from the GNR approach are broadly similar to those obtained using linear panel methods – and almost identical in Ghana. Further, in Appendix Tables A.2 and A.5, we report an extensive set of alternative specifications (OLS estimates, fixed effect estimates, dynamic panel estimates with alternative instruments, Wooldridge (2009) control function estimates, and Ackerberg et al. (2015) estimates). In general, our results remain remarkably stable across these alternative specifications. This provides reassurance that our preferred estimates are reasonable, in the sense that they do not change drastically with different specifications or estimators.

Second, turning to internal validity, in Appendix Tables A.3 and A.6, we show that it is reasonable to pool data from treatment and control firms; this rules out an alternative explanation of our results, in which the treatment serves somehow to shift the production function parameters, rather than acting through a TFP channel. Similarly, in Appendix Tables A.4 and A.7, we show that it is reasonable to pool production functions from different industries – in particular, between traders and non-traders.

Third, considering external validity, we note that the parameters are remarkably similar between Ghana and Sri Lanka.[14] In this sense, our results speak to the issue of external validity and generalisability across experimental sites. They suggest that the similarity in reduced-form results between DMW and FMQW owes much to a deeper structural similarity in microenterprise production functions across contexts. Second, our estimates are broadly similar to production function estimates for larger establishments in developing countries. Specifically, we consider estimates for medium to large plants in Chile (Pavcnik, 2002; Gandhi et al., 2020), Colombia (Gandhi et al., 2020) and Ghana (Söderbom and Teal, 2004). We obtain similar coefficient magnitudes as for those larger firms.

[14] When we run a cross-equation test of whether these production functions are the same in Sri Lanka as in Ghana, this comfortably passes for our linear panel estimator ($p = 0.55$).

4 The productivity effects of capital grants

4.1 Do capital grants affect total factor productivity?

We now turn to the question of whether capital grants are productivity-enhancing. To estimate the treatment effect of capital grants on productivity, we follow standard procedure from the experimental literature, comparing outcome distributions between treatment and control groups. Our main object of interest is the log of total factor productivity (TFP), which we construct as:

$$\log \widehat{TFP}_{it} = y_{it} - \hat{\beta}_k \cdot k_{it} - \hat{\beta}_l \cdot l_{it} - \hat{\beta}_m \cdot m_{it}, \tag{4}$$

where $\hat{\beta}_k$, $\hat{\beta}_l$ and $\hat{\beta}_m$ are the estimated production function coefficients.

We estimate the effect of treatment on productivity by exploiting the randomised assignment of treatment:

$$\log \widehat{TFP}_{isct} = \beta_T \cdot T_{it} + \gamma_{ct} + \mu_{sc} + \varepsilon_{isct}, \tag{5}$$

pooling microenterprises across all time periods and across both countries, for maximal statistical power. The coefficient of interest is β_T, the productivity treatment effect. We also include time t and industry s fixed effects separately for each country c (μ_{sc} and γ_{ct}). We calculate in turn TFP using production function coefficients from each of the two methods from above. For inference, we cluster standard errors at the unit of treatment assignment, which in this case is the firm (Abadie, Athey, Imbens, and Wooldridge, 2023).

This approach to estimating the treatment effect on TFP is fully consistent with the structure of the production function that we postulated in equation 2. This follows immediately from rearranging the equation as:

$$y_{it} - \beta_k \cdot k_{it} - \beta_l \cdot l_{it} - \beta_m \cdot m_{it} \equiv \beta_T \cdot T_{it} + \mu_{sc} + \gamma_{ct} + \omega_{it} + v_{it}$$

This expresses TFP as an identity: the right-hand side contains all the elements of $\log(TFP_{it})$ as defined in equation 5, and the left-hand side defines $\log(TFP_{it})$ as the difference between output and the sum of input shares times production function coefficients as in equation 4.

We also implement a third approach, which does not try to identify the production function coefficients in a first stage model. Instead, this approach defines productivity in terms of labour productivity $\log(Y/L)$. A potential disadvantage in this context is that, when firms are financially constrained, grants will relax constraints on capital and materials inputs, resulting in a mechanical increase of measured productivity per hour worked. We therefore control for capital and materials inputs directly in the regression (for a recent example of such an approach, see Bloom et al. (2019)). Formally, we re-write the production function 2 in terms of $\log(Y/L)$):

$$\log\left(\frac{Y_{it}}{L_{it}}\right) = \alpha \cdot \left(\frac{Y_{i0}}{L_{i0}}\right) + \beta \cdot T_{it} + \tilde{\beta}_k \cdot \log\left(\frac{K_{it}}{L_{it}}\right) + \tilde{\beta}_m \cdot \log\left(\frac{M_{it}}{L_{it}}\right) + \tilde{\beta}_l \cdot \log\left(L_{it}\right) + \gamma_{ct} + \mu_{sc} + v_{isct}. \quad (6)$$

In this model, β identifies the effect of capital grants on labour productivity after controlling for other inputs. Since there is no attempt to identify the production function coefficients themselves, we estimate this model in a single step using OLS.

Table 2 presents our main result. Panels A, B and C in turn use as outcome variable TFP estimated using production function coefficients of each the the two main approaches of the previous section: a linear panel Blundell and Bond (1998) estimation and the estimation method proposed by Gandhi et al. (2020). Panel C reports coefficients on treatment and on the input controls from our regression of labour productivity.

Our results are positive across the three productivity measures. We find that treatment increases productivity significantly by 3-6 percent on average, and 3-7 at the median. We find particularly an outward shift at the top of the distribution: productivity increases by 6-9 percent at the 80th percentile, which is significant across all production function estimators. These effects are statistically significant. (For TFP based on Gandhi et al. (2020), this is significant only at upper percentiles, not at the mean or median; this result appears to be driven by the inflated returns to capital and intermediates that this approach estimated in Sri Lanka).[15] We also test for differences in TFP of treated and control microenterprises non-parametrically. We show the distributions in Figure 1. Since the location of the log(TFP) distribution is country-specific, we report separate graphs for Ghana and Sri Lanka, and for both production function estimation methods. Visually, we see that TFP is higher in treated microenterprises than in control firms. The distributions drift apart particularly for higher levels of TFP, consistent with what we found using quantile regressions. We formally test for equality of distributions using a Wilcoxon rank-sum test, and reject equality clearly for Ghana (for both measures) and also for Sri Lanka (for the linear panel approach).[16]

In sum, these findings suggest the effect of capital grants on profits does not work through the adjustment of homogenous production factors – capital, materials and labour – alone. There is an additional effect of grants on output which is loaded onto measured productivity. This increase in productivity comes from the top of the distribution: capital grants enable the most productive microenterprises to become even more productive.

[15] See Appendix Tables A.16 and A.17.

[16] We allow for arbitrary correlation within firms across time using randomisation inference, where we simulate re-randomisation using the sampling designs in the original studies. We do not test for differences in labour productivity non-parametrically (i.e. without controlling for inputs), given that the treatment lead to large increases in capital and materials.

4.2 Robustness of productivity effects

We assess robustness of our findings in various ways. To begin with, instead of focusing on a single method, we already established a pattern of higher productivity in grant-receiving firms based on three different productivity estimates, each underpinned by sometimes quite different sets of assumptions. We find treatment effects not only at the mean, but also at various points of the distribution; this finding is robust to a completely non-parametric test of differences in the entire productivity distributions. All of this should give us confidence that we pick up a common signal about productivity effects of grants across these measurements.

Here, we summarise the results of a number of further robustness exercises (relegating the details in the appendix). First, we consider a larger set of alternative TFP measures in Table A.8. Specifically, we construct TFP using, in turn, the production function estimates from Tables A.2 and A.5. These are based on a large array of production function estimators using alternative specifications in addition to those used in Table 2. Again, the magnitude and pattern of our main results are upheld: TFP increases by 3-8 percent at the mean, and by 6-11 percent at the median of the TFP distribution.

Second, we explore robustness to different functional forms of the production function. In all our analysis so far, we maintained the assumption of a Cobb-Douglas production function that we made in equation 1. As an alternative, we consider the translog production function, a second degree polynomial expansion in the inputs capital, labour, and materials. This is a flexible empirical approximation to a more general CES production function. As the estimates in Table A.9 show, the results remain very similar when using this more flexible functional form. (However, as one would expect, the coefficient estimates for translog in Table A.10 are much noisier than those for Cobb-Douglas.) We further cannot reject the null hypothesis that all second-order terms are jointly zero and hence that the production function is Cobb-Douglas. We therefore conclude that, while our preferred functional form is Cobb-Douglas, our estimates are empirically robust to more flexible functional form assumptions.

Third, we explore robustness to alternative measures of the capital stock. In particular, while our main measure of capital stock follows the approach in DMW and FMQW and does not account for asset depreciation, we alternatively allow for a range of plausible depreciation rates for microenterprise capital stock between 5 and 25 percent per year. As Tables A.12 to A.15 show, our results are robust to this entire plausible range of depreciation rates, with minimal quantitative changes. Fourth and finally, we show results that are estimated separately for Sri Lanka and Ghana (Appendix Tables A.16 and A.17). We find very similar patterns in both countries, with TFP increases in the upper part of the distribution.[17]

The differential effects of capital interventions in informal firms by gender are of substantial interest in the literature: for example, they were specifically taken into account in the experimental design in the Ghana study, and have recently been further investigated in Bernhardt, Field, Pande, and Rigol (2019). While this is not the focus of our paper, we nevertheless test for gender heterogeneity in TFP effects in both datasets. Our results (in Tables A.18 and A.19) are inconclusive, and we note that our tests have low power. We find suggestive evidence of higher treatment effects for men in Sri Lanka, and for women in Ghana. However, we note that we cannot reject the null hypothesis of equal treatment effects across gender in either setting.

Lastly, we consider the difference between productivity effects of different treatment types. In Ghana, we find some, again only suggestive, evidence that TFP effects are higher for in-kind treatments Appendix Table A.21. These results add a complementary perspective to the 'flypaper effect' discussed by FMQW. The authors find stronger evidence for treatment effects in microenterprises which received in-kind grants (especially those run by women). For Sri Lanka, the point estimates are somewhat higher for cash treatments.

4.3 How important are the productivity effects of capital grants?

Having established productivity effects of capital grants in a methodologically robust way, we now assess their economic significance. In other words, we turn to the question of much of the effect of capital grants is driven by productivity, and how much is driven by adjustments in production factors. Using the production function in equation 2, we can decompose the average treatment effect (ATE) of capital grants on revenue as follows:

$$\mathbb{E}\left(\frac{\Delta y_{it}}{\Delta z}\right) \approx \mathbb{E}\left(\frac{\Delta a_{it}}{\Delta z}\right) + \beta_k \cdot \mathbb{E}\left(\frac{\Delta k_{it}}{\Delta z}\right) + \beta_l \cdot \mathbb{E}\left(\frac{\Delta l_{it}}{\Delta z}\right) + \beta_m \cdot \mathbb{E}\left(\frac{\Delta m_{it}}{\Delta z}\right), \tag{7}$$

where $a_{it} = \log A_{it}$ is the log of TFP and z is treatment status (which in our case is binary).[18] Equation 7 breaks down the revenue effects of capital grants into the contributions associated with adjustments to production factors, and changes in TFP. Replacing population quantities with sample analogues (our estimated coefficients of the production function, and estimated treatment effects on inputs and TFP) lets us immediately

[17] We note that country-level results are only individually statistically significant in Sri Lanka. However, the non-parametric evidence showed a significant improvement in TFP for treated firms in both countries, and especially in Ghana. Based on this, and also noting the similar effect sizes across countries, we conclude that treatment has shifted TFP in both countries in a similar way.

[18] This derivation is mathematically quite similar to the decomposition applied by growth accounting, which splits GDP growth into its components, based on the aggregate production function. Note, for example, that for $\Delta z \to 0$, the relationship can be expressed in partial derivatives, and the relationship becomes exact, rather than an approximation.

compute this decomposition.

We report the results from the decomposition in Table 3. Since production function coefficients differ by country, we report separate results for Sri Lanka and Ghana. We further report separate decompositions for each method we use to estimate TFP.[19] Using our preferred linear panel estimates, we find that changes in TFP account for about a third of the treatment effect on revenues (namely, 37% in Sri Lanka and 34% in Ghana). The labour productivity approach implies an impact of about 20%, in both countries. The GNR method agrees closely with the linear panel method in Ghana; in Sri Lanka, as our earlier results foreshadowed, this method implies a much lower TFP effect (namely about 3%). The increase in capital stock accounts for about 20-40%, and higher material use contributes between 40-60% of the increase in revenues. The contribution of changes to labour input on revenues is negligible.[20]

4.4 Are effects sustained in the long term?

Improvements in microenterprise productivity are especially noteworthy because they can potentially shift firms into a higher steady-state of capital, revenue and profits (see, for example, the theoretical framework in FMQW), resulting in lasting effects on firm size, revenue, and profits. We turn to the long-term follow up data for Sri Lanka to assess whether productivity improvements and shifts in the asset composition are sustained over time. de Mel et al. (2012) report a sustained increase in profits for the treatment group more than six years after the initial capital grants.[21]

In Table 4 we include these long-term follow-up surveys into our data, and report dynamic treatment effects separately by the year since the capital grant was given. While increasing firm heterogeneity over time makes the long-run estimates very noisy – as evidenced by the large standard errors – the point estimates are consistent with the idea that TFP *and* fixed capital are sustainably higher in treatment firms, as would have been the predicted effects of a productivity shock in any standard growth model. About six years after the intervention, point estimates for both outcomes are similar to the effects found in the first year (and equality of effects at different time horizons cannot be formally rejected); even though effects are individually not statistically significant beyond two or three years after the intervention, due to increasing noise. The fact that treatment effects on capital do not rise with time suggests that firms treated with grants make all their additional investments right after receiving their grants. Indeed, we find that the

[19] The treatment effects on production factors are not dependent on the TFP estimation method and therefore do not vary within a country. Contributions of these factors do vary since they again depend on the estimated production function coefficients.

[20] Indeed, it is even slightly negative in Ghana; this is due to a very small but negative treatment effect on labour inputs.

[21] In Ghana, FMQW find significant effects about three years after treatment. Their three-year follow up data, however, does not contain the variables that we would need to calculate productivity.

asset purchases of treated microenterprises are clustered in the period immediately after the grant payout; there is no crowding-in of follow-up investment (Appendix Figure A.2).

Where we do find significant disinvestment over time is in the stock of goods and materials that the firms hold in inventory. Firms decapitalise inventories quickly after the first year, such that stocks in any subsequent year revert back to the level of the control group. This evidence suggests that the most profound change in microenterprises immediately after treatment – a strong increase in inventories, which account for two thirds of business purchases from the grants – cannot explain the sustained increase in productivity and profits. This rules out a mechanism where productivity effects would be driven by a higher level inventories, for example through reduced stock-outs, better customer choice, or lower re-stocking costs potentially associated with higher inventories stocks. Rather, these results suggest that the mechanism is related to the purchases of fixed capital assets that grant-receiving firms undertook. Further, as noted earlier, this kind of inventory response seems inconsistent with the GNR assumption that, conditional on capital and labour, firms choose inventories optimally in each period.

To be clear, this result does not imply that all capital transfer programmes will have lasting impacts; nor does it imply that impacts will necessarily be sustained over longer horizons. For example, in their nine-year follow-up of a cash-grant programme in Uganda, Blattman, Fiala, and Martinez (2020) find minimal evidence for lasting impacts on income or consumption, notwithstanding large impacts at the four-year follow-up (Blattman, Fiala, and Martinez, 2014); this convergence was due, in part, to the control group having caught up to the treatment recipients. Thus, our results show that TFP gains are likely to form an quantitatively important mechanism by which capital grants impact microenterprise performance – but, as Blattman et al. (2020) note, the extent to which such grants have lasting treatment effects depends as much upon the opportunities available in a particular context to members of the control group.

5 Mechanisms

5.1 What kind of capital do capital grants buy?

After documenting the effects of capital grants on productivity as well as the importance of this channel for observed revenue increases, we now examine the mechanisms through which capital grants can enhance productivity. In particular, we test the plausible hypothesis that this occurs through productivity embodied in capital. Solow (1959) originally formalised this idea in an aggregate growth model. Unlike in its better-known cousin – '*the*' Solow (1956) growth model – firm productivity in Solow (1959) does not grow independently of capital investment. Exogenous frontier productivity growth increases availability of newer and more productive capital vintages. But frontier productivity growth does not automatically diffuse to all firms. Instead, technological progress is passed through to a firm only if and when it chooses to replace its old capital stock with the new, more productive frontier variety. Old capital is still perfectly useful (until it randomly breaks down) but newer capital can be used in the same activities more effectively. In other words, firms will lag behind the productivity frontier if they do not possess the most advanced equipment that is available.[22]

To test this mechanism, we turn to the detailed listing of capital assets in the questionnaire from DMW in Sri Lanka.[23] The questionnaire puts individual business into the following categories, as determined in the field by respondents and/or enumerators: business tools or utensils, machinery, furniture and equipment, vehicles, and other physical assets (excluding inventories). Assets were categorised and subsumed under a certain heading in the field by respondents and/or enumerators. We use the categorisation *as it is given in the data* to distinguish between essential businesses assets – such as machinery, tools, and furniture, owned by 90% of firms at baseline – and assets that are less essential. The latter include vehicles and other durable assets, including refrigerators and other household electronics. At baseline, only 30% of firms own any asset in this category.

In addition, since it is not clear which functional categories of capital should embody technology, we additionally hand-code individual items according to whether they have a more advanced technology component, irrespective of which category the items are recorded in.[24] In our context of Sri Lankan microenterprises, higher-technology assets tend to be powered tools, or items made out of better material than older vintages. These assets generally serve a similar purpose and are useful in similar activities and industries as their less technology-intensive counterparts. To give a few examples, we code electronic scales as higher-technology, but not scale weights. Battery chargers, motorised vehicles, glass showcases and hair dryers are higher-technology; tires and tubes, bicyles, wooden tables and scissors are not.

We find that microenterprises in the treatment group acquire different assets than the control group, and that those assets are technologically more advanced. Table 5 displays the effects of capital grants on different categories of microenterprise capital. As before, we report coefficients on treatment dummy from ANCOVA regressions. We find that, pooled across follow-up waves, microenterprises increase their fixed capital stock by about as much as their inventories stock. Within fixed assets, most of the investment

[22] The Solow (1959) model is therefore consistent with productivity dispersion among firms, consistent with a large body of modern empirical evidence.

[23] While FMQW use a similar questionnaire in Ghana, they do not ask for a list of individual asset items together with their names.

[24] While are aware that this exercise is arguably subjective, we performed this categorisation as one of the first analytical steps in this paper, according to some clear criteria (e.g. is the asset power-operated or not? Is it artisanal or industrially manufactured?). For complete transparency, we provide the complete list of items and our classification in Appendix Table A.25.

occurs in vehicles and in assets classified as 'other durable goods' – they increase by about 2,600 rupees (about 26 USD) or 70% relative to the control mean, compared to machines, tools and furniture which only increase by about 10% relative to the control mean. Almost all of these durables that treated firms acquire are classified as technologically more advanced. Thus, when we separate assets by their technology content, we find that high-technology assets increase significantly by about 2,800 rupees or about a quarter of the control mean. In total, about 70% of the increase in capital comes from high-technology vehicles and durable goods.[25]

This evidence shows that capital grants tilt the composition of fixed capital items in firms, and that investment following capital grants is not homothetic across assets. Treated microenterprises do not invest more into asset categories that are essential to running the firm – such as machinery, which comprises almost half of the average capital stock in control firms. Instead, treated firms acquire assets that previously played a more marginal role, including vehicles and electronic goods. We show this in Figure 2, which graphs this extensive margin of asset ownership over time for the treatment and control groups. Since at baseline, most firms already own essential assets, this leaves little room for treatment to exert an effect. Indeed, at endline, in both the treatment and the control group, 96% own such item. On the other hand, only 30% of microenterprises own vehicles and durables at baseline. We therefore call these categories 'non-essential' assets. It is in these assets that we see all the effects of treatment. During the intervention window, ownership of non-essential assets climbs to more than 50% for treated firms, but stays unchanged for control firms. While longer-term estimates are again very noisy, point estimates suggest that the tilt of the asset composition is sustained over time (Appendix Table A.24).

Detailed qualitative evidence on the type of assets purchased gives us another angle to understand how capital grants change the composition of capital. Among the most commonly purchased assets in the treatment group are vehicles, refrigerators, and showcases.[26] Refrigerators and showcases make up around 60% of other durable assets, both by quantity and by value. Such items are often not essential for carrying out the small-scale manufacturing, trade and service activities that small Sri Lankan firms engage in. But they can allow business owners to carry out their activities more effectively. Table 6, which reports the effects of treatment on a range of different indicators of how firms do their business, illustrates this. The customer base, the likelihood to introduce new

[25] In Appendix Table A.23 we provide a more detailed breakdown of effects by asset category, as well as for the extensive margin (asset ownership). The results further support our interpretation here.

[26] These items are much less commonly purchased by the control group. Even though the absolute number of cases for each specific item are small – e.g. fewer than 1% of treated firms purchase refrigerators – the available evidence suggests that increases in these asset categories are substantial. For instance, after receiving capital grants, the number of firms with refrigerators doubles, and the number of bicycles and showcases increases by 50 percent.

products, and the share of revenue from new products all increase by about 20% each.[27] We also find statistically significant effects on the share of firms selling refrigerated or perishable products – a tripling and doubling, respectively, relative to the control group – despite the fact that the absolute magnitude of this effect over the entire sample is of course low, and similar to the share of all firms that adopt refrigerators (about 1%). We find no effect on reducing spoilage of goods (Column (6)) or on entry into new businesses or new business locations (Columns (7) or (8)). Taken together, this illustrative evidence suggests that capital grants enabled entrepreneurs to carry out their existing businesses to a higher standard.[28]

An alternative interpretation of our findings on asset ownership is that these changes in the asset composition reflect purchases of consumer durables for use in the entrepreneurs' household consumption, rather than for productive use in the business. If changes in assets were entirely due to private consumption, then we should find no productivity effect accompanying the asset effect. To anticipate results in the next section, we will find that changes in firm productivity are almost entirely driven by changes in the asset composition. Our results illustrating the usefulness of refrigerators for business are also inconsistent with this alternative hypothesis. More generally, if diversion of assets to private consumption is a concern, then our estimates would reflect a lower bound to the true effects of capital-embodied productivity.[29] To further test this possibility, we test for heterogeneity of results by whether the business is run from home, a proxy for the divertability of business assets. We find no difference in effects (Appendix Table A.26).

5.2 What drives the treatment effect?

To take stock of our findings so far: we have documented that capital grants, viewed through the lens of a standard gross output production function, increase productivity in microenterprises by about 3-7% on average. This explains up to 37% of the overall effects that capital grants had on revenue, the rest being explained by larger stocks of materials and capital. The expansion of the capital stock in grant-receiving firms is far from uniform: almost all of the extra capital purchases consist of technologically more

[27] See also Cai and Szeidl (2024), who find that an increase in financing for Chinese firms – 70% of which were in retail – increased the size of firms' customer base, their probability of renovating, and the likelihood of introducing new products (among other impacts).

[28] Since a large share of firms in our sample are retail firms, the question whether higher 'productivity' does not simply reflect higher prices driven by product quality or price markups. We find (Appendix Figure A.1) that sales margins are the same between treated and control firms – and, indeed, may even be lower in treated firms.

[29] One might further hypothesize that assert diversion has an effect on household wealth, or makes them more efficient at home production. This could be reflected in more follow-up investments by wealthier treatment microenterprises, or by an increased labour supply of treated entrepreneurs, as evidence that such mechanisms drive the results. We find no difference in follow-up investments (Appendix Figure A.2) or in hours worked at the firm (Table 3).

advanced assets, especially durable goods and vehicles. This suggests that different, more productive capital vintages may be a mechanism behind the effects we document on productivity. But does the effect on productivity work *through* the composition of capital?

To test for whether the increase in productivity can indeed be attributed to the change in the capital composition, we turn to formal mediation analysis. In particular, we estimate the 'average controlled direct effect' (ACDE) proposed by Acharya et al. (2016), which decomposes the effect of treatment T on an outcome (in our case, TFP) into the direct effect of treatment on the outcome while leaving the mediator M constant, and the remainder of the treatment effect which can be attributed to the mediator. Formally, we first estimate the auxiliary model:

$$\log \widehat{TFP}_{isct} = \alpha \cdot \log \widehat{TFP}_{i0} + \beta_1 \cdot T_{it} + \beta_2 \cdot M_{it} + \beta_3 \cdot T_{it} \cdot M_{it} \gamma_{ct} + \mu_{sc} + \epsilon_{isct}, \tag{8}$$

which augments our baseline TFP effects model with the mean-zero mediator M_{it} and its interaction with treatment status. Second, we de-mediate the outcome by taking out the contribution of the mediator contained to the outcome:

$$\log \widetilde{TFP}_{isct} = \log \widehat{TFP}_{i0} - \hat{\beta}_2 \cdot M_{it} - \hat{\beta}_3 \cdot T_{it} \cdot M_{it}. \tag{9}$$

The de-mediated outcome is then the outcome that would have ocurred had the level of the mediator been equal to its mean. The third and final step involves estimating the ACDE by repeating the treatment effects regression, but replacing the outcome by its de-mediated value:

$$\log \widetilde{TFP}_{isct} = \alpha \cdot \log \widehat{TFP}_{i0} + \beta_{ACDE} \cdot T_{it} + \gamma_{ct} + \mu_{sc} + \epsilon_{isct}. \tag{10}$$

We report the results from this exercise in Table 7, using the Blundell and Bond (1998) TFP estimates. We consider four different mediator variables: the logs and the shares of high-tech and non-essential capital. Since these variables are only available in Sri Lanka, we again restrict the analysis to this setting. We find average controlled direct effects that are close to zero and statistically very far from significant. That is, the results imply that essentially the entire effect of treatment of capital grants on measured productivity is explained by the adoption of higher-technology and non-essential assets.

These results lend credence to the interpretation that a mechanism of technology-embodied capital can explain why capital grants incresed microenterprise productivity. We interpret technology here in a broad way, meaning both assets with a higher technology component as well as asset classes (such as non-essential durables and vehicles) that allow the microentrepeneur to run their business more efficiently for a given set of inputs.

6 Conclusion

In this article, we look at microenterprises through the structural lens of a production function. We use several complementary methods to estimate production functions for microenterprises; this enables us to analyse the effects of capital grants on productivity. We find that capital grants to microenterprises in Sri Lanka and Ghana have significant effects on total factor productivity. A decomposition analysis suggests that returns to capital grants for microenterprises contain a significant return to increased productivity. We find evidence for a plausible mechanism behind this: capital items that embody superior technology allow firms to improve total factor productivity. Treated firms acquire more technologically-advanced asset vintages, and invest into capital that previously played a less essential role for firms, such as vehicles and durables. Firms change how they do business as a consequence: we find that they serve more customers, introduce and sell more new products. We also find suggestive evidence of lower prices for consumers.

Taken as a whole, our results provide a more nuanced interpretation to the previous assumption in this literature that treatment effects reflect returns to capital alone. This has three main implications for our understanding of microenterprises – and, more generally, for the design of policy that provides capital to such firms (whether through grants or through microfinance).

First, our results have implications for the scale of potential transfers. If we believe that the impact of capital grants operates exclusively through a capital channel, we should anticipate that the highest marginal returns should accrue to small grants (given diminishing returns to capital in any standard production function framework); in turn, this implies that optimal policy for the distribution of capital should be to provide a large number of small grants or small loans. In contrast, the productivity effect that we find in this paper, both in our main results and in our results on higher-technology durables, imply that capital returns are likely to be non-convex – in the sense that they require a relatively large and lumpy change in the capital stock, of the kind observed in both the Sri Lankan and Ghanaian studies. This implies that policies on capital provision – again, whether through cash grants or through microfinance – should be sufficiently large as to encourage a shift in the kind of capital that a microenterprise is using. Put differently, the kind of capital upgrading that we document in this paper is clearly a non-marginal behaviour. This is consistent with recent evidence from the microfinance literature, suggesting that larger transfers – and, in particular, asset-based transfers – may provide for important gains that smaller transfers do not (see, in particular, Bauchet and Morduch (2013), Bari, Malik, Meki, and Quinn (2024) and Bryan, Karlan, and Osman (2024)).

Second, if we believe that capital grants operate exclusively through a capital channel, we should anticipate the impact of capital grants to be transitory (as in the model, for example, in Fafchamps et al. (2014)). In contrast, the result that lumpy transfers have

TFP effects implies that the impact of such transfers is likely to be quite persistent. This is consistent with our results (as discussed in section 4.4), and with evidence showing long-term impacts of the Sri Lankan capital drops (de Mel et al., 2012). This has direct relevance for policy design; any welfare assessment of the value of capital transfers needs to make some assumption about the time horizon over which the gains are likely to be enjoyed.

Third, and most generally, we see our results as speaking to broader policy debates on the persistence of small informal firms in developing economies (Hsieh and Klenow, 2009; Meghir, Narita, and Robin, 2015; Ulyssea, 2018). Such debates often view microenterprises as having intrinsically low productivity – and, therefore, such debates are often very pessimistic about the prospects for encouraging growth through sustainable microenterprise expansion. Our results suggest cause for guarded optimism in this space; by showing that capital transfers can enable firms to adopt higher-technology durables, and that such durable adoption explains TFP increases, we show that there are prospects for technological upgrading, and that such upgrading can be facilitated through capital transfers.

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Tables and Figures

Table 1: **Production functions estimates for microenterprises in Sri Lanka and Ghana**

	Sri Lanka		Ghana	
	(1)	(2)	(3)	(4)
Specification:	Blundell-Bond	Gandhi-Navarro-Rivers	Blundell-Bond	Gandhi-Navarro-Rivers
Log capital	0.12*	0.28***	0.23***	0.23***
	(0.07)	(0.03)	(0.09)	(0.03)
Log labour	0.13***	0.16***	0.20***	0.22***
	(0.05)	(0.04)	(0.05)	(0.04)
Log materials	0.42***	0.51***	0.39***	0.39***
	(0.06)	(0.03)	(0.10)	(0.03)
L.Log revenue	0.34***		0.20***	
	(0.06)		(0.04)	
Observations	2610	2312	3105	2071
Microenterprises	382	372	770	770
Hansen (p-value)	0.12		0.29	
$\hat{\beta}_k + \hat{\beta}_l + \hat{\beta}_m$	0.67		0.82	
Constant returns (p)	0.00		0.05	
AR(1) (p)	0.00		0.00	
AR(2) (p)	0.42		0.18	
Instruments	77		45	
Underidentification (p-values):				
Log capital	0.00		0.00	
Log labour	0.00		0.00	
Log materials	0.00		0.00	
L.Log revenue	0.00		0.00	

Note: Estimators employed are Blundell and Bond (1998) System GMM and the Gandhi et al. (2020) first-order condition based estimator. All models partial out wave dummies, sector dummies, and post-treatment status (not reported). For Blundell-Bond, we report p-values for the Hansen (1982) test of over-identifying restrictions, the Arellano and Bond (1991) autocorrelation test, and the Windmeijer (2024) test of instrument informativeness. Samples are equivalent to the preferred samples in the respective original studies. *, ** and *** denote significance at the 10, 5 and 1 per cent levels.

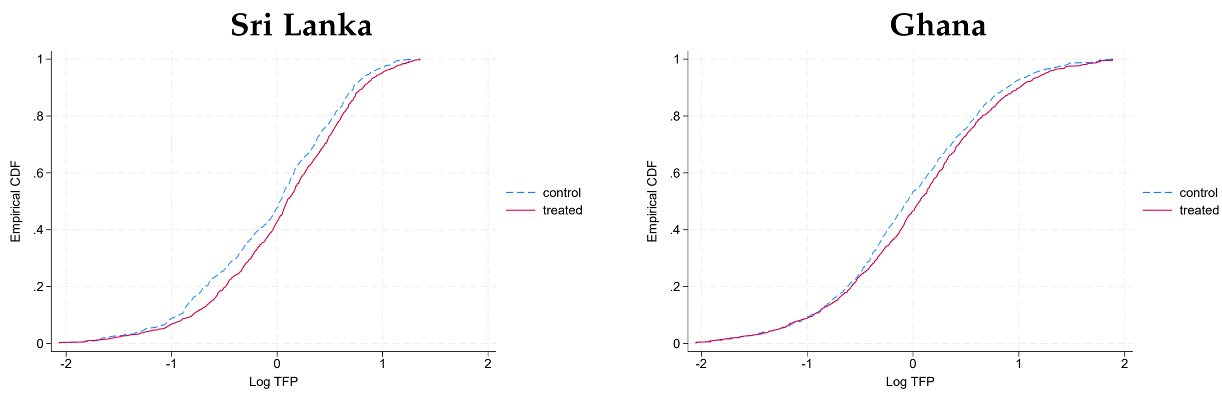
Table 2: **Capital grant treatment effects across all measures of productivity (Sri Lanka and Ghana - pooled)**

	(1) OLS	(2) Quantile (0.2)	(3) Quantile (0.4)	(4) Quantile (0.5)	(5) Quantile (0.6)	(6) Quantile (0.8)
A. Dependent variable: log(TFP) estimated using Blundell-Bond estimator						
Dummy: Treated	0.06*	0.02	0.08*	0.07*	0.09**	0.09**
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Observations	4777	4777	4777	4777	4777	4777
Microenterprises	1114	1114	1114	1114	1114	1114
B. Dependent variable: log(TFP) estimated using Ghandi-Navarro-Rivers estimator						
Dummy: Treated	0.03	-0.01	0.03	0.03	0.03	0.07**
	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
Observations	4777	4777	4777	4777	4777	4777
Microenterprises	1114	1114	1114	1114	1114	1114
C. Dependent variable: log(revenue/hours worked)						
Dummy: Treated	0.03	-0.01	0.01	0.04*	0.05*	0.05*
	(0.03)	(0.04)	(0.02)	(0.02)	(0.02)	(0.03)
Log(Capital/labour)	0.11***	0.03**	0.04***	0.06***	0.07***	0.11***
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Log(Materials/labour)	0.63***	0.82***	0.79***	0.77***	0.74***	0.63***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)
Log labour	-0.06**	-0.01	-0.03	-0.04**	-0.06***	-0.11***
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	4777	4777	4777	4777	4777	4777
Microenterprises	1114	1114	1114	1114	1114	1114

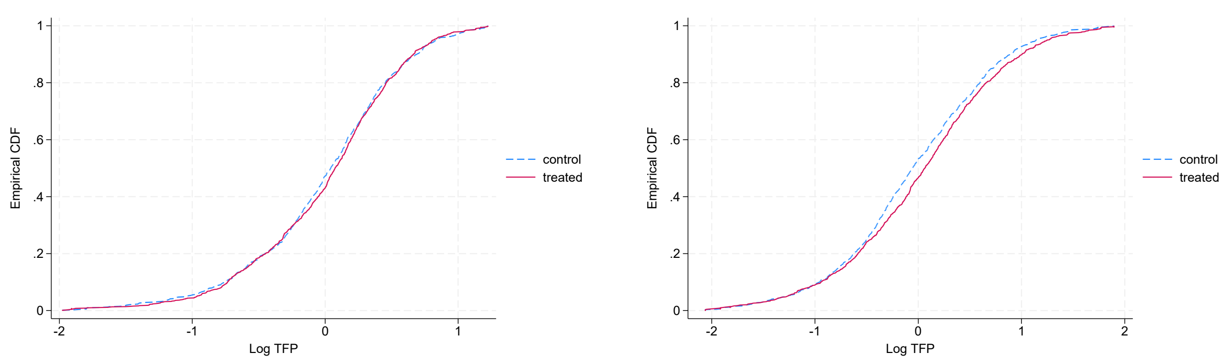
Note: This table reports the effect of treatment on TFP at different moments of the distribution, for microenterprises in Ghana and Sri Lanka. In Panel A, TFP is estimated using the Blundell and Bond (1998) System GMM estimator. In Panel B, TFP is estimated using the Gandhi et al. (2020) estimator. In panel C, the dependent variable is the standard measure of labor productivity. All regressions include wave-times-survey and industry-times-country fixed effects.

Figure 1: **Capital grant treatment effects on productivity**

(a) Blundell-Bond



(b) Gandhi-Navarro-Rivers



Note: CDFs of TFP for treated and untreated microenterprises in the post-treatmenet waves. Wilcoxon rank-sum test of equality of distribution p-values: 0.00 (Sri Lanka, Blundell-Bond), 0.49 (Sri Lanka, Gandhi-Navarro-Rivers) and 0.02 (Ghana, Blundell-Bond), 0.02 (Ghana, Gandhi-Navarro-Rivers). P-values were obtained using randomisation inference (with 100,000 replications) and take into account the clustering of the data at the level of the microenterprise across survey waves.

Table 3: **Decomposing the effect of capital grants on revenue**

Sample:	**Sri Lanka**			**Ghana**		
Production function estimates:	Blundell-Bond	Gandhi-Navarro-Rivers	OLS (Y/L)	Blundell-Bond	Gandhi-Navarro-Rivers	OLS (Y/L)
Treatment effect: Revenue	0.202	0.202	0.202	0.140	0.140	0.140
Treatment effect: TFP	0.074	0.006	0.038	0.046	0.047	0.030
Treatment effect: Capital	0.307	0.307	0.307	0.173	0.173	0.173
Treatment effect: Materials	0.190	0.190	0.190	0.140	0.140	0.140
Treatment effect: Labour	0.050	0.050	0.050	-0.007	-0.007	-0.007
Contribution: TFP	0.367	0.027	0.190	0.333	0.337	0.212
Contribution: Capital	0.182	0.424	0.168	0.282	0.280	0.131
Contribution: Materials	0.391	0.480	0.593	0.394	0.392	0.622
Contribution: Labour	0.033	0.040	0.056	-0.011	-0.012	-0.010

Note: This table decomposes the effect of capital grants on revenue into the contribution of TFP and the contribution of production factors, following equation 7. Average treatment effects (ATE) are estimated with OLS (and hence are identical across columns, within country, for capital, materials and labour). Relative contributions of each factor are calculated according to equation (7) by multiplying ATE with factor elasticities, divided by ATE on revenues. Factor elasticities and TFP treatment effects are specific to the production function estimate used in each column; and are reported in earlier tables. Treatment effects on revenue, capital, materials and labour are common for each sample. We apply the same sample restriction as for the production function estimation, retaining observations with non-missing data on revenues and all inputs. Contributions may not add up to 1 due to rounding.

Table 4: **Long-term effects of capital grants on productivity, capital, and intermediate inputs (Sri Lanka)**

	(1) ln(TFP)	(2) Fixed capital	(3) Inventories	(4) Total expenditure
Dummy: Treated × Year 1	0.10**	3876.51***	4112.53***	1800.20
	(0.05)	(984.62)	(1465.20)	(1528.26)
Dummy: Treated × Year 2	0.11**	3288.10**	1672.77	-150.26
	(0.05)	(1346.60)	(1762.09)	(1554.64)
Dummy: Treated × Year 3	0.06	3357.59*	-216.79	-1207.77
	(0.07)	(1838.41)	(2044.44)	(1479.31)
Dummy: Treated × Years 5-6	0.09	4450.13	-0.97	1266.10
	(0.06)	(4077.48)	(2202.90)	(4051.92)
Control mean: baseline	-0	12,624	14,131	9,015
Control mean: 3 years	0	22,647	14,606	11,596
Observations	4,164	4,763	4,749	4,527
Microenterprises	385	385	385	385
p-value: Year 1 = Year 2	0.75	0.43	0.05	0.06
p-value: Year 1 = Year 3	0.56	0.73	0.01	0.04
p-value: Year 1 = Year 4	0.83	0.89	0.07	0.90

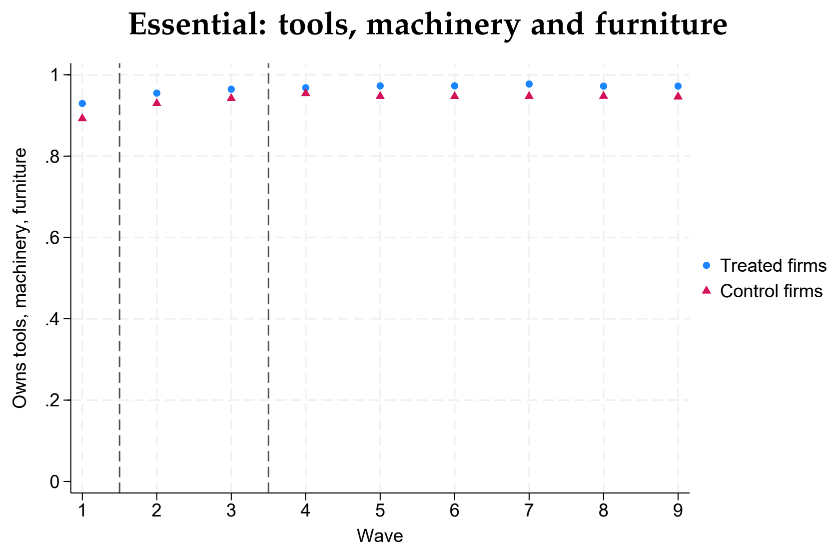
Note: This table shows the evolution of effects of capital grants on TFP, assets and materials for up to six years after treatment. TFP is from the preferred Blundell-Bond estimator. All other variables are as defined in Table 5. In additional, total expenditure in column (8) is total business expenditure in the last month, minus the wage bill. Breadown of individual asset items not available in year 5 and 6 surveys. All regressions are ANCOVA and control for wave dummies. *, ** and *** denote significance at the 10, 5 and 1 per cent levels.

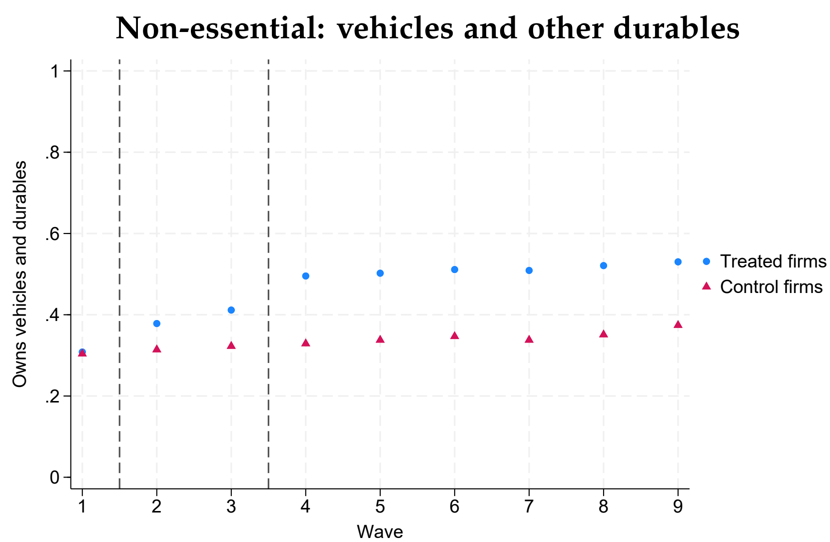
Table 5: **Effects of capital grants on microenterprise capital (Sri Lanka)**

	(1) Inventories	(2) Fixed capital	(3) Machines, tools & furniture	(4) Vehicles & other durables	(5) Low-tech capital	(6) High-tech capital
Dummy: Treated	4130.42**	3594.79***	688.70	2630.88***	697.04**	2814.05***
	(1848.19)	(961.61)	(712.43)	(625.69)	(320.58)	(892.88)
Control mean	14,519	15,555	11,581	3,763	4,717	10,838
Observations	3,358	3,341	3,329	3,345	3,341	3,341
Microenterprises	385	385	385	385	385	385

Note: This table breaks down the effect on grants on different categories of capital. Fixed capital is broken down by functional category in columns (3) and (4) following the DMW questionnaire, and into technology components in columns (5) and (6) based on our coding. All specifications control for wave dummies and baseline values of the dependent variable. *, ** and *** denote significance at the 10, 5 and 1 per cent levels.

Figure 2: **Ownership of fixed assets: Treatment and control by wave (Sri Lanka)**





Note: This figure shows the share of treatment and control firms that own assets in different categories. Tools, machinery and furniture are owned by almost all microenterprises and are therefore labelled 'essential'. Vehicles and other durables are owned by a smaller fraction of microenterprises, and increase significantly in the treatment as opposed to the control group. The intervention window lies between the two vertical lines.

Table 6: **Effects of capital grants on business practices, market and product scope (Sri Lanka)**

	(1) Customers	(2) New product introduction	(3) New product sales	(4) Refrigerated product	(5) Perishable product	(6) Materials spoilage	(7) New business	(8) New location
Dummy: Treated	2.095**	0.010	0.382**	0.007*	0.011*	0.001	0.001	-0.001
	(1.054)	(0.011)	(0.187)	(0.004)	(0.006)	(0.002)	(0.005)	(0.004)
Control mean	11.831	0.049	1.492	0.003	0.011	0.009	0.013	0.007
Observations	3267	2233	2890	2233	2233	3244	3358	2961
Microenterprises	385	385	385	385	385	385	385	385

Note: This table reports the effect of treatment on business practices. The first column is estimated using ANCOVA, columns (2) to (8) are estimated using OLS. New product introduction and share of sales from new product refer to past three months. Perishable and refrigerated products are coded from the names of new products introduced. Materials spoilage is the share of all materials purchased in the past month that were spoiled in the past month. New business and new location refers to business respondent was running in the previous survey round, three months ago. *, ** and *** denote significance at the 10, 5 and 1 per cent levels.

Table 7: **Mediation analysis using average controlled direct effects (ACDE)**

Mediator:	(1)	(2) Log Tech Capital	(3) Log Non-Essential Capital	(4) Share Tech Capital	(5) Share Non-Essential Capital
Dependent variable: log(TFP) estimated using Blundell-Bond estimator					
Average Treatment Effect (ATE)	0.08				
	(0.047)				
Average Controlled Direct Effect (ACDE)		0.01	0.01	-0.01	-0.01
		(0.048)	(0.047)	(0.048)	(0.047)
Observations	3036	3032	3036	3032	3036
Explained by mediator (%)		87.9	81.8	107.5	108.3

Note: This table uses the method of Acharya et al. (2016) to calculate the Average Controlled Direct Effect (ACDE) for TFP, using as mediators the level (in logarithms) and share of high-tech and non-essential capital, respectively.