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Stimulus effects of a large public employment programme

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Abstract

We characterize the spending and factor income effects of a large public employment programme in South Africa. We match anonymized participant IDs with weekly individual-level sales data from one of the country's largest retailers, and estimate the treatment effect on participant spending at the retailer. Our event studies show flat pretrends with a sharp increase in average spending of 15%. Effects are substantially higher for non-food products that likely have higher income elasticities, and there are smaller positive effects of 4% that persist in the months after the end of the programme. We use administrative firm data, input-output tables and a survey of participant spending to extrapolate effects of the increase in retailer sales on domestic factor incomes, particularly highlighting effects on the national and local wagebill. Our estimates contribute to evidence that government spending programmes benefit non-programme employment and wages.

Keywords: Public employment; spillover effects; South Africa

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Résumé
Nous caractérisons les effets de dépenses et de revenus factoriels d'un vaste programme d'emploi public en Afrique du Sud. Nous faisons correspondre des identifiants anonymisés de participants avec des données de ventes individuelles hebdomadaires provenant de l'un des plus grands détaillants du pays, et nous estimons l'effet du traitement sur les dépenses des participants auprès du détaillant. Nos études d'événements montrent des tendances préalables stables avec une augmentation marquée des dépenses moyennes de 15%. Les effets sont nettement plus importants pour les produits non alimentaires qui ont probablement des élasticités de revenu plus élevées, et il existe des effets positifs plus modestes de 4% qui persistent dans les mois suivant la fin du programme. Nous utilisons des données administratives d'entreprises, des tableaux entrée-sortie et une enquête sur les dépenses des participants pour extrapoler les effets de l'augmentation des ventes au détail sur les revenus factoriels nationaux, mettant particulièrement en évidence les effets sur la masse salariale nationale et locale. Nos estimations contribuent à la preuve que les programmes de dépenses gouvernementales bénéficient à l'emploi et aux salaires en dehors du programme.

Mots-clés: Emploi public; externalités ; Afrique du Sud

Introduction

What effect does government spending on social programmes have on private sector incomes and jobs? Policy-makers, especially in developing countries with low average incomes and productivity, often perceive a trade-off between alleviating poverty and unemployment directly with social assistance and public employment programmes, versus other resource allocation options intended to support economic growth and job creation with long-run benefits. These aims are somewhat reconciled if government social spending has broader benefits for incomes and jobs among non-beneficiaries. We study such initial stimulus effects from a large public employment programme in South Africa, by matching anonymized programme participants with high-frequency sales data from a major retailer, finding a sharp increase in spending which we show implies benefits to value added, job creation and wages.

The subject of our study is the Presidential Youth Employment Intervention (PYEI), which is a series of programmes announced in April 2020 focused on addressing the high youth unemployment rate in South Africa, estimated to be 49% or 4.7 million youth aged 15-34 (Statistics South Africa 2022).[1] The main component of the PYEI is the Basic Education Employment Initiative (PYEI-BEEI), which is run the through the Department of Basic Education and employs youth for 5 months in each phase. The programme targets 18 to 35 year olds, with high school graduates eligible as Education Assistants (about two thirds of participants) to help teachers in classrooms, and those with less than high school still eligible as General School Assistants (about one third of participants) to help with miscellaneous school tasks such as infrastructure maintenance. We focus on Phases 2 and 3, which employed about 270,000 participants (re-hired for Phase 3), and lasted for 10 months between 1 November 2021 and 31 August 2022. To give a sense of scale, the programme placed an average of 12 assistants in nearly 90% of all schools across the country.

Participants were paid the national minimum wage, which is substantial compared to the distribution of income in the country, and compared to other public employment programmes.[2] The programme focuses on no-fee schools, determined loosely as schools in communities with high unemployment, low income and low literacy, with a preference for participants living nearby. Given the large scale of the programme (nearly 6% of youth employment or 2% of total employment) along with its concentration in low income areas, the PYEI-BEEI payments constituted a large proportion of local area incomes. Appendix figure A.1 shows programme payments as a proportion of local area income, which is up to 10% of real-adjusted income based on the 2011 Census.[3]

In this paper, we study the domestic factor income effects of the PYEI-BEEI payments. The programme likely increases

[1] See overview from January 2021 here. An updated dashboard is kept here.

[2] The relevant national minimum wage was ZAR3,817.44 per month, approximately USD233 per month using the average exchange rate over 2022 of ZAR16.4 per USD. In 2022, the median employed worker was paid about ZAR5,300, and the median household income per capita was about ZAR1,500. The main prior public employment programme, the Expanded Public Works Programme, was exempted from the national minimum wage with an alternative minimum of ZAR12.75 per hour (55% of the national minimum wage of ZAR23.19 per hour).

[3] We use the Census "main place" classification as the definition of local areas. This seemed subjectively like an informative regional level to use, though of course smaller units would make the proportion larger and larger units would make the proportion smaller. Proportions are estimated by dividing through the approximate participant income based on schools by the approximate area income based on the Census.

participant income substantially, which causes them to increase consumption expenditure, for example on groceries at the nearby store; we term this the programme's "direct" effect on sales. The store which has an increase in sales apportions that revenue as payments to the factors of production: labour (wagebill), capital (profits, interest, etc.) and intermediate goods. The former two can be thought of as the programme's direct effects on *incomes* due to factors of production. The third, intermediate goods, similarly increases sales of the store's suppliers, and in turn their suppliers' suppliers, which is a "recirculation" effect that in turn implies a division into payments to factors of production. The direct effect can therefore be decomposed into the payments to labour and capital, accounting for the recirculation payments through intermediate goods. This corresponds to the value added due to these direct effects, which we may think of as the first round of stimulus effects. Further rounds of stimulus effects would account for the resulting expenditure by those factors of production, for example the increase in workers' consumption due to the payments to labour. We cannot estimate these further rounds of stimulus effects, because this would require a marginal propensity to consume, which we are unable to estimate because we do not observe all participant income and expenditure. What we estimate in this paper is therefore *not* a multiplier, but rather the initial stage of the stimulus mechanism.[4]

We focus on the direct effects on sales specifically at a very large South African retailer (henceforth, "the firm"), which has an especially large presence in lower income areas. Indeed, we match 62% of participants to the firm's (fully anonymized) sales data. Our survey of participants suggests that the firm captures a large part of participant spending: supermarket chain stores account for a quarter of all spending, 60% of which is captured by the observed level of spending at our specific firm. The advantage of focusing on spending at this large firm is that we observe a high-frequency, large-sample, precise and reliable measure of participant and control sample spending, both before and after the programme.

We use a simple difference in differences strategy which compares changes in spending of (anonymized) participants with changes in spending of a random sample of the firm's (anonymized) customers who shop in the same area and at the same type of store. We find similar trends in sales before the payments and then a sharp differential increase in participant sales at the beginning of the programme, which we interpret as a direct treatment effect of the PYEI-BEEI payments on the firm's sales of 15%. We confirm robustness to specification (e.g. comparing control sample spending changes within the exact same store), and show that the sales increase came from both an increase in the level of spending when shopping (intensive margin) and in the frequency of shopping (extensive margin).

We find that this direct effect on the firm's sales is sustained for the duration of the programme, and then decreases to about 4% in the months after the programme.

[4] A conventional and comprehensive multiplier would be the increase in GDP over the initial programme payment, which can be calculated from the expenditure side (all increases in expenditure across households, from our participants as well as the expenditure increase by non-participants) or from the income side (all increases in payments to factors of production, including participants and non-participants). This would include further rounds of expenditure increases from workers and owners of capital, all of which are excluded from our calculations since this requires credible identification of a marginal propensity to consume.

Such positive post-programme treatment effects may be due to some degree of participant consumption smoothing (spending from savings during the programme), or participants being better placed to find other employment after the programme, perhaps due to the benefits of their work experience in the programme.

At the product category level, the largest items of spending were groceries, frozen and refrigerated perishables, toiletries, fresh fruit and vegetables, and butchery items, with corresponding direct treatment effects on sales ranging from 14% to 20%.[5] Since these items may be thought of as necessities, their elasticity of sales to income is likely to be relatively low, and we find some evidence in favour of this: the direct effects on sales for non-food items are generally higher (on average 24%), with some items like electronics (63%) and home appliances (51%) much higher. This has implications for the representativeness of spending effects at the firm, where spending on food items is five times larger than spending on non-food items.

After focusing on these well-identified "direct" treatment effects at the firm, we then present a more exploratory and speculative discussion of the division of these direct effects into payments to labour and capital. We first extrapolate the payments to labour or wagebill component accruing directly from the firm, i.e. before accounting for recirculation through intermediate goods. In order to do so, we estimate the elasticity of firm wagebills to sales in the retail sector using firm-level administrative data and following the event-study empirical strategy in Lamadon et al. (2022). We then estimate the recirculation effects from the firm's increased demands to suppliers (and their respective wagebills) by combining this with input-output data. Together, from the direct effect of combined participant spending on the firm of ZAR 7.7 million per month, these imply payments to domestic factors of production or value added of ZAR 5.5 million per month (excluding imports), of which half goes towards wagebills. Finally, we scale these estimates up using spending estimates from a survey of participants, leading to a tentative estimate of the direct effect of PYEI-BEEI on domestic value added of ZAR 38 million per month, ZAR 19 million of which goes towards wagebills. Assuming that local stores hire labour from local areas, ZAR 13 million of this goes towards increases in employment and wages in the local areas (in addition to the actual payments). Although these estimates rely on numerous very strong assumptions, we include them because of the topic's importance, and the lack of prior literature in our context. We note this initial stimulus effect implies substantial economic and local returns to other government social programmes, such as the South African government's large cash grant transfers.

Relevant literature. We review the literature in a companion report (Bassier and Budlender 2021), and briefly highlight a few results here. Perhaps the most compelling study of social spending stimulus effects is a large scale experiment by Egger et al. (2022), which randomly transferred cash of USD 1,000 to over 10,000 poor households in rural Kenya. They find resulting increases in wagebills and profits at nearby enterprises, with minimal price inflation, such that total expenditure in the local economy increased by a 2.5 times the initial combined transfers. Similarly compelling evidence is provided by Muralidharan et al. (2023),

[5] We exclude the few product categories with divergent pre-trends from this list; the most important excluded category by size is Wine & Liquor.

who exploit a randomly rolled out reform to the world's largest public employment programme (India's NREGS), and find substantial increases in non-programme earnings driven by private sector wages and employment. While we cannot estimate a multiplier because we do not observe all household incomes and expenditure, we identify a small part of this mechanism for a major retailer (the direct treatment effects on sales), characterising in detail what would be the first stage of such stimulus effects in our context.[6]

In the South African context, there are several studies on the large government cash transfers programme. Most of these focus on the effects on grant claimants, such as on poverty (see Woolard and Leibbrandt 2013 for a review), or individual labour supply (Ranchhod 2006), or household bargaining power (Ambler 2016). A few consider effects on non-recipients within the same households, such on the job search of co-residents (Ardington et al. 2009; Abel 2019). There are also several structually based studies estimating multipliers, such as Kemp (2020). As far as we are aware, we are the first to partially estimate the stimulus effects of government spending using microdata with a credibly identified quasi-experimental design.

1. Data

1.1. Retailer sales data

Under the auspices of the South African Presidency and the EU-AFD Research Facility on Inequalities programme, we collaborated with a large private sector chain store retailer, henceforth "the firm". The firm is one of South Africa's largest supermarket chain stores, mainly stocks groceries, and is perceived to have a large middle- and lower-income customer base. In the years prior to the onset of the PYEI-BEEI programme, the firm launched a customer loyalty rewards programme; as of December 2021, a very substantial proportion of country's adult population were members. As is the case for other retailer loyalty programmes, customers who sign up provide personal details to the firm, and qualify for discounts on a large range of products. Customers are asked to swipe their rewards card with every purchase.

The firm agreed to provide us with limited access to their fully anonymized customer sales records from the rewards programme, in order to facilitate our research into evaluating the effects of the PYEI-BEEI. The anonymized sales records are held on a secure, encrypted data platform managed by the company Omnisient. We were granted access to records matched with hashed and encrypted participant IDs (see below), as well as an approximately equivalently-sized random sample of other customers. All of our analysis is based on aggregated statistics from large samples, and it would be impossible for us to uncover personally identifiable information of individuals in the data. This project was given formal research ethics clearance by the University of Cape Town on this basis.

The main fields contained in our dataset are an anonymized and encrypted customer

[6] There are also some studies with randomized assignment showing supply constraints result in negative effects on food prices and food security, such as Beegle et al. (2017) in Malawi and Filmer et al. (2021) in Philipines.

identifier, the week end date, the store-specific "banner' (referring to types of stores), the particular store location, and the sales amount. Sales were also disaggregated into 27 categories chosen by the firm.

1.2. Participant data

The PYEI-BEEI is run in partnership with a number of non-governmental organizations. One such partner is Harambee Youth Employment Accelerator, which provided an online job applications portal for the programme. Harambee thus holds the personal details of most participants for phase 2 of the programme (195,540 out of 270,073), with explicit permission from participants that these records may be used for programme evaluation research. In an agreement brokered by the South African Presidency, Harambee agreed to facilitate anonymized and encrypted matching of PYEI-BEEI participants in the firm's rewards sales database, on the basis that participant anonymity and personally identifiable information would not be compromised.

This matching process was made possible by Omnisient's proprietary technology, which involves cryptographically hashing PYEI-BEEI participant national identity numbers such that the original ID numbers cannot be recovered from the encrypted one-way hash. However, the hash allows the matching of similarly encrypted and hashed ID numbers in the firm's dataset to these encrypted and hashed ID numbers in the PYEI-BEEI dataset. Harambee hashed the ID numbers on their premises using the Omnisient encryption algorithm prior to uploading to Omnisient for matching in the PYEI-BEEI database; none of us, Omnisient, nor the firm ever see the original un-hashed ID numbers.[7]

Harambee also provided other non-identifying information on participants from their applications platform, such as gender, race, age, and education level. The programme targeted young people, with a strong equity component, such that for example 65% of participants were women according to official reports. We match 121,449 out of the participants provided by Harambee, which is a 62% match rate and 45% of the total PYEI-BEEI participants.

1.3. Survey of participants

Since the retailer data are limited to formal sector purchases, we also collaborated with Harambee to run a digital financial diary survey of participants to capture broader spending patterns. Participants could log expenses and income on a Whatsapp channel, including basic descriptions of the type of expense. We selected survey questions to broadly follow the national spending surveys (see below) for comparison. Whatsapp prompts were sent to 31,250 participants with phone numbers on Harambee's database, only 2,279 of whom logged entries. The response uptake was therefore low, and most respondents only logged a few entries. Since we ideally wanted to capture full monthly spending, we restricted the analysis sample to those who spent 50-200% of their reported income (490 participants)

[7]The Omnisient technology also ensures removal of any other personally identifiable information such as phone numbers or addresses in case they are inadvertently left in the uploaded data.

and who also spent at least ZAR3,000 in the month (205 participants). This final sample of 205 participants had logged 1,597 entries collectively, and about 60% of them were the sole earners in their households.

We then conducted a targeted follow-up survey after the programme, targeting about 1,000 of the participants who responded most and providing a small airtime incentive for participation. 158 of these participants responded. However, only 36 of these participants met the restriction above (i.e. part of the 205 participants), meaning that the "panel" sample size of this survey was very small.

1.4. Other data

National spending surveys. We use nationally representative income and expenditure data from the national statistics agency, from 2011 (Income and Expenditure Survey) and 2014 (Living Conditions Survey). These record detailed records of consumption expenditure at the household level, which we use to compare the external validity of our sample of PYEI-BEEI participants. We restrict to household per capita expenditure deciles 2-7 for broad comparability to participants.

Firm-level administrative data. To help understand firm responses to revenue shocks, we accessed anonymized firm balance sheet data from tax records housed in the South African National Treasury's Secure Data Facility in Pretoria. These tax records are retrieved from the South African Revenue Service as facilitated by UNU-WIDER, and have been used in a large number of past and ongoing research projects (see Ebrahim et al. 2017; Pieterse et al. 2018 for a description of the data). We primarily use firm data on annual sales, employees, earnings and industry to estimate the elasticity of employment and earnings to sales by industry.

Input-Output data. As a measure of formal sector value chain linkages, we use the Social Accounting Matrix produced by the national statistics agency (van Seventer and Davies 2023). This is a correspondence table between products and industries, with sales amounts in each cell reflecting the distribution of revenue.

2. Motivating descriptives

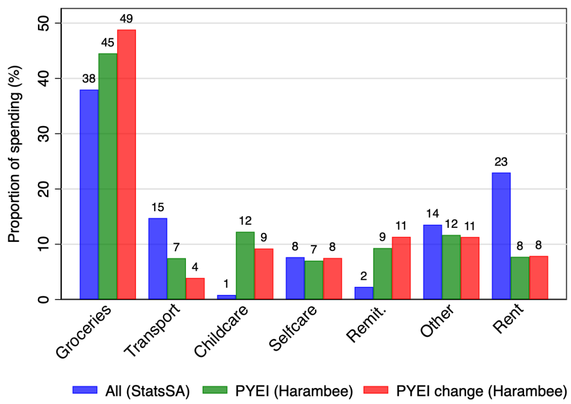
2.1. Survey of participant spending

As a first look at the spending patterns of PYEI-BEEI participants, Figure 1 shows results from the participant survey described in the previous section. We present three sets of results: a baseline corresponding to the average spending from national surveys, labeled "All (StatsSA)" in blue; the participant spending proportion in levels during the programme, labeled "PYEI (Harambee)" in green; and the participant spending proportion corresponding to the change in spending between the first and follow-up surveys, labeled "PYEI change (Harambee)" in red. The proportions are similar across these three sets of results.

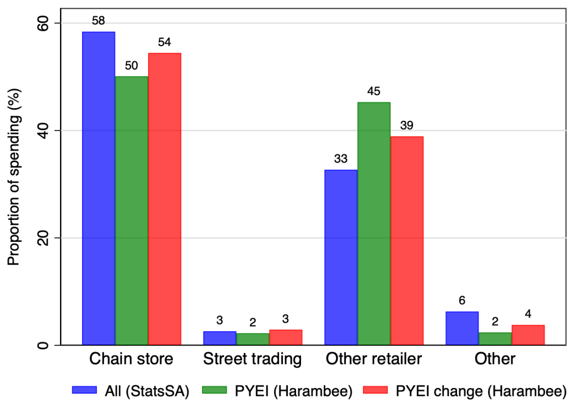
Panel A shows that by far the largest category of spending is on groceries, though with substantial spending also on rent, transport and self-care. Compared to the national survey, the participant survey spending was higher on groceries and childcare, and lower on transport (possibly due to nearby employment) and rent (possibly because these are youth part of larger households). The higher spending on childcare aligns with the PYEI-BEEI participant demographic, which is youth of child-bearing age and strongly gender progressive (two-thirds of participants were women); prior studies suggest women are more likely to spend on their children than men (e.g. Bertrand et al. 2003; Anderson and Eswaran 2009). These proportions by spending category are very similar using levels or changes in participant spending.

Panel B shows that over half of groceries expenditure is at chain stores, a substantial amount at other retailers (which includes permanent but informal stores), and a small remainder at temporary street traders and others. The proportions for all expenditure are lower at chain stores for participants (33%) but higher in the national survey (65%).

Figure 1: **Expenditure by participants: Survey evidence**



(a) Categories



(b) Store type

Notes. Panel A shows spending by category, where other includes all spending not classified in other categories. Panel B is restricted to food expenditure, where "other retailer" includes both formal and informal stores (not chain). "All (StatsSA)" refers to national consumption surveys; for panel A this is the Living Conditions Survey of 2014, and for panel B this is the Income and Expenditure Survey of 2011. "PYEI" refers to the participant survey carried out by Harambee, where the first is based on the cross-section and the second ("change") takes the proportions of changes in income between the two surveys. See data notes above for cleaning of the analysis sample.

Overall, the participant survey suggests that PYEI-BEEI payments were spent as we would expect from national surveys. It is particularly reassuring that these patterns are similar whether we look at the cross-section of participant spending or at changes. However, these participant survey responses are limited by very low response rates and self-selection into survey participation (e.g. selection on those who spent more), small sample sizes with potentially partial responses (as described in section 1) and also lack of a before-treatment baseline or control sample (though we do see post-treatment for a small number). Our preferred estimates therefore rely on anonymized data from the firm, which we describe next.

2.2. Spending at the firm

The firm is a supermarket chain store group with over 1500 stores across the country.[8] Out of the participant list provided by Harambee, 62% of participants are matched in the firm data at least once, using the secure and anonymized protocol described above.

Using the anonymized data from the firm, Table 1 provides descriptive evidence on the average sales per month for the 16 month window surrounding the first paycheck paid to participants at the end of November 2021, in aggregate and for the largest spending categories which allow causal inference (see Section 3). There is significant month-by-month volatility in the data if one aggregates sales by calendar month, which seems to be caused by concentrated spending in the month-end weekends, and that calendar months sometimes contain zero or two such weekends. We therefore define an alternative month aggregation which defines each month from the 16th of the calendar month to the 15th of the next calendar month. This substantially reduces volatility in the monthly series of sales. The columns show average spending before and after, for participants as well as the non-participant random sample of customers. We define the pre-period as the 6 months before the first pay-check (16 May - 15 November 2021) and the post-period as the 10 months of programme pay (16 November 2021 - 15 September 2022). For these descriptive statistics we drop the first month (16 November-15 December 2021) as it represents a partially treated month. Table 1 shows unconditional monthly means – that is total spending per individual in each period divided by the number of months in that period.

Recall that the PYEI-BEEI gave preference towards hiring applicants living close to schools in the programme, which were more likely from poorer communities, and so therefore participants were from relatively lower income households. As expected then, the average spending of the participants is about ZAR160 lower per month before the programme compared to the random sample of customers. Average participant spending is much higher during the programme, though the control sample spending is higher too which highlights the importance of a differences-in-differences strategy.

Table 1: Average spending per month, by treatment group & period

	Control		Treated	
	Pre	Post	Pre	Post
Aggregate	483.40	550.36	326.99	436.79
Product cateogries				
Groceries	165.84	178.07	121.69	153.64
Frozen Perishables	45.13	51.33	37.27	48.92
Refrig. Perishables	57.60	64.65	31.02	42.39
Toiletries	30.80	33.71	23.68	31.12
Fruit, Veg. & Flowers	30.73	35.98	15.18	20.66
Butchery	26.21	29.31	13.65	18.89
Other	127.08	157.31	84.51	121.18

Notes: Table shows individual mean spending per month, by treatment group and period, for sales in aggregate as well as selected spending departments. Means are unconditional (each individual's total spending in the period divided by number of months in the period), with each individual equally weighted.

[8]We do not provide the exact number of stores in our data in order to avoid identifying the firm, but also note that our data may not include all of its stores because we are provided a data sample, not the full universe of records.

In terms of spending categories, groceries are a large part of participant spending (37%) in the pre-period, and other similar food-related categories make up the bulk of the main spending groups. The percentage of groceries in total spending is marginally smaller (35%) in the post-period, making the treated group more similar to the control in this respect (34% in the pre-period). While the percentage of spending which is groceries in Table 1 seems lower than the percentage in the participant survey (Figure 1; 45%), this is because categories such as refrigerated and frozen perishables, fruit and vegetables and butchery items (among others) would be lumped together as groceries in the participant survey. In fact, grocery-type expenditures are significantly over-weighted in the firm data – 65% of participant spending in the post period even if one only looks at the top 5 grocery categories – and Table 2 in the next section shows that food spending is 5 times larger than non-food spending at the store. This has implications for the representativeness of the spending elasticities at the firm, given that food items are likely to be necessities with low income elasticities of demand.

3. Expenditure effects at a major retailer

3.1. Empirical strategy

In estimating the direct effects of the programme on the firm's sales by participants, we would like to incorporate both intensive (magnitude of sales) and extensive (frequency of shopping) margins. To this end, we begin by transforming the firm sales data into a balanced panel, such that $sales_{ijt}$ for customer i and month t (with store j imputed as individual i's modal store, weighted by sales) is equal to zero when originally missing and equal to the original sales value otherwise.

We then implement an event-study difference-in-differences empirical specification with an exponential mean function, estimated using a Poisson pseudo maximum likelihood estimator (Wooldridge (2021)):

$$\mathbb{E}[sales_{ijt}] = \exp\left(\sum_{s=-6}^{9} \delta_s \times \mathbb{1}\{s=t\} \times treat_i + \alpha_i + \gamma_{a(j)b(j)t}\right) \tag{1}$$

Equation 1 includes fixed effects for each customer α_i, as well as for the geographic area (a, the main place) by store banner (b, the type of store) interacted with event-time $\gamma_{a(j)b(j)t}$. Event-time is in months, centred around the first paycheck received at the end of November 2021, and ranges from -6 to 9.[9] As described above, the sample consists of anonymized matched participants (with $treat = 1$) and a random sample of anonymized non-participants in the customer data (with $treat = 0$). δ_s thus identifies the event-time treatment coefficients, with δ_s for $s < 0$ identifying pre-treatment effects, and for $s > 0$ identifying the treatment effects of interest for the programme.

[9] Phase 1 ended just before $t = -6$, and we exclude this to avoid possible contamination. The programme lasts until $t = 9$; in additional analysis we evaluate post-programme effects. Note that we use same 16th-15th month definition as is discussed in Section 2, for the same reason.

In our main results below, we also estimate the average pre-treatment and treatment coefficients. We replace the monthly event-time treatment coefficients δ_s in the term $\sum_{s=-6}^{9} \delta_s \times \mathbb{1}\{s=t\} \times treat_i$ in equation 1 with pre- and post-event indicators δ_{pre} (for $s<0$) and δ_{post} (for $s>0$). We exclude $t=0$ from the post-treatment effect δ_{post} since this month is only partially treated.

We estimate the above equation 1 for aggregate spending as well as separately for each of the 25 department categories. We also separately estimate the aggregate intensive and extensive margin effects by OLS using the equivalent OLS specification to equation 1, with the outcomes y_{ijt} as the magnitude of log sales (intensive margin) or an indicator for non-zero $sales_{ijt}$ (extensive margin):

$$y_{ijt} = \sum_{s=-6}^{9} \delta_s \times \mathbb{1}\{s=t\} \times treat_i + \alpha_i + \gamma_{a(j)b(j)t} + \epsilon_{ijt} \tag{2}$$

This OLS specification serves as robustness on the Poisson specification.

Our identifying assumption is the familiar parallel trends assumption: loosely, we assume that changes in average monthly spending among participants and non-participants shopping in the same store area and banner would have been identical in the absence of the PYEI-BEEI programme. As usual, a test for the plausibility of this assumption is that the pre-treatment coefficients δ_s for $s<0$ are close to zero and do not have a trend.

As robustness on this identifying assumption, we also estimate a specification with store fixed effects, in which we are comparing changes in average monthly spending among participants and non-participants shopping at the same exact store. Finally, we estimate treatment coefficients δ_s over a longer time frame $s \in [-6, 14]$ to test for any post-programme treatment effects.

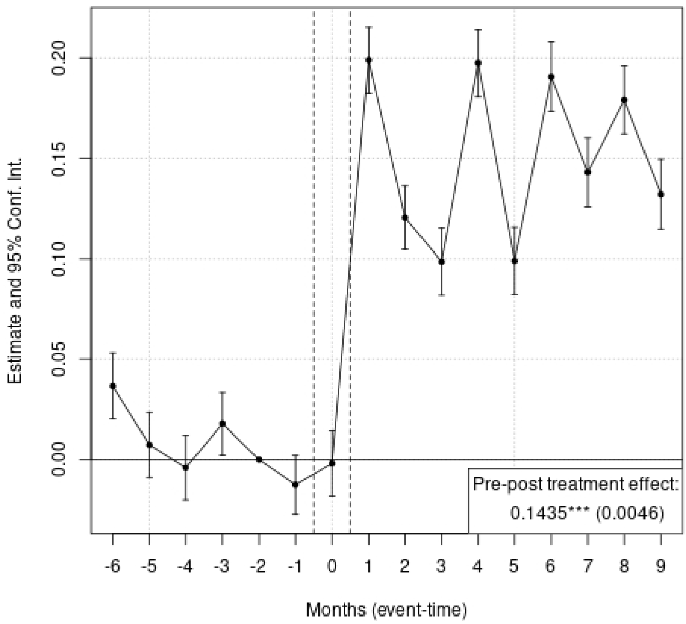
3.2. Results

Figure 2 presents the aggregate results. The pre-event coefficients are reassuringly close to zero prior to the event with no trend, and there is a large jump between the partially treated month $t=0$ and the first fully treat month $t=1$. This suggests that the control group is a good proxy for the treated group in the absence of treatment. The average treatment coefficient is 15.4% and is highly statistically significant.[10] We view this as clear evidence of a large treatment effect on aggregate participant sales from the programme.

Figure 3 shows effects for the largest 6 departments with clean pre-trends.[11] They are estimated exactly as in Figure 2, except only expenditure in the relevant department is considered. One important difference is to note that individuals who never purchase goods from a particular department in our time range are necessarily excluded from the estimation sample as there is no variation in the outcome. This is reflected in varying sample sizes for each departmental regression, shown in Table 2.

[10] The log coefficient is 0.1435, which is converted to percentage terms as $exp(0.1435)-1$.
[11] The only consequential exclusion is the Wine & Liquor department; see Appendix Figure A.5.

Figure 2: **PYEI-BEEI direct effects on the firm's sales, aggregate**



Notes: Figure shows direct treatment effects on aggregate spending at the firm of the PYEI-BEEI programme. Monthly event-study treatment effects are shown, with the program partially starting in period 0. Because period 0 is partially treated, it is excluded from calculation of the aggregate pre-post difference in differences treatment effect shown in the bottom right corner. The event study is specified with an exponential mean function and estimated using a Poisson pseudo maximum likelihood estimator. Treatment effects represent the combined effect of intensive margin and extensive margin responses. Standard errors are clustered at the individual level.

The treatment effects in Figure 3 range from 13.8% (log coefficient 0.1293; Frozen Perishables) to 20.8% (log coefficient 0.1890; Butchery). The range of treatment effects is much larger in Table 2, where there is an increase in expenditure on Electronics and Media of 63% (log coefficient 0.4888) and on Home and Small Appliances of 50.6% (log coefficient 0.4096). It is unsurprising that Figure 3 shows smaller treatment effects because it focuses on the biggest expenditure categories, which is made up food items, demand for which is likely to be less income elastic. Indeed 2 shows that treatment effects on non-food items are substantially higher than for food items, and that expenditure on food items is 5 times larger than expenditure on non-food items at the store.

The high weighting of food items in participants' expenditure baskets at the firm is likely an important explanation for what seems to be a relatively small treatment effect when one considers the size of the treatment (minimum wage employment) against pre-period spending levels at the firm. We do not observe participants' full spending; we only observe what they purchase at the firm, and this spending is over-weighted towards income inelastic necessities. The other factor to keep in mind is that we do not observe all of participants' income either. Participants may be substituting away from household income sources or other kinds of (potentially informal) employment when they join the program, so that the proportionate income change due to the programme, especially at the household level where consumption occurs, may be less dramatic than it seems at first glance.

While Figure 3 and Table 2 only include departments with clean or downward-sloping (thus conservatively-biased) pre-trends, the full range of results is shown in Appendix Figures A.2-A.5.

3.3. Robustness

Figure 4 shows the OLS estimates for the aggregate treatment effects. The pattern is similarly re-assuring, in that the pre-period coefficients are close to zero and there is a sharp jump after the event. The average treatment effect is broadly similar to figure 2, and the similarities show robustness to using an OLS or Poisson specifications.[12]

Appendix Figure A.6 shows that the main results are robust to interacting the time dummy with store fixed effects rather than the banner-location interaction used in equation 1; the results are almost identical to those in Figure 2.

Figure 5 shows results for the same event study as in 2 and described in equation 1, but extends the post-period to the limit of our data. Aggregate pre-post difference-in-differences effects are estimated separately for the actual period of the programme payments (months 1-9 in event time) and after the programme payments end (months 11-14 in event time). In the same way that we exclude period 0 from the pre-post differences in Figure 2, in Figure 5 we also exclude period 10, which represents a month where payments from the programme are only partially still received.

There is a consistent post-treatment effect of 3.8% on aggregate sales (0.0376 log coefficient), which may indicate consumption smoothing from the participants and/or increased employability from the experience of formal work during the programme.

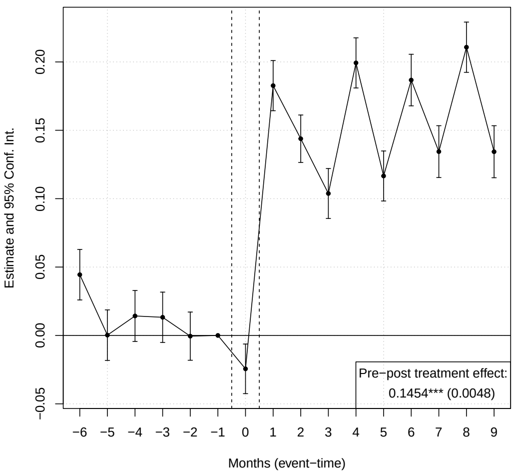
4. Division into payments to labour and capital

Thus far we have focused on the direct effects of the PYEI-BEEI payments on a major retailer's sales, based on the estimated treatment effects on participant purchases at that retailer. While we view these effects as well-identified, an assessment of the initial stimulus total effects should consider the actual effects on wagebills (through the increase in sales), and also include the effect of recirculation in the economy. In this section, we estimate the effect on labour and capital incomes via expenditure at the firm; in the next section we discuss the effect from all participant spending at any store. We use the reduced form effects along with supplementary estimates and sources of data which embed some strong assumptions about the structure of the economy and responses to spending shocks. Because these

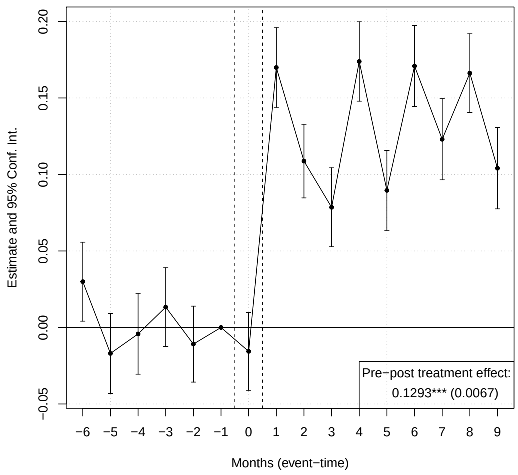
[12] Specifically, if $E[sales] = pr(shop = 1)E[sales|shop = 1]$, where "shop" denotes observed sales data for a given month, then $\frac{\partial ln(E[sales])}{\partial z} \approx \frac{\partial ln(pr(shop=1))}{\partial z} + \frac{\partial ln(E[sales|shop=1])}{\partial z}$, where the derivatives indicate marginal effects from the regression. The latter is just the coefficient from the intensive margin regression (0.1205). The former is the coefficient from the intensive margin regression, multiplied by 1 over the probability of shopping (0.42 or the inverse 2.4), which is equal to 0.54. The approximate OLS overall effect is therefore 0.174, which is higher than the Poisson estimate of 0.1435 but not by much.

Figure 3: PYEI-BEEI direct effects on the firm's sales, by department

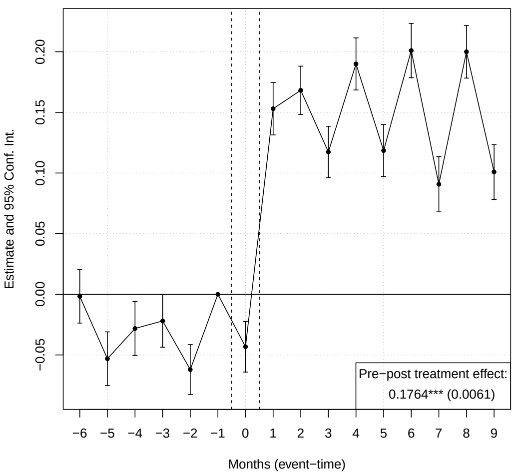
(a) Groceries



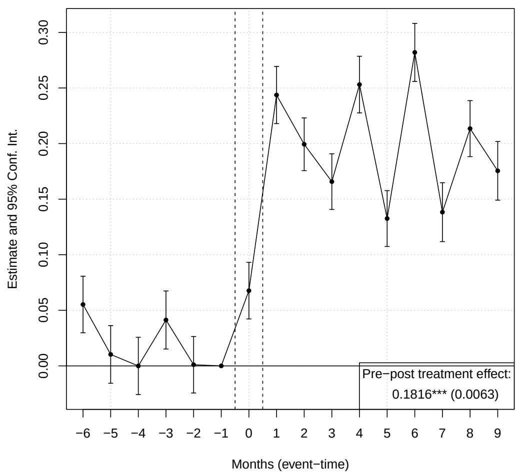
(b) Frozen Perishables



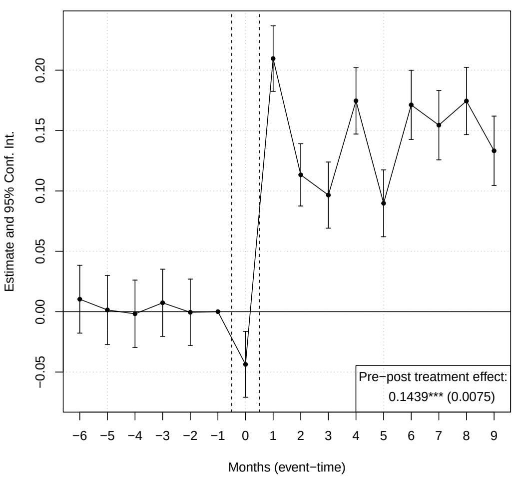
(c) Refrig. Perishables



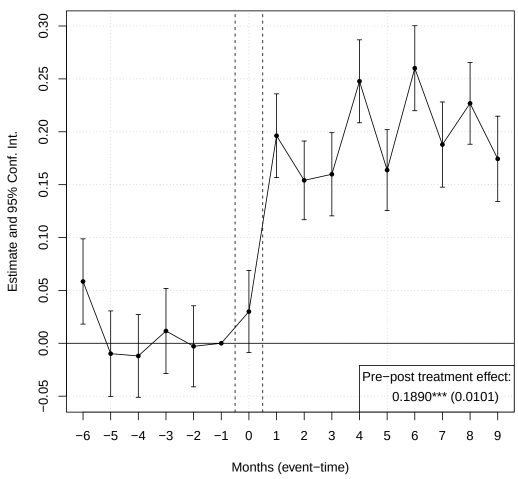
(d) Toiletries



(e) Fruit, Veg. & Flowers



(f) Butchery



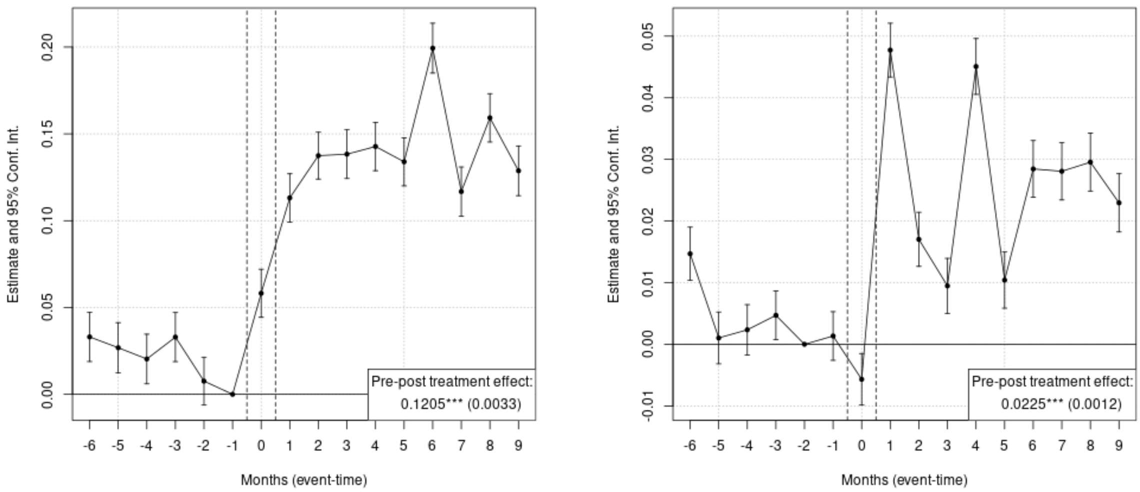
Notes: Figures are analogous to those of Figure 2, but estimated separately by department.

Table 2: Treatment effects by department

Department	**Treatment effect**	**Treated group pre-period spending**		
		Mean spend	*Indiv.*	*Tot. spend (000s)*
	Panel (a): Food			
Bakery	0.1393*** (0.0070)	13.38	89587	1199
Butchery	0.1890*** (0.0101)	21.17	68155	1443
Deli	0.1262*** (0.0130)	5.01	52885	265
Eggs	0.1507*** (0.0104)	8.09	49958	404
Frozen Perishables	0.1293*** (0.0067)	46.83	84134	3940
Fruit, Veg. & Flowers	0.1439*** (0.0075)	19.02	84383	1605
Groceries	0.1454*** (0.0048)	125.06	102868	12864
Refrig. Perishables	0.1764*** (0.0061)	34.45	95189	3279
Aggregate	**0.1489**			**25000**
	Panel (b): Non-food			
Basic Softs	0.3128*** (0.0315)	2.63	13548	36
Electronics & Media	0.4888*** (0.1467)	6.37	992	6
Footwear & Apparel	0.1045*** (0.0259)	5.76	21840	126
Hard Goods	0.2390*** (0.0190)	7.81	33608	263
Healthcare & Medicine	0.1932*** (0.0138)	7.31	45422	332
Home & Small Appliances	0.4096*** (0.0217)	17.04	28514	486
Household & Cleaning	0.2430*** (0.0195)	2.93	36017	106
Kitchenware	0.3331*** (0.0179)	6.22	49011	305
Pet Food & Health	0.1291*** (0.0339)	12.28	6578	81
Stationery & Luggage	0.2054*** (0.0172)	4.20	52981	222
Toiletries	0.1816*** (0.0063)	27.62	90653	2503
Toys & Festive	0.2412*** (0.0240)	5.74	27320	157
Virtual Airtime	0.0008 (0.0201)	3.88	30449	118
Aggregate	**0.2171**			**4740**

Notes: Table shows pre-post difference-in-differences treatment effects and spending characteristics by department. Mean spend is the unconditional mean per month in ZAR, Tot. spend is the total per month in ZAR, and Indiv. is the count of distinct (anonymized) individuals; all for the treated group in the pre-period. Aggregate elasticities are the mean of department-specific elasticities weighted by department total sales for the relevant super categories of Food vs Non-food items.

Figure 4: **Event study of PYEI-BEEI participant sales effects at the firm: OLS**



(a) Intensive Margin

(b) Extensive Margin

Notes: Figures are analogous to the event study of Figure 2, but with the (more typical) linear specification of equation 2 and estimated via OLS. Panel (a) shows the event study for the outcome of log sales, while Panel (b) shows the event study for a dummy outcome indicating whether the individual spent at the firm in the relevant month.

supplementary data and estimates are not from a credible quasi-experiment with clear causal identification, we view these exercises, which build on our core results, as more tentative and speculative.

4.1. Direct effects on the firm's wagebill

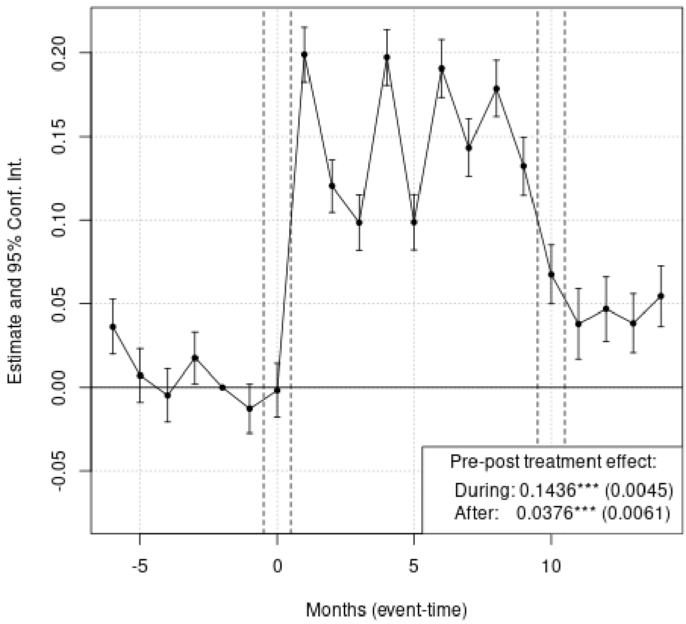
We use the firm-level administrative data to estimate the average elasticity of the worker wagebill to sales across the formal sector. We also estimate this elasticity for the formal retail sector, to back out the direct effects of the PYEI-BEEI payments relevant to the firm's wagebill.

In terms of estimation strategy, we follow Lamadon et al. (2022) (LMS) to estimate firm responses to "shocks" to sales (demand shocks), in a context where we do not have quasi-experimental variation from a natural experiment. Because Lamadon et al. (2022) are interested in effects on wages and wish to abstract from composition effects occasioned by new hires, they restrict their examination of wage effects to wages of "stayers", that is workers who remain employed at a firm for a number of consecutive years. We do not need such a restriction, and the only important sample restriction we make is to restrict our analysis to firms which are continuously in the tax data with positive employment for 6 consecutive years.[13]

We then create "events" for each 6-year spell by defining the pre-period as the first three

[13]We also drop firms which do not have provincial or 1-digit industry values.

Figure 5: **Event study of PYEI-BEEI participant sales effects at the firm: longer time period**



Notes: Event study is analagous to that of Figure 2 except the post-period is extended to event month 14, and the post period is split into a "during programme" period (event months 1-9) and "after programme" period (event months 11-14). Event month 10 is excluded from the pre-post aggregate treatment effects because, like event month 0, participants receive payments for part of the month.

years, and the post-period as the last 3 years, with treatment being defined as an increase in log sales between periods -1 and 0 greater than the (employment-weighted) median. We then examine the effect of this treatment on (log) sales and log wagebill.

We stack the events and then estimate the following for firm j and event-year t, for event e, for outcomes y_{jte} as log sales and log wagebill, separately for each industry:

$$y_{jte} = \sum_{s=-3}^{2} \delta_s \times \mathbb{1}\{s = t\} \times treat_j + \gamma_{je} + \tau_{tep(j)} + \epsilon_{jte} \tag{3}$$

The fixed effect $\tau_tep(j)$ is a time-specific dummy for each province p.[14]

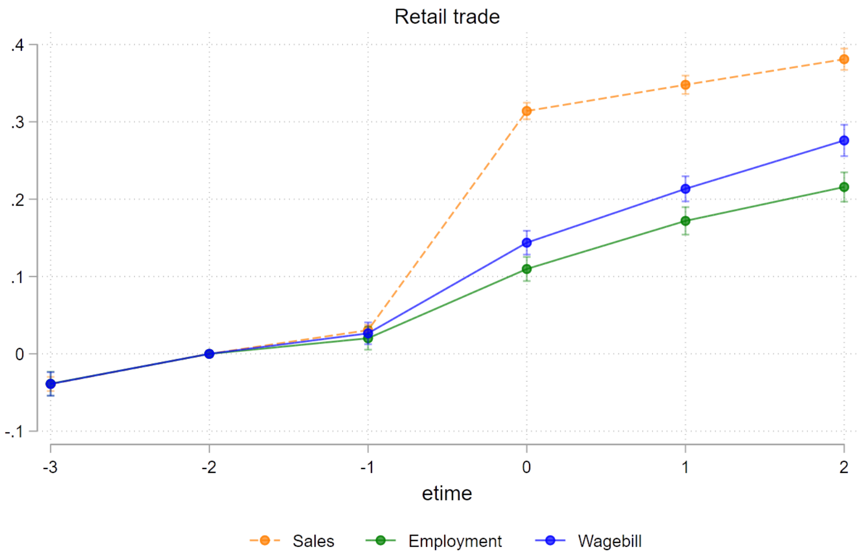
The identifying assumption is that above-median changes in sales (the treatment) are not correlated with ϵ_{jt}. Although as usual the pre-treatment effect δ_s for $s = -3$ provides an implicit test of the parallel trends assumption, we must still assume (as Lamadon et al. (2022) do) that there are no confounding shocks coincident with the above-median sales increase, for example any simultaneous firm supply shocks.[15]

[14]In the original Lamadon et al. (2022) paper area and industry are interacted to create time-varying local labour market fixed effects. Because we estimate the regressions separately by industry we use only area-specific time varying fixed effects.

[15]Following Lamadon et al. (2022), δ_s for $s = -1$ or $s = 0$ may reflect measurement error or dynamic adjustments – some mean reversion in particular is very likely given the way the treatment is defined – so we ignore these periods.

Figure 6 shows the results. The pre-period coefficients do have a slight pre-trend but are relatively close to zero, and show a sharp increase at the event-date as expected, with sustained, stable effects after the shock. On average, a 39% increase in sales led to a 28% increase in wagebill, which is an elasticity of approximately 0.72.

Figure 6: **Elasticity of wagebill to sales**



Notes: Figure shows LMS event-study for the retail trade sector, a stacked event study where treatment is defined as an above median increase in sales between periods -1 and 0 (which are tax years). See Equation 3. Standard errors are clustered at the province-event level.

To back out the direct effect on the firm's wagebill, note that the elasticity is equal to $\varepsilon^{wagebill}_{sales} = \frac{\Delta wagebill}{\Delta sales}\frac{sales}{wagebill}$, so then $\Delta wagebill = \Delta sales\frac{wagebill}{sales}\varepsilon^{wagebill}_{sales}$ where the fraction is the wagebill share of sales. To get the change in sales, $\Delta sales$, we multiply the reduced form treatment effect of 15.4% (or 0.1435 log-points) by the average pre-period participant spending per month ZAR 327) to get an estimated magnitude of the marginal effect, multiply this by the number of participants who shopped at the firm to get a total increase in monthly spending (ZAR 7.35 million).[16] We then use the wagebill-sales elasticity estimated above, and the retail industry wagebill share of sales of 20% estimated from the firm-level administrative data.[17] The resulting estimated increase in monthly wagebill at the firm is ZAR 1.06 million. Using the same method, along with our estimated elasticity of capital income to sales of 0.84 and capital income share of 0.15, the resulting estimated increase in monthly capital income at the firm is ZAR 0.93 million.

As a benchmark, the average monthly salary of workers is ZAR 10,500. Given that figure 6 shows most of the increase in wagebill came through an increase in employment, the direct effect on the firm's wagebill could have been up to 100 additional jobs. A few caveats are worth bearing in mind. Aside from the credibility of the estimated elasticity of wagebill to sales (which follows the literature), the PYEI-BEEI stimulus may engender different responses

[16] As discussed in section 1, we observe 105,719 anonymized participants in the firm data. We scale this up to account for participants not matched in the data, i.e. a factor of 270,073 over 195,540. This assumes that non-matched participants were just as likely to shop at the firm, and had similar reduced form effects.

[17] Note that the this share includes inventory, and so is not the more familiar labour share out of value added.

if employers perceive it as temporary: though this programme lasted nearly a full year, with new cohorts at the same schools both before and after, the pandemic context of the programme may have made retailers more cautious. Note we also have not assessed employment composition here, for example if retailers shift to differently paid new hires.

4.2. Effects on jobs and incomes via the firm's value chain

We next use input-output data to extrapolate effects on the firm's value chain supplier sales, and, similarly to above, the suppliers' wagebill. This accounts for re-circulation – that suppliers to the firm will also experience a sales effect with concomitant impacts on labour and capital incomes and expenditure on inputs – and therefore allows us to estimate the effects of the PYEI-BEEI payments via the firm on incomes in the value chain. As noted in in the introduction, this does not account for the effects of increased expenditure from workers and owners of capital whose incomes increase due to the effects of the programme via the value chain; this is only the "first round" of a broader multiplier mechanism, and may be understood as characterising the "income side" of the participants expenditures.

Using the industry and product linkages provided by van Seventer and Davies (2023) in the form of a national Social Accounting Matrix, we map the firm's commodity categories to their list of national commodities, which are in turn linked to national industries. We then map these industries back to industries in the firm-level administrative data to retrieve their wagebill-sales and capital income-sales elasticities as estimated above in equation 3 for each separate one-digit industry.[18] With these linkages in place, we can input the commodity-specific treatment effects from section 3 which correspond to the increase in input demands from the firm, and output the sales effects on the firm's suppliers (from the industry-product linkages in the Social Accounting Matrix) as well as their respective wagebill and capital income responses. We can do the same in turn for these suppliers to account for their respective increased input demands from their suppliers, and so on, such that ultimately the input-output table matrix fully apportions the spending increase into incomes to domestic factors of production via the commodity value chain (imports, which are accounted for in the input-output matrix, enter as "leakages" which reduce the income effect of the sales increase).

The key sets of estimates we rely on in this exercise are the reduced-form treatment effects, the estimated wagebill-sales and capital income-sales elasticities, and the national input-output linkages. We have already discussed the credibility of the former two sets of estimates, where the key test is the pre-period coefficients. In this exercise, since we are disaggregating by commodity and industry, some estimated retailer treatment effects fail the pre-period test and in these cases we replace them with the aggregate estimated effect.[19] The third key set of estimates we rely on, the national input-output linkages, is

[18]We have to perform some manual matching between the firm's commodities (25 categories) and the national commodities (102 categories), as well as between national industries (65 categories) and our estimated broad industry wagebill-sales elasticities (9 categories). When matching categories, we weight by the relative importance of each sector.

[19]Specifically, we replace 3 of the 25 estimates for categories: "Baby", "Cigarettes and Tobacco", "Wet Fish" and "Wine and Liquor". A few other categories have negative pre-trends, but we keep these estimates as this implies a conservative estimate (the implied counterfactual treatment effect is actually larger). See Figure 2 and Appendix

in our view much more speculative. Aside from assuming our retailer's linkages can be approximated by the retail industry's linkages, perhaps the most important concern is that these linkages representing pre-existing static average flows, which may not reflect dynamic marginal responses. A centrally important reason marginal responses may differ from average responses is due to supply and resource constraints, which may lead to price increases rather than a scaling up of production; note that such price increases will primarily change the proportions of income allocated to labour versus capital, rather than the sum of the two. Given these strong assumptions, we view this subsection as more tentative than the above analyses.

Table 3 presents the results from this value chain exercise, by industry and for the aggregate. The first column reports the increases in retailer demands due to the direct effects of the PYEI-BEEI participant purchases. These retailer demands focus only on the firm's effects through intermediate goods and thus excludes other direct effects such as on its wagebill. Specifically, the total increase in sales at the firm due to the PYEI-BEEI payments is ZAR8 million per month, and 47% or ZAR3.8 million of this goes to intermediate goods.[20]

Table 3: **Recirculation effects due to PYEI-BEEI direct effects at the firm**

	(1) Retailer Demands	(2) Sales Recirculation	(3) Wagebill Share	(4) Wagebill Elasticity	(5) Wagebill Effect	(6) Capital Effect
Industry						
Agriculture	65	740	.15	.64	71	230
Mining	0	190	.72	1	140	29
Manufacturing	3600	5200	.19	.63	600	790
Utilities	0	170	1.3	.77	170	27
Construction	0	160	.21	.75	25	24
Trade	0	1800	.12	.93	200	230
Transport	120	920	.46	.4	170	220
FIRE	35	1000	.43	.63	270	250
CSP	1	110	.37	.68	28	26
Aggregate	3803	10257	.25	.68	1674	1818

Notes: The firm demands, sales recirculation, and wagebill and capital income effects are reported in thousands of ZAR. The firm demands focus on implications for the retailer's value chain through intermediate goods, i.e. excludes the wagebill and capital income components of the firm's income received through participant spending. Sales recirculation is the cumulative increase in sales due to the firm demands, and includes payments for intermediate goods.

As expected, the vast majority of the increased retailer demands goes to manufacturing. However, the total effect on sales through the full value chain (column 2), which includes both the firm demands and recirculation effects (suppliers also demand additional inputs), is distributed much more evenly across the industries, with trade, transport, CSP and agriculture increasing sales substantially. The wagebill effect is the total sales effect multiplied by the labour share and wagebill-sales elasticity (as in subsection 4.1), yielding an aggregate effect on the wagebill of about ZAR 1.67 million. This is larger than the direct effect on

Figures A.2-A.5.

[20] This is slightly mismatched with the sales effect reported in subsection 4.1 of R7.35 million because here we use separate treatment effects by retailer product category, which do not average exactly to the average treatment effect.

the firm's wagebill, and shows that accounting for the value chain more than doubles the apportionment of incomes which goes to employment and wages. We perform a similar calculation to yield the capital income effect, i.e. the total sales effect (ZAR 10.2 million) times the capital share out of sales (on average 20%) times by the elasticity of capital income to sales (on average, 0.88), and this yields a total effect on capital of ZAR 1.82 million.

What does this imply about how the initial sales income is divided between capital and labour incomes? The recirculation effect of ZAR 10.3 million on sales contains double-counting due to inter-industry purchases. Domestic value added, on the other hand, is just the sum of all wagebill and capital income effects. The wagebill effects directly on the firm (ZAR 1.06 million) plus the recirculation effect (ZAR 1.67 million) are together equal to ZAR 2.73 million. The capital income effects directly on the firm (ZAR 0.93 million) plus the recirculation effect (ZAR 1.82 million) are together equal to ZAR 2.75 million, i.e. the wagebill and capital income components are very similar.

The total value added from this calculation is ZAR 5.5 million, which is 72% of the direct effect of ZAR 7.7 million expenditure at the firm (weighted average of estimates above). It is not surprising that this number is less than one, as leakages due to imports mean that not all of the sales at the firm are ultimately reflected fully in incomes of domestic workers and firms. In addition, we have combined reduced form elasticities of the wagebill and capital income to sales with the static estimates for the value chain, so part of the reason it is less than one takes into account dynamic responses.[21]

Conclusions

Implications for the PYEI-BEEI direct effects on labour and capital

What does the preceding analyses imply for the effects of the PYEI-BEEI payments on domestic labour and capital incomes? Section 4 used the direct sales effect on the firm to extrapolate income effects from spending *at the retail store*, but as our participant survey in figure 1 suggests, spending by participants on supermarket-related items at chain stores is only about 25% of their total spending.

A simple further extrapolation is to scale up the estimated factor income effects from subsection 4.2 due to the firm by the inverse of this fraction of spending on supermarket items at chain stores to account for this additional spending outside of retail stores. We also need to adjust for the fact that not all chain store spending is at our retailer, using the average spending at our retailer versus the average spend on chain store retailers in the participant survey; during treatment, these are ZAR437 and ZAR719 respectively, which actually suggests the firm captures a remarkably large proportion of participant chain store spending.[22] This scaling up exercise assumes that the estimates for retail are similar for other spending destinations; specifically the treatment effects on spending, the wagebill-sales and capital

[21]One should also note that the capital income component probably also includes a non-negligible income share which is actually transferred out of South Africa, as firms which are registered domestically and which report domestic income for tax purposes may still transfer a significant share of their profit to foreign owners.

[22]Recall participants are paid a wage of ZAR3,817.44 per month.

income-sales elasticities, and the input-output linkages. Essentially, this exercise entails asking what the total effects would be if all participant spending induced by the program followed the pattern of formal chain-store retail. We know this is a strong assumption since spending at the firm is disproportionately on food items with likely low sales to income elasticities, which biases the scaled up estimate below towards an underestimate of the true effect. Secondly, this assumes that the estimated fraction and level of spending on chain stores from the participant survey are credible. Thirdly, it assumes that the exercise in subsection 4.2 as a whole is credible; we reiterate that we have concerns about the strong assumptions underlying the input-output value chain analysis. Finally, these calculations do not account for any negative economic impact of taxes that are used to fund the programme.[23] Clearly this is a speculative exercise which requires ample caution in interpretation.

Given these caveats, we estimate that the direct effect in spending of participants on *any* purchase was about ZAR 53 million, or about 6.5 times the direct effect on the firm. This is about 5% of the monthly total participant payments of ZAR 1.03 billion. This is much smaller than expected, and is mostly due to the low estimated treatment effect relative to the large payments; as discussed in section 3, this may be related to a lower income elasticity of essential goods purchased from the supermarket retailer, as well as participants substituting away from other income sources. Note that if the treatment effect is in fact higher for other expenditure categories, then the direct effect of the programme would be higher too.

The corresponding total effect of the PYEI-BEEI payments using this approach, i.e. scaling up the combined effects on the firm and the firm's value chain, is ZAR 38 million on value added, and ZAR 19 million on wagebill.[24] Using an average monthly salary of workers as above of ZAR 10,500, this would imply about 1,800 additional jobs per month.

What portion of the factor income increase is likely to go to local incomes in the area? We focus on the direct effects on wagebills since effects through the value chain are likely produced elsewhere. Our participant survey suggests that about half of all spending was at formal sector stores, for which we can use the estimates from table 3; that is, half times by the direct effect (ZAR 53 million) times by the elasticity to of sales to wagebill 0.68 and labour share (0.25), or ZAR 4.5 million. The other half of participant direct effects were spent on non-formal stores, such as informal stores, and services such as childcare or transport. For these purchases, we can improve on the estimated wagebill effect above which relies on the formal sector. Data from the national survey of informal enterprises shows that the average labour share for informal enterprises is much higher, on average 49%. Using this labour share instead, but maintaining the same wagebill-sales elasticity of 0.68, we extrapolate that the wagebills at local enterprises increased by ZAR 8.8 million (half times 53 million times 0.68 times 0.49). Adding the formal and non-formal store wagebill effects together, this implies local incomes increased by about ZAR 13.3 million in addition to the actual PYEI-BEEI payments.

[23]The main tax change in the surrounding period of the programme was a *reduction* in the corporate tax rate of 1% applicable from 1 April 2022.

[24]The specific calculations are as follows. For the effect on value added, we multiply the direct effect (ZAR 53 million) by the sales to domestic factor income conversion (72%) implied in subsection 4.2. We then divide this amount according to the shares going to the wagebill and capital income components implied in subsection 4.2, i.e. 2.73/5.48 to wagebill and 2.75/5.48 to capital.

Implications for other government social spending: Social grants

The sales to domestic factor income conversion estimated in this paper for the large public employment programme, the PYEI-BEEI, implies that other public spending may have similar initial stimulus effects. One example of particular interest is the large amount spent on social grants by the South African government. In the 2022 National Budget, ZAR248.2 billion was collectively allocated to the Old Age Grant, Child Support Grant and "Other grants" (notably the COVID-19 Social Relief of Distress grant). These grants collectively reach nearly all households, are well-targeted towards lower-income people, and reduce poverty substantially (Bassier et al. (2021); Goldman et al. (2021)).

Given that Figure 1 looks similar for participants and the national survey, a very crude extrapolation is to use the estimates above on the social grant payments. We noted above that we estimated an increase in spending relative to programme cost of 5%, which implies an absolute spending effect of about ZAR 12.5 billion from the ZAR248.2 billion paid in the three major grants above. Using the sales to factor income conversion extrapolated in subsection 4.2 of 72%, the effect of these social grant payments is ZAR 9 billion on domestic factor incomes, of which about ZAR 4.5 billion goes to wagebills. Following the calculations above again, the implied boost to local incomes would be ZAR 3.1 billion per year in addition to the social grant payments. We stress again these estimates are based on numerous assumptions outlined in the previous subsection; additionally, we are assuming spending by participants and social grant claimants is similar.

Concluding thoughts

We investigate the domestic factor income effects of a large public employment programme in South Africa, the PYEI-BEEI. We focus on a cohort contracted for 10 months, consisting of about 270,000 youth (nearly 2% of national employment or 6% of youth employment) and paid at the national minimum wage. Tracking anonymized participant spending at one of the country's largest retailers, and using a differences in differences event study comparing against spending changes of a random sample of customers within the same local area, we find a sharp, large and sustained increase in spending of 15.4%. We use these reduced form estimates to extrapolate to factor income effects, firstly (as supplemented by elasticity estimates from national firm-level administrative data) on the firm's wagebill, and secondly (supplemented also by input-ouput tables) on the firm's suppliers. We then use financial diary responses from a participant survey to scale up these estimates account for the size of the programme as a whole, which imply that the programme spending translates to ZAR 38 million paid to domestic factors of production.

Strong caution is needed in interpreting these results however, and we have presented our estimates from most credible to least: the treatment effects on participant spending at the firm are well-identified, the estimates of the firm effects on the wage-bill less so, the estimates of the firm on the value chain makes much stronger assumptions, and the estimates on the overall factor income apportionment are speculative at best. Nevertheless, we include these more tentative extrapolations because we think it is important to shed some light on the important topic of stimulus effects of social programmes, and while far from estimating actual multipliers, we can suggestively characterize the expenditure and income effects in the "first round" of such a multiplier mechanism.

How do all of these estimates line up with what we would have expected? The treatment effects on participant retailer sales at first glance seem quite low; given the high rate of unemployment, we would expect the PYEI-BEEI payments to represent a large increase in income, so a 15% rise in supermarket spending is underwhelming. While well-identified, as discussed above these treatment effects are at a supermarket where the majority of purchases are necessities, which may be income inelastic; we presented some evidence in line with this hypothesis, with higher treatment effects for non-food purchases. Another substantial issue is that consumption expenditure likely reflects *household* demands, and we do not observe household incomes and expenditures. We cannot tell the extent to which the PYEI-BEEI income is supplementary to other incomes the participant already has access to, or how the participation in the programme may induce household dissolution and reformation, and substitution away from existing income sources. Other explanations include that the participants were substituted away from other opportunities; or that participants had a high savings rate perhaps because this was a temporary job. In terms of the extrapolated parts of the paper, we had few prior expectations about the distribution of factor incomes occasioned by social programme spending; as far as we understand these estimates are new in the South African context.

Overall, this paper characterises the "first stage" of a possible multiplier mechanism of the PYEI-BEEI payments. Of course for the PYEI-BEEI in particular there may be many other benefits such as additional educational value for students and increased employability via career experience for participants, which we do not address here. In general, domestic factor income effects may be important in evaluating the economic efficiency of government spending programmes, and may help reconcile some of the perceived trade off in public objectives between poverty reduction and private sector jobs.

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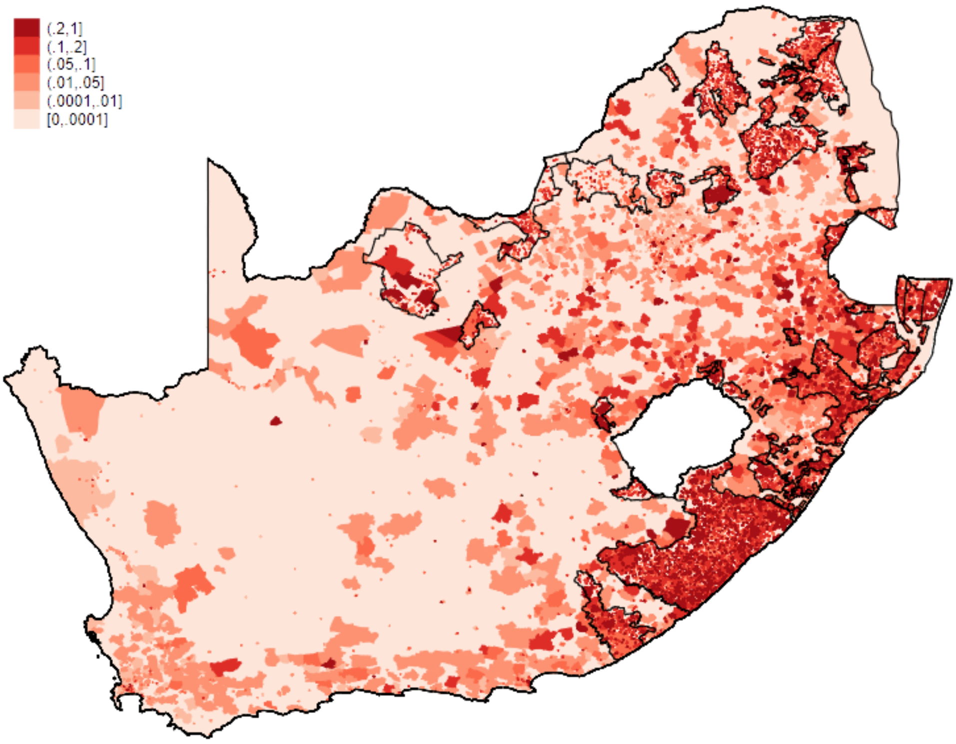
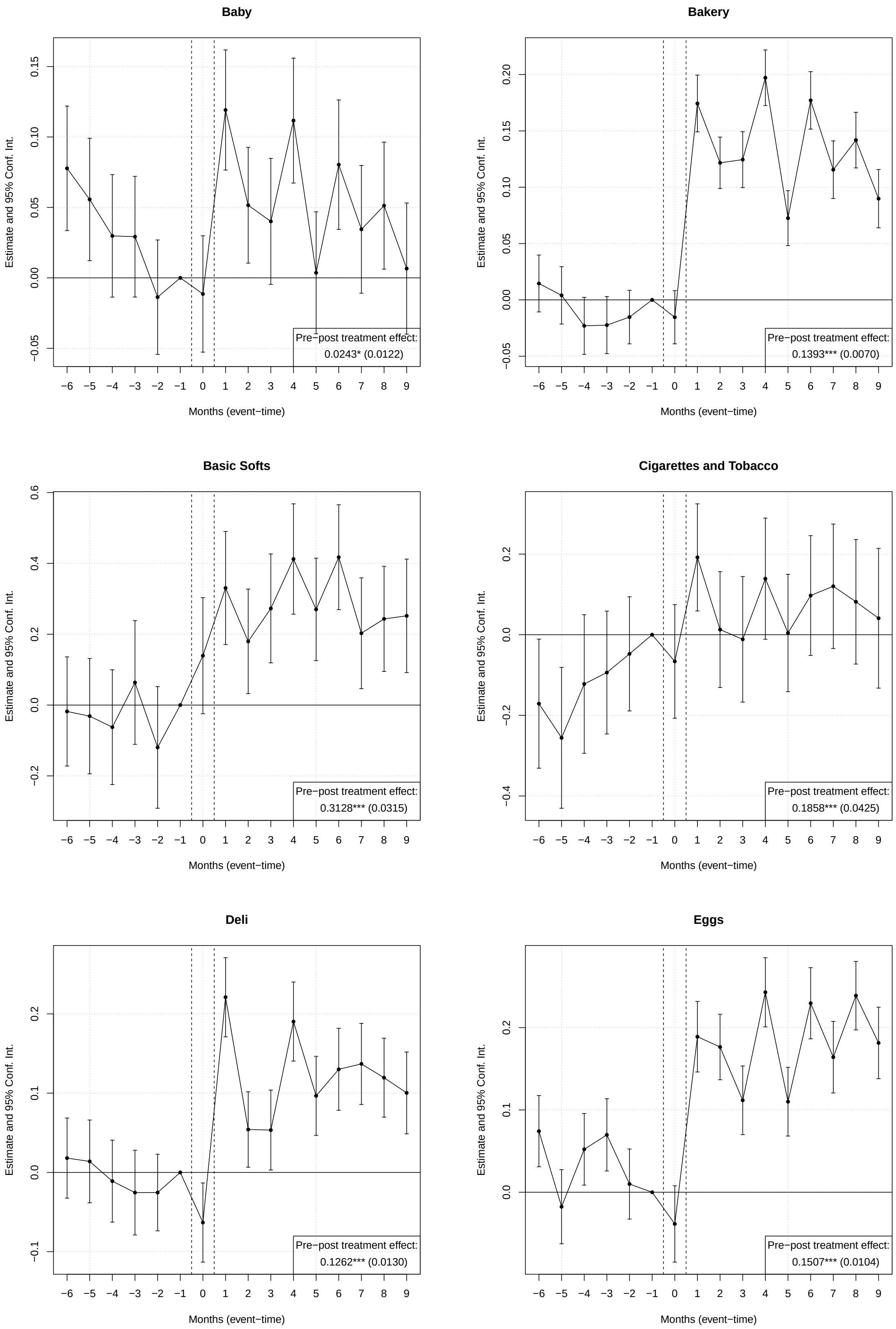
Appendix: Tables and figures

Figure A.1: **Map of PYEI-BEEI payments as a proportion of local area income**



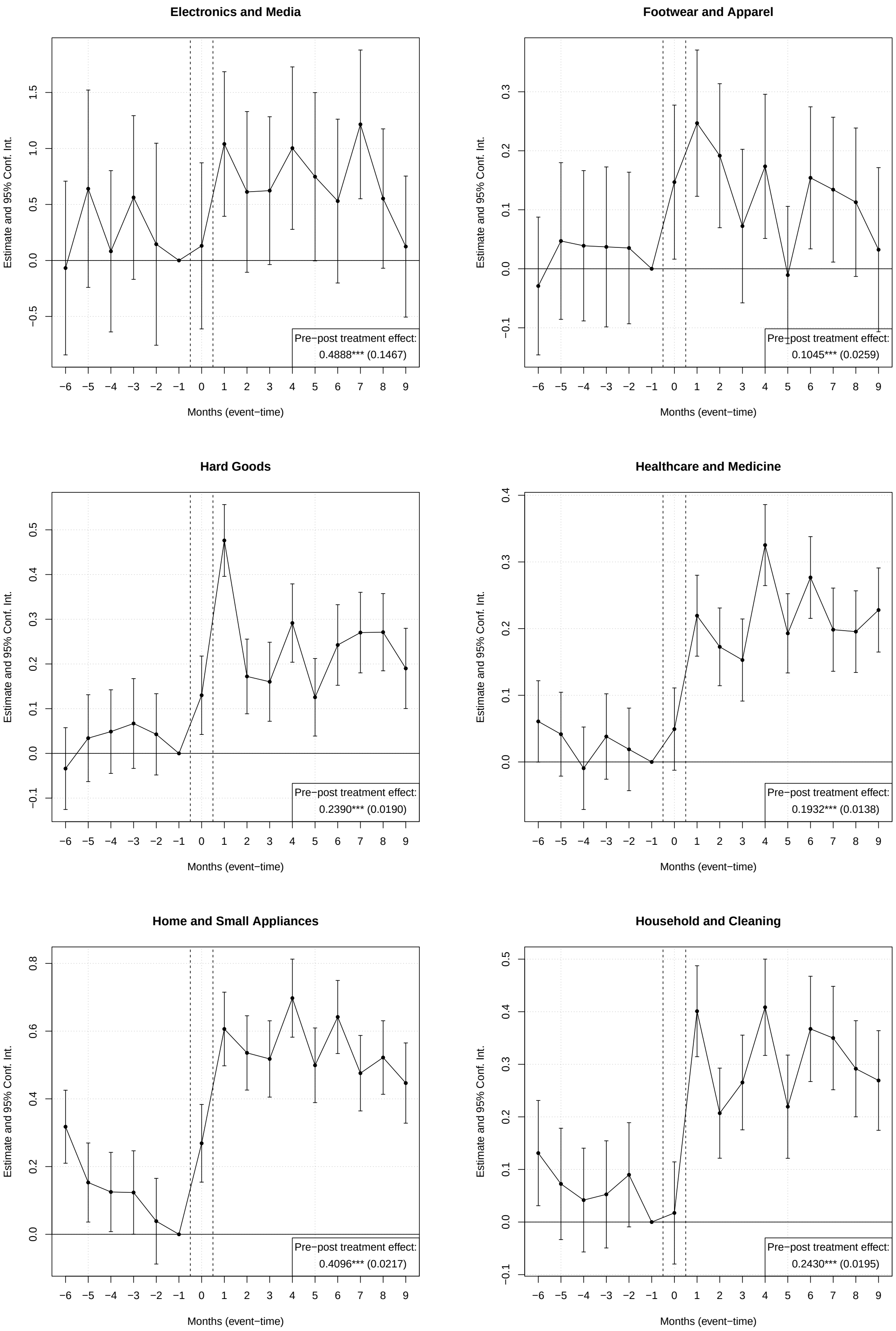
Notes: Map shows approximate proportion of local income constituted by PYEI-BEEI payments, based on income reported in the 2011 Census and PYEI-BEEI payments simulated to schools based on their socioeconomic quintile (and therefore number of PYEI-BEEI employees). Local areas are defined using the Census main place classification. South Africa's former Apartheid "homelands" borders are shown, illustrating how the programme payments make up a larger share of main place income in historically deprived areas.

Figure A.2: PYEI-BEEI direct effects on the firm's sales, by department: Panel (a)



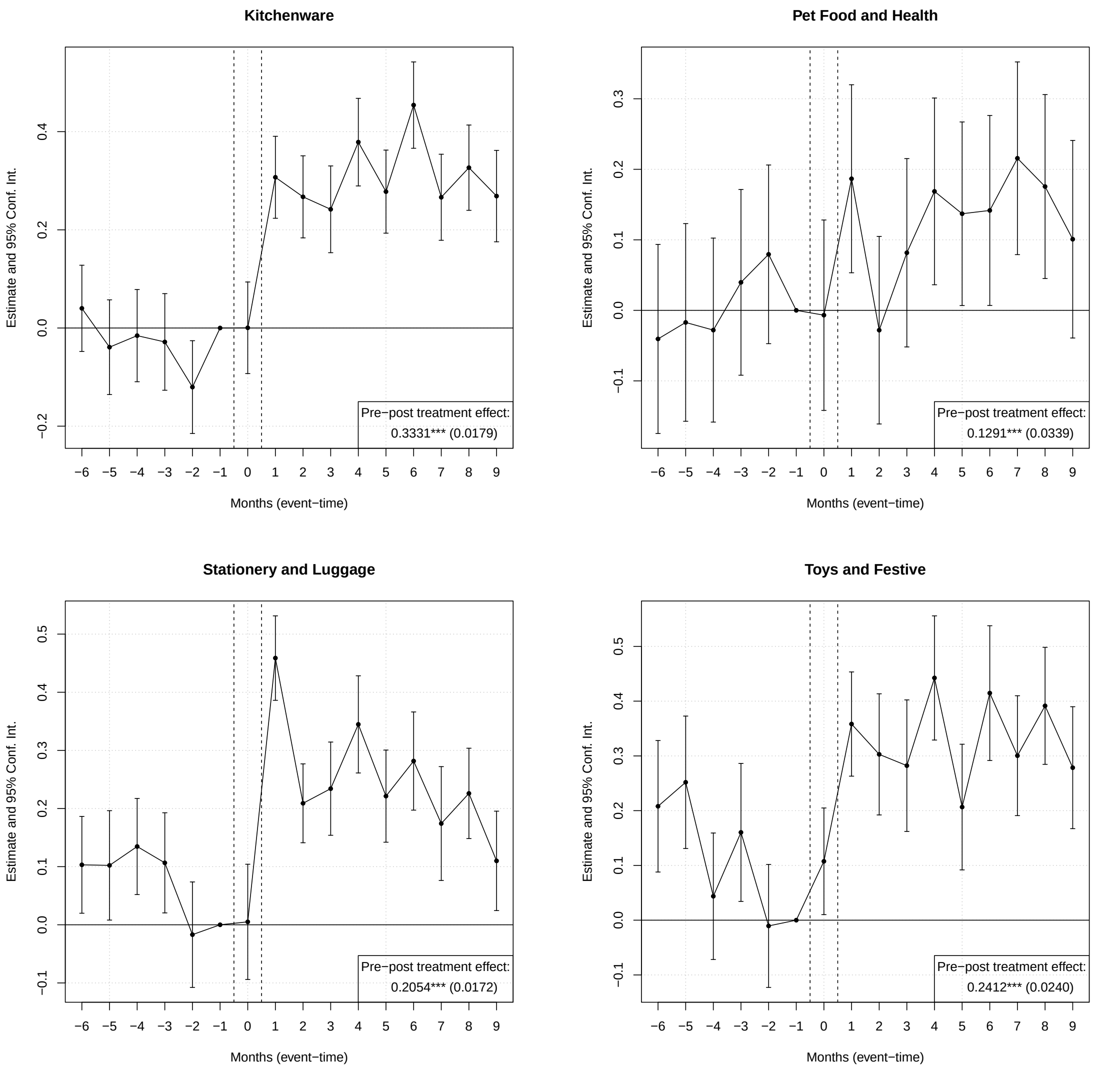
Notes: Figures are analogous to those of Figure 2, but estimated separately by department.

Figure A.3: PYEI-BEEI direct effects on the firm's sales, by department: Panel (b)



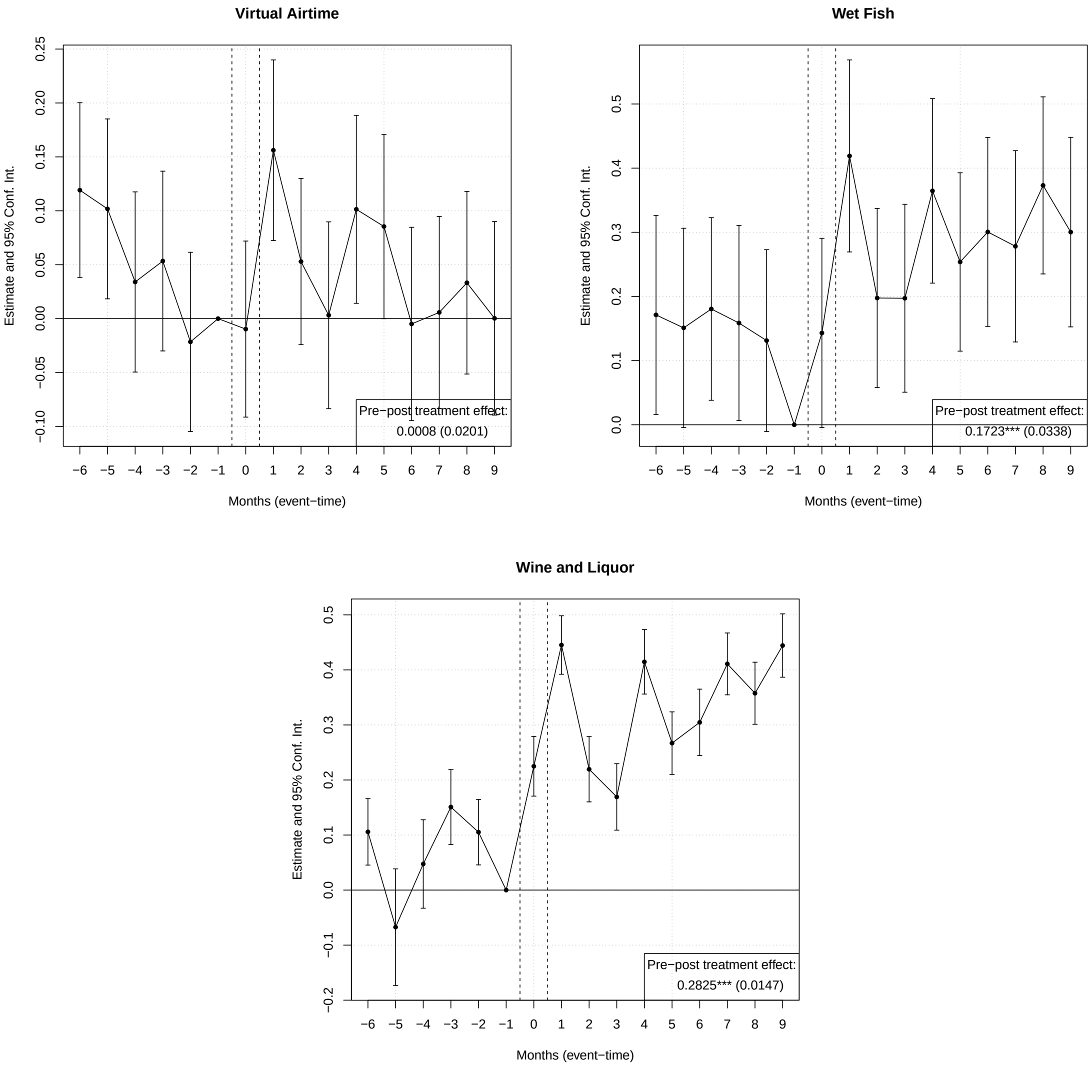
Notes: Figures are analogous to those of Figure 2, but estimated separately by department.

Figure A.4: PYEI-BEEI direct effects on the firm's sales, by department: Panel (c)



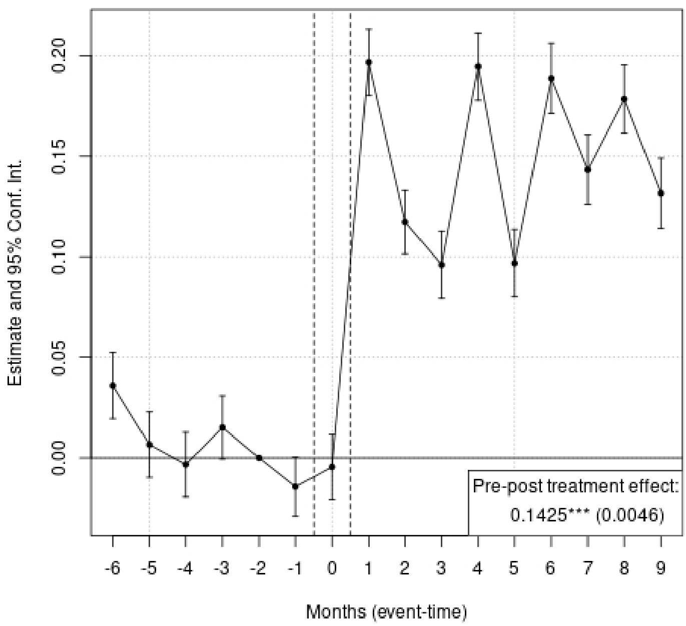
Notes: Figures are analogous to those of Figure 2, but estimated separately by department.

Figure A.5: PYEI-BEEI direct effects on the firm's sales, by department: Panel (d)



Notes: Figures are analogous to those of Figure 2, but estimated separately by department.

Figure A.6: **Event study of PYEI-BEEI participant sales effects at the firm: store fixed effects**



Notes: Figure is analogous to Figure 2 but the time dummy is interacted with store fixed effects rather than the banner-location interaction used in equation 1.

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