Cost-Benefit Analysis of Uganda's 10 Year Tax holidays for Qualifying (Strategic) sectors and Exporters

Tax Policy Department

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Executive Summary

The paper evaluates whether Uganda's 10-year tax holiday generates sufficient economic returns using basic cost–benefit analysis and causal analysis. This paper evaluates the economic and fiscal effectiveness of Uganda's 10-year Corporate Income Tax (CIT) holiday for strategic investors (CITE 4)[1] and exporters (CITE 5)[2], using a combination of cost–benefit analysis and causal inference methods. The objective is to determine whether these incentives generate sufficient economic returns to justify their fiscal cost and to assess their contribution to Uganda's broader structural transformation agenda.

At the aggregate level, the tax holiday generates positive net benefits. For every UGX 1 revenue forgone on a firm under qualifying sector, a benefit of UGX 2.49 in benefits was generated. Similarly, for every UGX 1 of cost on an exporter, UGX 1.85 in benefits was generated.

Qualifying sectors generate larger benefits and employment than exporters, though exporters still contribute meaningfully. Average estimated benefits over 2021/22 and 2024/25 accruing from firms under qualifying sectors, include PAYE (UGX 32.4 billion), Domestic VAT (UGX 59.4 billion), Import VAT (UGX 219.9 billion) and Capital investment (UGX 1,003.8 billion). Firms under qualifying sectors also employ approximately 12,602 workers. As for Exporters, benefits are smaller but still significant: PAYE (UGX 6.0 billion), Domestic VAT (UGX 5.7 billion), Import VAT (UGX 3.9 billion) and Capital investment (UGX 236.1 billion). In addition, exporters created 4,731 jobs.

[1] ITA section 21(1)z
[2] ITA section 21(1)ae

Manufacturing and export agriculture yields strong returns, while other sectors show weak or negligible impact. Manufacturing and export-oriented agriculture deliver very high returns, with strong value addition, employment, and linkages. In contrast, construction, transport, and several service sectors exhibit negligible or near-zero returns, suggesting weak economic spillovers.

Tax holidays boost sales and investment in strategic sectors but fall short in improving exports or local supply links. For strategic (qualifying) sectors, tax holidays are associated with strong and sustained increases in sales and modest but persistent increases in capital expenditure. These results suggest that the incentive supports firm-level expansion and scaling. However, this growth does not translate into broader structural transformation. There is no clear causal impact on export performance, and local input use declines relative to control firms, indicating a scope for improving domestic supply-chain linkages.

Tax holidays drive firm growth but yield limited and short-lived tax revenue gains, creating fiscal trade-offs. There is a disconnect between firm growth and fiscal returns. While beneficiary firms in qualifying sectors experience significant sales growth, the increase in taxes paid is small and short-lived, eventually plateauing. This suggests a reduction in effective taxation to the extent that profit expansion does not translate into proportional revenue gains. There is a need to balance the support for firm expansion while securing a durable revenue mobilization, which is an important balance in a revenue-constrained environment.

Exporter incentives show modest improvements in investment, exports, and taxes. For exporters, the results are more moderate. Beneficiary firms exhibit higher capital expenditure, stronger export levels, modest sales gains, and a gradual increase in taxes paid relative to firms that did not benefit from the tax - holiday. However,

due to data limitations, the causal interpretation of exporter performance should be treated with caution.

Tax holidays increase employment but gains largely reflect pre-existing firm strengths and vary in quality across sectors. The employment effects of the tax holiday are positive in aggregate, with over 12,600 jobs associated with strategic investors and 4,700 with exporters. However, the evidence suggests that much of this reflects pre-existing firm characteristics rather than a causal impact of the incentive. Moreover, job quality varies significantly across sectors, with more productive and durable employment concentrated in manufacturing and agro-processing.

Beneficiaries meet local employment rules but show limited improvement over comparable firms. The analysis also finds that beneficiaries generally comply with statutory requirements on local employment and wage allocation to Ugandan nationals. However, they do not consistently outperform comparable firms in local employment intensity, indicating limited additionality in this dimension.

Local sourcing remains weak, with firms relying more on imports and missing domestic targets. One of the most policy-relevant findings is the weak performance on local sourcing. Firms in qualifying sectors remain below the 70 percent local-input benchmark and, in some cases, reduce local input use following the policy. This suggests that incentives may encourage reliance on imported inputs rather than strengthening domestic production networks, thereby weakening the intended multiplier effects.

The tax holiday works but should be better targeted toward high-impact sectors with clear performance conditions. Overall, the evidence indicates that the 10-year tax holiday is effective. However, there is scope for improvement in targeting sectors with strong productive spillovers, by eliminating low impact sectors with

limited economic returns and high fiscal cost such as construction, transport, and several service sectors. The findings strongly support a shift away from broad, untargeted tax holidays toward a more selective, performance-based, and fiscally disciplined incentive regime. Such a regime should prioritize high-impact sectors such as manufacturing and export agriculture, link benefits to measurable outcomes, such as export performance, local sourcing, and job quality, and incorporate clear monitoring, evaluation, and exit mechanisms.

In conclusion, tax incentives help firms, but Uganda needs better targeting to achieve growth and revenue goals. While tax incentives can play a role in supporting firm growth, there is a need to improve the current design in Uganda to consistently deliver the broader structural and fiscal outcomes required to justify their cost. A more targeted and evidence-based approach is necessary to align tax incentives with the country's development and revenue mobilization objectives.

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Acknowledgement

This report was prepared by Corti Paul Lakuma (DRMS Specialist) and Isaac Arinaitwe (Principal Economist) of Ministry of Finance, Planning, and Economic Development's (MoFPED) Tax Policy Department (TPD) using data from the Uganda Revenue Authority (URA).

1. Introduction

Tax holidays aim to drive growth and investment, but their effectiveness remains debated, especially given their fiscal cost. Tax incentives, particularly long-duration corporate income tax (CIT) holidays, have become a central instrument of industrial policy in many developing economies, including Uganda. These incentives are intended to attract investment, stimulate firm growth, promote exports, generate employment, and ultimately expand the domestic tax base. However, their effectiveness remains a subject of ongoing debate, particularly in revenue-constrained contexts where forgone tax revenues must be justified by measurable economic gains.

We assess whether Uganda's 10-year tax holiday generates sufficient economic returns or mainly transfers public resources to firms. This paper evaluates the economic and fiscal effectiveness of Uganda's 10-year CIT tax holiday, focusing on two major beneficiary groups: strategic investors (CITE 4) and exporters (CITE 5). The analysis is motivated by a fundamental policy question: do tax holidays generate sufficient economic returns, through investment, employment, value addition, and fiscal revenue, to justify their cost, or do they primarily represent a transfer of public resources to firms without commensurate developmental impact.

The study combines basic cost–benefit analysis and causal methods to assess fiscal efficiency and firm-level impacts of tax incentives. The study adopts a dual analytical framework combining cost–benefit analysis (CBA) and causal inference methods. The cost–benefit component quantifies the full spectrum of fiscal costs, including CIT forgone, administrative and compliance costs, and associated public expenditures, against measurable benefits such as PAYE, VAT, excise revenues, and capital accumulation. These are evaluated using benefit–cost ratios (BCRs)

across sectors and over time to assess economic efficiency. Complementing this, causal analysis employs firm-level panel data and quasi-experimental techniques, including difference-in-differences and matched comparisons, to isolate the impact of tax incentives on key firm outcomes such as investment, sales, exports, employment, and tax payments.

The study's strength lies in sectoral analysis and clear methods. The study builds up on previous analysis such as that conducted by Namunane, Carson, & McNabb (2023). However, unlike previous studies, a key strength of this study lies in its sectoral disaggregation and methodological transparency. By distinguishing outcomes across ISIC sectors and separating aggregate efficiency from causal effects, the analysis provides a nuanced understanding of where tax incentives work and where they fail. In particular, the introduction of the strategic sector incentive in FY2018/19 offers a credible identification strategy for evaluating causal impacts, while the exporter regime is assessed through matched post-treatment comparisons due to data constraints.

The study examines whether tax incentives balance growth goals with fiscal sustainability and broader economic spillovers. The paper situates its contribution within a broader policy context characterized by Uganda's need to balance investment promotion with domestic revenue mobilization. While tax incentives are often justified on the basis of growth and competitiveness, their design must also ensure fiscal sustainability and alignment with structural transformation objectives. This study therefore goes beyond aggregate assessments to examine whether observed firm-level gains translate into broader economic spillovers—such as export expansion, domestic supply-chain development, and sustained tax revenue growth.

The paper combines fiscal and causal analysis to show that effective tax incentives require proper targeting, design, and

alignment with the economy. Overall, the paper contributes to the evidence base on tax expenditures in developing economies by integrating fiscal accounting with causal evaluation. It provides policy-relevant insights into the conditions under which tax incentives can be justified, highlighting the importance of targeting sectors, incentive design, and alignment with domestic economic structures.

2. Methodology and Data

The study uses a mixed method approach which combines a basic cost benefit analysis and a causal inference methodology to estimate the economic outcomes of tax expenditure.

The basic costs and benefits were quantified, sector-classified, and evaluated using Benefit–Cost Ratios. This basic cost benefit analysis comprised three main steps. First, all relevant costs and benefits for strategic investors (TE4) and exporters (TE5) were quantified and organized into annual streams for TE4 and TE5 cost and benefit components. Second, these annual cost and benefit streams were classified according to the International Standard Industrial Classification (ISIC) sector of the tax-incentive recipient (e.g., agriculture, manufacturing). Third, Benefit–Cost Ratios (BCRs) were computed both annually over the study period (2014/15–2024/25) and by ISIC sector. This method was used by Bahemuka, et al. (2023).

The analysis adopts the standard theoretical interpretation of the BCR. In this case, values greater than one indicate that benefits exceed costs, implying that the tax incentive is economically justified, while values below one indicates that costs exceed benefits, suggesting that the incentive is economically inefficient. Box 1 summarizes the variables used in the basic cost benefit analysis. The analysis also produced complementary indicators, including employment levels among incentive recipients and the number of beneficiaries under TE4 and TE5. (See Appendix 1 for a glossary of variables utilized).

Administrative data from URA was mostly used to carry out the analysis. This included (i) data from CIT returns relating to recipients of the 10 year tax- holidays incentives; (ii) customs data particularly import values, and import values on plant and machinery acquired by the recipients of the tax incentives; (iii) data from income tax returns particularly on PAYE by recipients of the 10-year tax holidays, number of people employed by the recipients,

and legal costs and audit fees which constituted the compliance costs. The additional data used in the analysis was sourced from Bank of Uganda website (TB rates as interest rates), MoFPED on development expenditure for UIA and UFZA, and UBOS on deflators for the period studied (2011/12-2021/22).

Box 1: Variables adopted for the basic costs and benefits analysis

Costs[3] included:

1) The costs considered in the analysis comprised the following:

2) Corporate Income Tax (CIT) is forgone, calculated as 30 percent of the sum of chargeable income and exempt income.

3) Administrative costs incurred by the Uganda Revenue Authority and the Tax Policy Department in processing and approving applications for tax incentives.

4) Compliance costs borne by incentive recipients during the application process. These were proxied by legal fees and audit costs reported in income tax returns. Since recipients also incur these costs for activities unrelated to tax incentives, only a proportion was attributed to the incentives. This proportion was calculated as exempt income expressed as a share of annual CIT forgone.

5) Interest in costs on additional government borrowing, reflecting the assumption that foregone CIT would otherwise have been collected as revenue, thereby reducing borrowing needs. Interest was computed using the average annual 364-day Treasury Bill rate.

6) Public development expenditure undertaken to support incentivized investments, including annual development spending by the Uganda Investment Authority and the Uganda Free Zones Authority.

Benefits included:

1) Pay-As-You-Earn (PAYE) revenues generated from employment created by recipients of CITE 4 and CITE 5, under the assumption that such employment would not have occurred in the absence of incentives.

2) VAT and Excise duty paid on domestic inputs used in production by incentive recipients. This assumes that, without the incentives, firms would not have undertaken production and therefore would not have demanded these inputs. Estimates were based on the input tax credit formula specified in item (f) of the Fourth Schedule of the VAT Act.

[3] Other costs that should have been appropriate but could not be included due to unavailability of data are: (i) leakage due to abuse of CITE 4 and CITE 5, and (ii) investments crowded out by CITE 4 and CITE 5 i.e., investments that could have been in pipeline to be implemented even without CITE 4 and CITE 5 but were probably abandoned when competitors obtained tax incentives for the obvious reason that there would be unfair competition that could lead to losses on part of the non-recipients of the tax incentives.

3) Taxes on dividends distributed to shareholders of firms benefiting from CITE 4 and CITE 5.

4) Capital accumulation, particularly investment in plant and machinery by incentive recipients. This reflects the assumption that capital formation contributes to short- and long-term economic growth, which ultimately expands the tax base. Import values of plant and machinery (HS Chapters 84 and 85) acquired by CITE 4 and CITE 5 beneficiaries were used to proxy this benefit.

Source: Authors own construction with reference to Bahemuka, et al., 2023

In the causal inference estimation of the effect of the CIT tax holiday for qualifying (strategic) sectors, we compare similar firms before and after 2018 to estimate impact. The empirical analysis exploits the introduction of the qualifying (strategic) sector tax incentive in FY2018/19 using a difference-in-differences framework applied to firm-level panel data from 2012 onward. The method used compares how outcomes for benefiting firms changed after FY2018/19 to how outcomes changed for similar non-benefiting firms over the same period. By looking at these differences, the analysis isolates the effect of the tax incentive. It focuses on changes before and after the policy. The model also controls for firm characteristics that do not change over time. These include factors such as business structure. It also accounts for yearly changes affecting all firms. These may include overall economic conditions. This ensures the results reflect the policy impact. The approach estimates how much benefiting firms improved relative to others. It shows whether the incentive led to better performance.

We track yearly impacts and adjust for different pre-policy characteristics. The study tracks how the policy's impact changes over time (event study). It compares each year to the period before the policy. This helps check if the results are reliable. It also recognises that benefiting (treated) firms may have been different before the policy. To address this, similar firms are matched based on pre-policy characteristics. The analysis also allows firms to follow their own trends over time. In addition, weights are adjusted to improve comparability between firms.

Results reflect causal impact, allowing for post policy differences. The results are interpreted as the effect of the policy. This relies on the assumption that firms would have followed similar trends without the policy. This assumption is important for

making a fair comparison. The analysis checks whether this assumption is reasonable. It also allows for some differences in trends across firms. Additional methods are used to improve the comparison. These steps help ensure the results are more reliable. The detailed methodology is in Appendix 2.

On the other hand, when it comes to exporters, we compare similar firms after 2012, not the original 2008 policy effect. The 10 -year tax holiday for exporters was introduced in 2008, but we only have data beginning the year 2012. Therefore, all beneficiary firms have already enjoyed the benefit at the time they first appear in the dataset in 2012. Consequently, the analysis cannot identify the full causal effect of the policy's introduction. Instead, it estimates the medium-run post-2012 effect of being a tax holiday beneficiary relative to a comparable non-beneficiary firm.

We match similar firms, then compare their outcomes after 2012. Benefiting firms are compared to non-benefiting firms that are as similar as possible in 2012. These firms are matched based on key characteristics such as sector, size, assets, employment, profitability, and export orientation. This ensures a fair comparison between the two groups. It reduces the risk that differences in results are driven by underlying firm characteristics. After matching, the analysis follows both groups over time. It examines how their outcomes change after 2012. The method controls for factors that do not change within firms. These include core business structure and long-term characteristics. It also accounts for changes affecting all firms each year. These include broader economic conditions. The analysis then compares outcomes between benefiting and non-benefiting firms each year. This helps identify differences in performance over time. In simple terms, it tracks how similar firms evolve. It focuses on whether firms receiving the incentive perform differently. The year 2012 is used as the baseline for comparison.

We assume similar trends, so results show post-2012 differences only. We assume that, after matching similar firms, they would have followed the same path if there was no tax holiday. So, the results show how treated firms performed differently after

2012 compared to similar firms, but they do not capture the full impact of the policy since it started in 2008. More details are provided in Appendix 3.

Table 1 presents baseline summary statistics comparing treated (beneficiary) and control (non-beneficiary) firms across key outcomes for both qualifying sectors and exporters. There is a strong sales growth of beneficiaries of the 10-year tax holiday, which suggest beneficiaries are already larger. However, there is weak export effects, which suggest benefitting firms are not export-intensive. In addition, there is limited local sourcing as witnessed by lower domestic input ratios and limited tax gains. These results justify the use of Mahalanobis (Rubin, 1976; Rubin, 1976a), fixed effects and event study designs in studying qualifying sectors and the need to embrace conditional and carefully qualified comparisons among exporters given that we do not have the data for when the policy started.

Table 1: Summary of Variable used in the Analysis

Qualifying Sectors	**Beneficiaries (Treated)**	**Non-Beneficiaries (Control)**	**Observations**
CAPEX (Billions)	12.40	30.00	48
Exports (Millions	2.1	7.7	19
Employees	361	380	49
Value Added (Billions	48.10	11.60	31
Sales (Billions)	64.40	34.30	50
Domestic Input (ratio)	0.50	0.61	50
Local Employee (ratio)	0.93	1	49
Taxes Paid	9.84	2.24	50
Share of wages to National (ratio)	0.79	0.91	49
Exporters	**Treated**	**Control**	**Observations**
CAPEX (Billions)	11.50	0.14	4,172
Exports (Millions)	1.89	91.00	106
Employees	262.83	11.01	1,294

Qualifying Sectors	Beneficiaries (Treated)	Non-Beneficiaries (Control)	Observations
Value Added (Billions	5.35	0.68	904
Sales (Billions)	45.30	0.17	6,895
Domestic Input (ratio)	0.83	0.97	6,895
Local Employee (ratio)	1.00	0.98	1,294
Taxes Paid	0.90	0.03	6,895
Share of wages to National (ratio)	0.72	0.99	1,294

Source: Authors Calculation using URA data

3.1. Results: Basic Cost Benefit Analysis

Uganda's 10 Year tax holiday generates benefits overall. The cost–benefit analysis indicates that Uganda's corporate income tax holiday generates positive net returns overall, but with substantial variation across sectors and incentive provision. Qualifying sectors, also referred to as Strategic investors, (CITE 4) exhibit a higher average benefit-cost ratio (BCR = 2.49) compared to exporters (CITE 5, BCR = 1.85), suggesting relatively stronger aggregate efficiency (Table 2).

Table 2: Cost Benefit Analysis of CIT Tax Holidays by ISIC Sector for period 2021/22 to 2024/25

Cost -Benefit Analysis Variable	All CIT - Recipients	A- Agriculture, forestry and fishing	C- Manufacturing	F- Construction	G- Wholesale and retail trade; repair of motor vehicles and motorcycles	H- Transportation and storage	Q- Human health and social work activities	S- Other service activities
CITE 4 - Strategic Investors	46	7	25	1	7	1	3	2
CITE 5 - Exporters	18	8	9		1			
Employment (CITE 4)	12602	2103	5880	23	667	8	3853	68
Employment (CITE 5)	4731	2357	2358		16			
BCR (CITE 4)	2.49	2.26	5.49	0.02	0.95	0.00	0.23	0.42

BCR (CITE 5)	1.854	4.784	1.592		0.003			
BCR (Overall)	5.03	7.05	7.08		0.96			

Source: Authors calculations using URA Data

However, there are sharp sectoral disparities with manufacturing and export-oriented agriculture delivering very high returns, **while construction, transport, and certain services exhibit negligible or near-zero returns**. Manufacturing and export-oriented agriculture has BCRs exceeding 5 and 4 respectively, while construction, transport, and certain services have near zero returns indicating significant deadweight losses.

Manufacturing stands out as the most efficient recipient of the 10-year tax holiday. Manufacturers in the qualifying sectors (CITE 4) have a BCR of 5.49 and employed 5,880 people; while exporting manufacturers (CITE 5) have a BCR of 1.592 and employed 2358 people. Therefore, manufacturers generate substantial economic returns per unit of tax expenditure. The high BCR reflects strong value addition, deep backward and forward linkages (input-output networks) and learning spillovers and productivity gains.

The 10-year tax holiday targeted at agro-processing and export agriculture is highly effective and aligns with other government objectives (10-fold-growth). On one hand, Agricultural Firms in the qualifying sectors (CITE 4) have a BCR of 2.26 and employ 2,103. On the other hand, export-oriented (CITE 5) agricultural firms have a BCR 4.784 and employ 2,357 (CITE 5), which points to a consistently strong return, especially under exporter incentives. This is likely due to high marginal productivity gains from formalization, export market integration and relatively low baseline capital intensity.

However, there is a scope for improvement when it comes to wholesale & Retail Trade where the 10- year tax holiday has not been welfare-enhancing and has been subsidizing a risky activity that would occur regardless (deadweight loss). The BCR for qualifying sectors (CITE 4) is 0.95 and the sector generates 667. This sector sits at the margin of efficiency. While it generates

employment, it records low value addition likely characterised by high informality and thin margin.

The construction sectors are inefficient in utilising the 10 - year tax. The fiscal costs far outweigh economic benefits. The BCR for qualifying sectors (CITE 4) is 0.02 and it produces negligible employment (23 jobs). This near-zero returns, indicate very weak linkage effects; short-term, project-based activity; and a high likelihood that investments are not incentive-induced.

The 10- year tax holiday in transportation and storage sector represents a fiscal leakage and should be reconsidered. There is zero BCR for transportation and storage under qualifying sectors (CITE 4). The sector generates a negligible number of jobs (8). This sector generates no measurable return relative to cost. This is likely dominated by capital imports with limited domestic spillovers and weak employment and productivity effects.

Under the human health and social work sector, the 10- year tax holiday may still be justified, but not on revenue grounds rather on social policy grounds. The BCR is 0.23 and employment is 385 jobs. Although economically inefficient in strict fiscal terms the sector may generate social returns not captured in BCR. Nevertheless, the public good characteristics does distort purely financial evaluation.

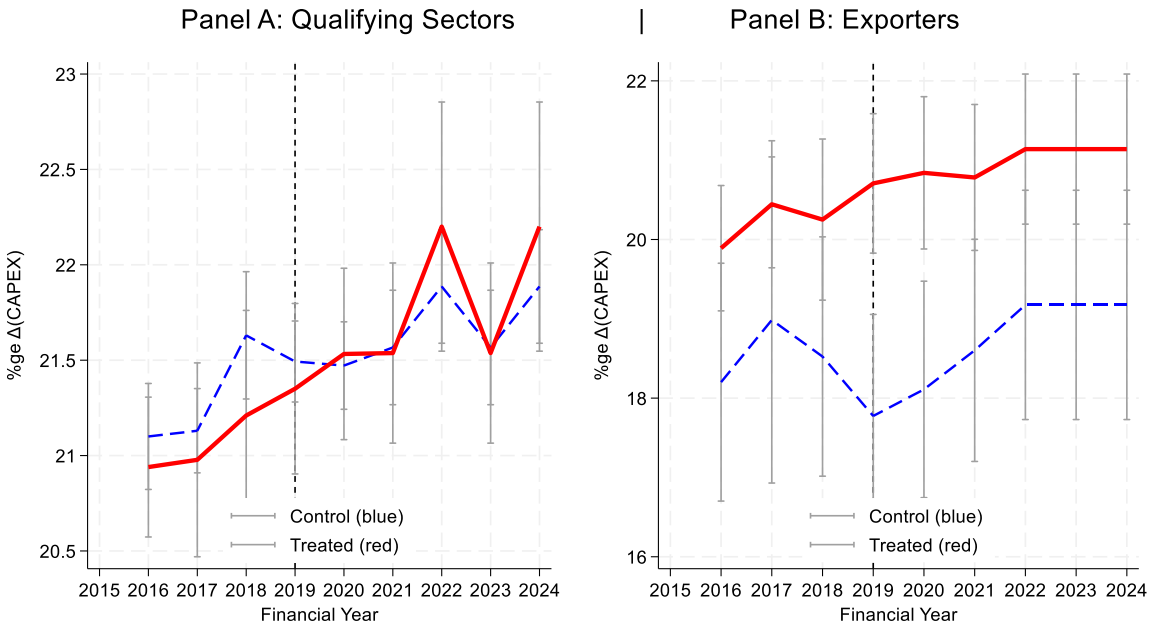
The 10-year tax holiday for other sectors has a low impact and is poorly targeted. The BCR is 0.42 and employment is 68. This category shows weak economic multipliers and limited productivity spillovers.

3.2. Results: Causal Effect Analysis

Tax holidays induce modest increases in investment among strategic investors but have large effect on exporters. The CAPEX (Capital Expenditure) dynamics reveal important differences in the responsiveness of firms to tax holidays (Figure 1). Strategic investors exhibit a modest increase in capital expenditure following

the introduction of tax holidays, consistent with some degree of investment timing or scaling. The effect is strongly persistent, suggesting significant additionality. On the other hand, exporters display a stronger response, with benefiting (treated) firms maintaining higher levels of CAPEX throughout the period.

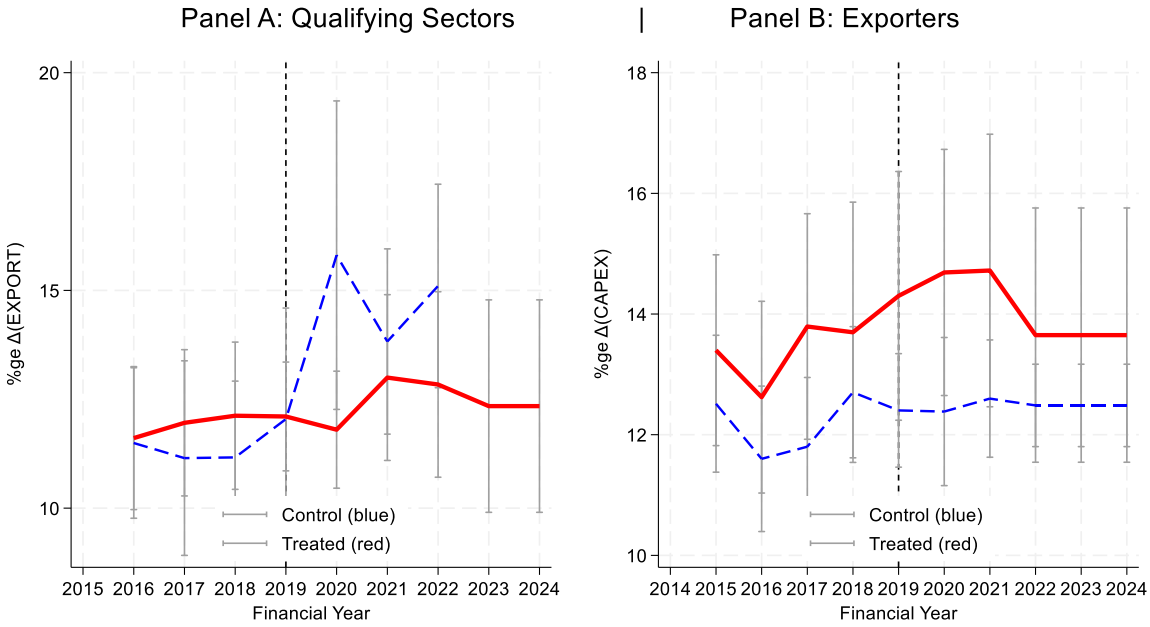
Figure 1: Capital Expenditure Trends



Source: Authors Calculation using URA data

Tax holidays do not causally improve export performance in qualifying sectors and are therefore ineffective for export promotion. However, treated exporters exhibit higher export levels. There is no clear evidence that tax holiday has a causal impact on export performance among qualifying sectors (Figure 2). In this case, tax holidays are poorly suited as instruments for export promotion among qualifying sectors and that policy efforts should instead focus on addressing structural constraints to trade. However, treated exporters exhibit higher export levels than their comparable counterparts.

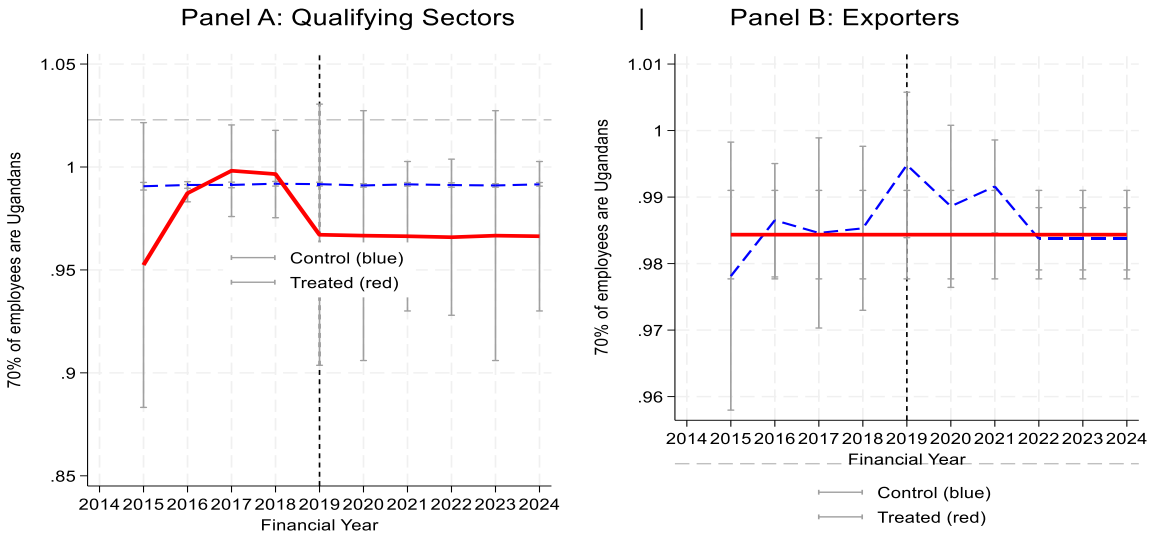
Figure 2: Exports Trends



Source: Authors Calculation using URA data

Although firms benefiting from tax holidays employ fewer Ugandans than comparable control firms, they still meet the statutory requirement of maintaining at least 70% Ugandan employees. While comparable control firms employ more Ugandans than beneficiaries of tax holidays, beneficiaries of tax holidays satisfy the legal threshold of 70 percent of employees must be Ugandans (Figure 3).

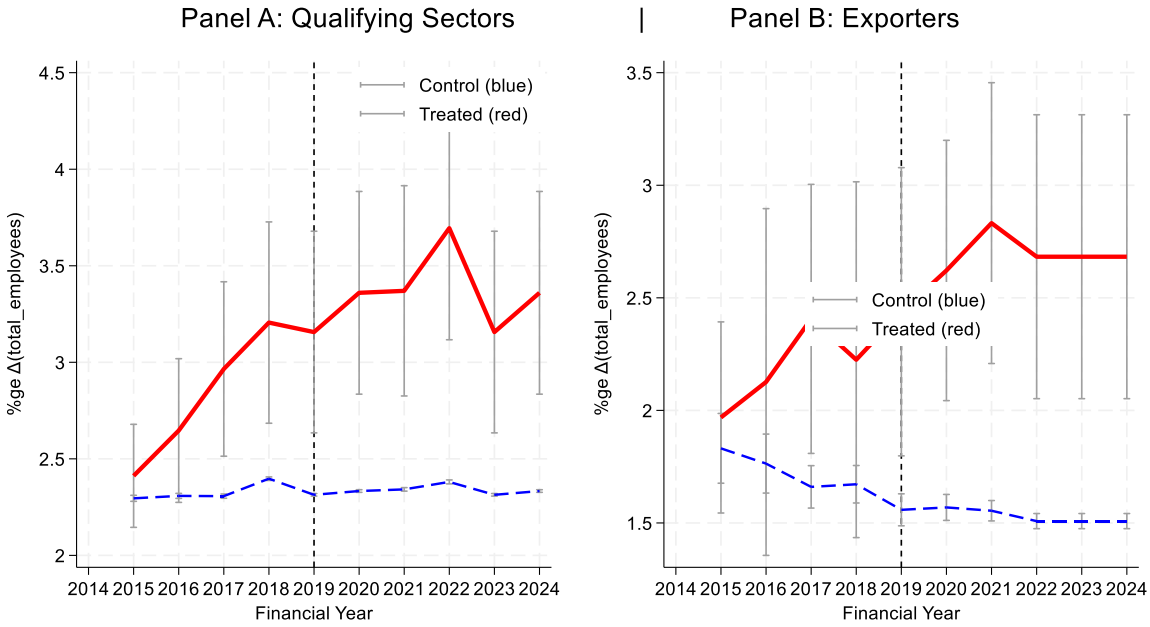
Figure 3: Ugandan Employees



Source: Authors Calculation using URA data

Tax holidays are associated with higher employment growth among beneficiaries, but this reflects pre-existing firm characteristics rather than a causal effect of the policy. For qualifying sectors, treated firms already show higher employment growth than control (Figure 4). In this case, Upward trend begins well before 2019. In addition, there is no clear structural break at treatment. This implies that there is trend continuation, which is not necessarily a response to the 10-year tax holiday. As for exporters, there is a clear divergence even pre-treatment. This suggests that treated firms are rising while control firms are declining. There is evidence of strong selection effects in both panels, and it is likely that beneficiaries are already high-growth firms

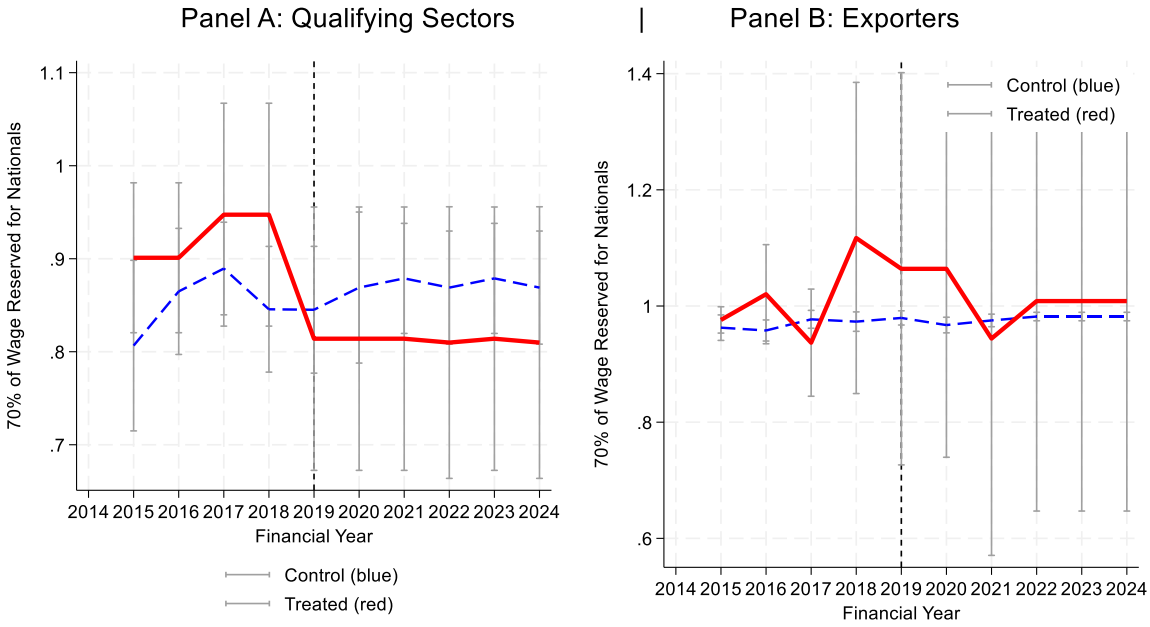
Figure 4: Increase in the Number of Employees



Source: Authors Calculation using URA data

Firms benefitting from tax holidays are compliant to the pre-condition to keep the share of wages paid to Ugandan nationals at more than 70%. While treated qualifying sectors (Figure 5, Panel A) remain below control and exhibit high volatility in the proportion of wages paid to nationals, tax holidays sustain wage allocation to nationals at more than 70% of total wage. On the other hand, treated exporters (Figure 5, Panel B) show temporary fluctuations which eventually converge with control at 100% of the total wage bill.

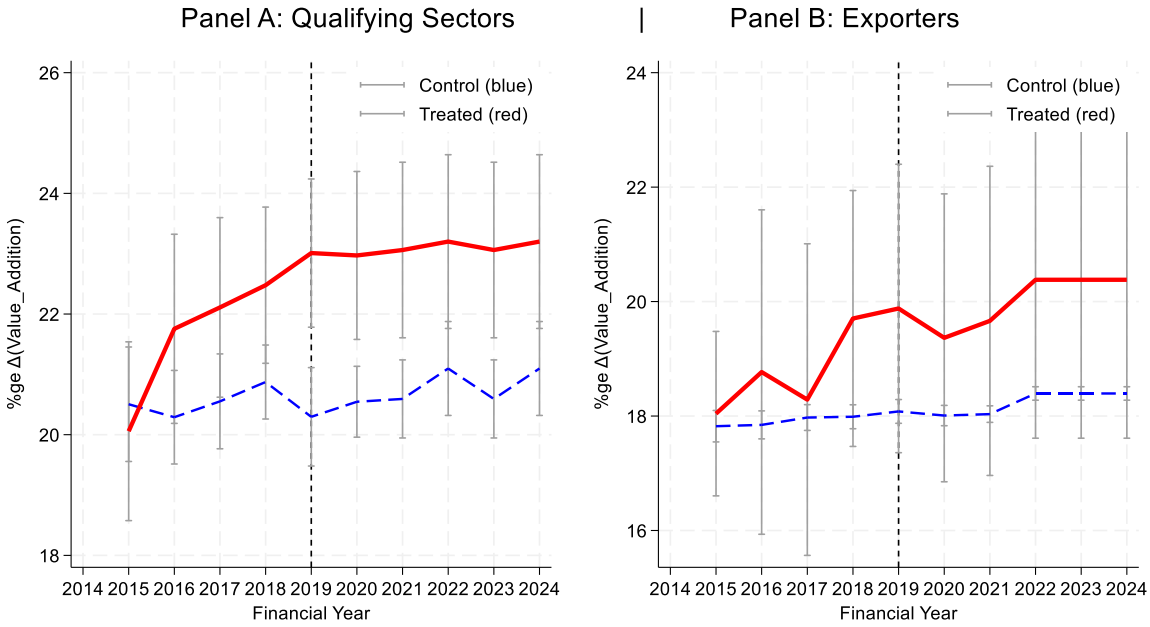
Figure 5: Proportion of wages to Nationals



Source: Authors Calculation using URA data

Tax holiday recipients already outperform non-recipients, with the tax holiday modestly reinforcing trends rather than causing a strong impact regarding value addition. Treated qualifying sector firms already exhibit higher value addition than controls in pre-policy years (before 2018/19) (Figure 6). The gap is persistent in the post-policy years, and treated firms grow slightly faster. The policy reinforces existing differences in value addition. This suggests limited relative value addition. Smaller gap between treated and control firms compared to Panel A. As for exporters, trends are relatively flat and similar, and treated firms show a modest increase in value addition. There is a slight upward shift after 2017. There is some evidence of positive policy impact, but the effect may reflect gradual adjustment rather than a strong causal break.

Figure 6: Value Addition

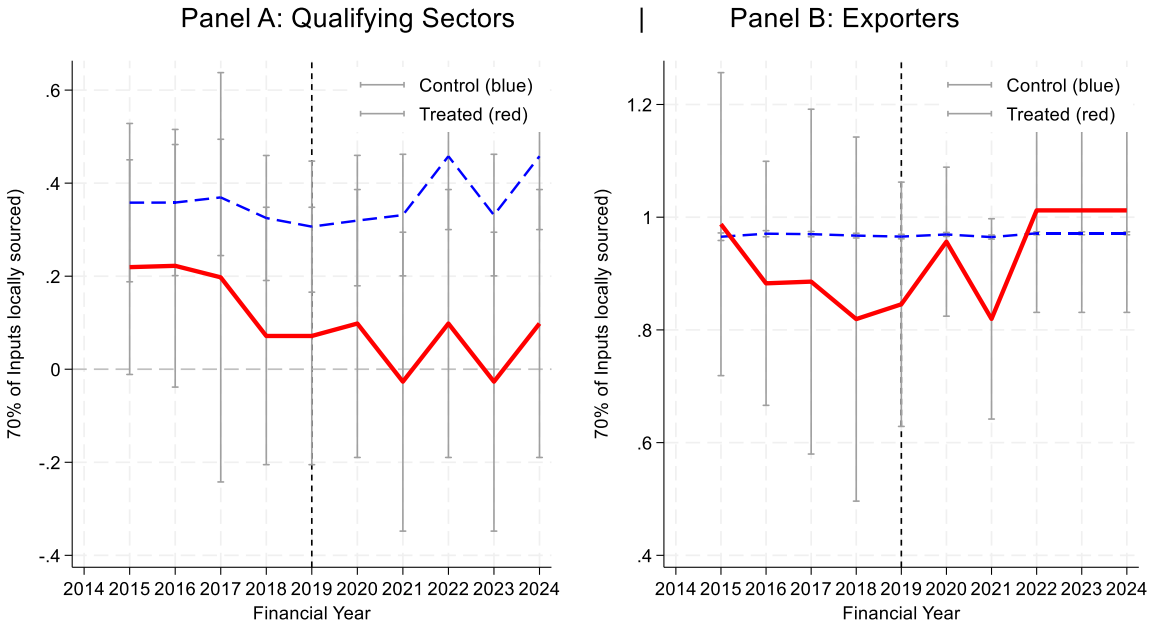


Source: Authors Calculation using URA data

Tax holidays have little impact on the use of local inputs. For qualifying sectors, both treated firms (red) and control firms (blue) are below the 70% threshold in use of local inputs (Figure 7). Treated firms show a decline in locally sourced inputs after the policy. Control firms remain relatively stable or slightly increasing. Treated firms start below controls. Trends are roughly parallel but at different levels. There is clear divergence where treated firms drop sharply around 2019–2021 and become volatile afterward. Meanwhile, control firms continue to increase gradually. Overall, policy does not appear to increase local sourcing and is associated with reduced local input use, or no improvement relative to controls. This is because qualifying firms may rely more on imported intermediate inputs and scale production faster than local supply chains can support. Also qualifying sectors may be substituting higher-quality or specialized imports.

Exporters show marginal difference from controls, with minor and short-term effects on local sourcing. On the other hand, treated and control exporting firms track each other closely and there is no strong or persistent divergence. Treated firms show some short-term fluctuations but return close to control levels. Exporter tax holiday do marginally change local sourcing behavior.

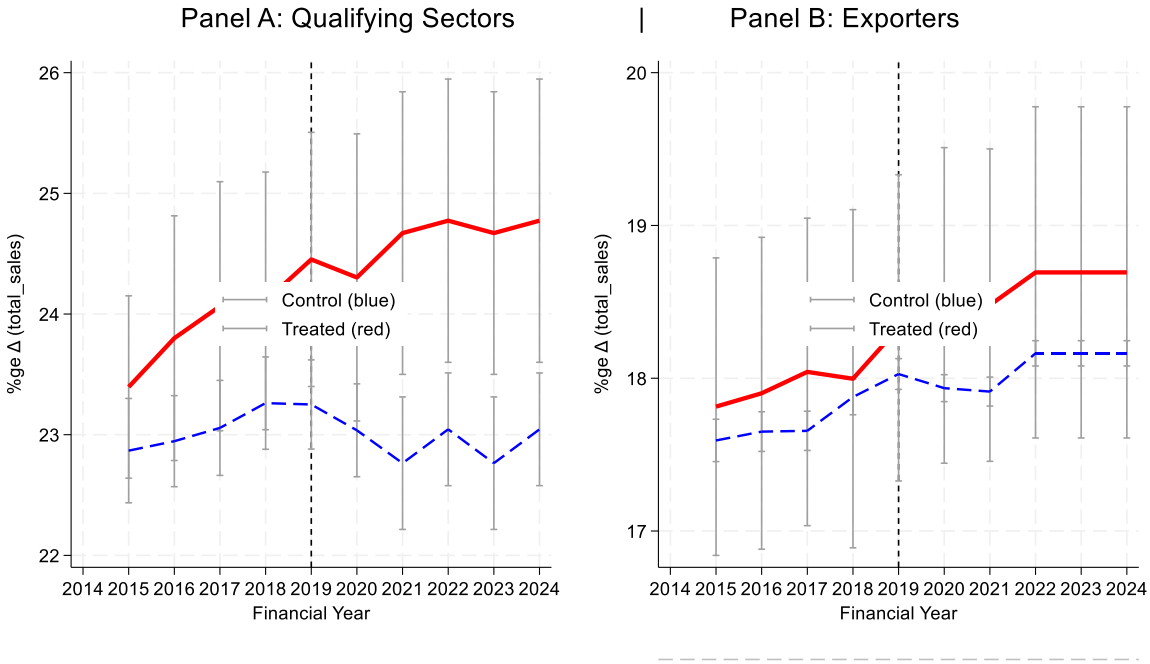
Figure 7: Locally Sourced Inputs



Source: Authors Calculation using URA data

Qualifying sectors' tax holidays drive strong, sustained sales growth, while exporter incentives have modest, short-lived effects. Panel A in Figure 8 suggests that trends are roughly parallel, though treated firms grow slightly faster. Clear positive divergence where treated firms continue steady growth maintain a widening gap over controls. Control firms show slower growth and some fluctuations. There is strong evidence that tax holiday is associated with higher sales growth for treated firms, for there is a persistent and increasing gap relative to controls. This suggests that tax holidays may support qualifying sectors expansion or scaling. As for exporters (Panel B), treated firm experience modest increase in sales. Control firms also increase, but more slowly. A gap in the two groups opens slightly but remains small. This supports the conclusion that exporter tax holiday has a positive but modest effect on sales. The effect is limited in magnitude and not strongly persistent

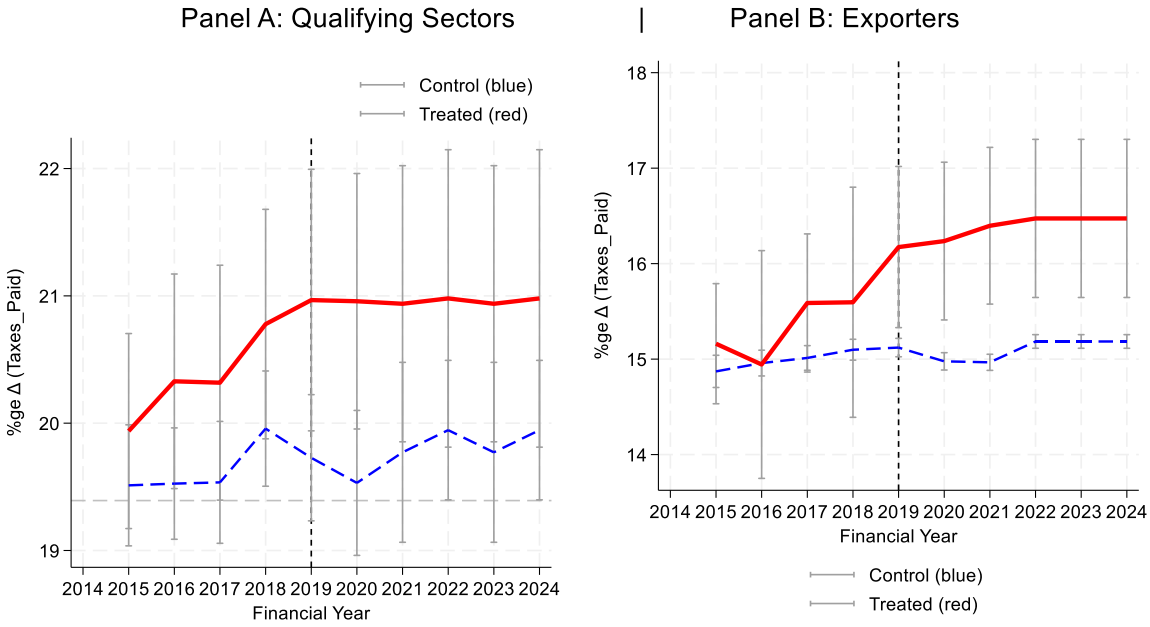
Figure 8: Sales



Source: Authors Calculation using URA data

Tax holidays to qualifying sectors raise taxes briefly but not sustainably, while tax holidays to exporters moderately increase tax revenue. In the pre-policy years before 2018/19, treated qualifying sectors firms (Panel A, Figure 9) were already paying more taxes than controls. However, there is a level increase around 2018/19 among treated firms than a plateau. This suggests that tax holidays are associated with a small increase in taxes paid initially but no sustained growth afterward, which contradicts earlier results which showed sales increasing strongly, yet taxes do not increase proportionately. This suggests reduced effective taxation, likely due to the tax holiday itself lowering effective rates and profit expansion not translating into tax revenue. As for exporters (panel B, Figure 9), treated firms show a clear upward trend in taxes paid. While control firms remain relatively flat. This suggests that exporter tax holiday is associated with a moderate increase in taxes paid. While this may seem counterintuitive, it likely reflects increased activity and scale expansion of taxable base (even with lower rates).

Figure 9: Taxes Paid



Source: Authors Calculation using URA data

4. Conclusions

At the aggregate level, the 10-year tax holiday is beneficial. The cost-benefit analysis reports an average benefit-cost ratio of 2.49 for strategic investors (CITE 4) and 1.85 for exporters (CITE 5), implying that, on average, benefits exceed costs.

Manufacturing and export agriculture yield high returns, while construction, transport, and services yield little. A disaggregation reveals that manufacturing and export-oriented agriculture generate very high returns. However, construction, transport, and several service activities generate negligible or near-zero returns. In substance, the paper is efficient only where it supports sectors with strong productive spillovers, deeper value chains, and stronger formal-sector linkages. In this case the incentive is not uniformly efficient.

The 10-year tax holiday works well for manufacturing. The paper reports a BCR of 5.49 for strategic-investor manufacturers and identifies manufacturing as the strongest-performing recipient of the incentive.

Export agriculture is also shown to perform strongly, especially under the exporter regime, with a BCR of 4.78. These are not trivial differences. They suggest that where the incentive supports sectors with strong backward and forward linkages, learning effects, and capital deepening, fiscal concessions can plausibly catalyze broader economic gains.

By contrast, the extremely weak BCRs in construction (0.02) and transport/storage (0.00) suggest that those sectors are receiving relief without generating commensurate economic returns. In those cases, the incentive looks less like inducement and more like a transfer of rents to investments that likely would have occurred anyway.

Qualifying sectors show gains in sales and investment. The causal analysis reinforces this selective interpretation. For qualifying sectors, the paper finds the clearest post-policy gains in sales and some improvement in capital expenditure. Sales growth among treated strategic firms is strong and sustained after FY2018/19, with a visibly widening gap relative to matched controls. CAPEX also rises modestly and persistently. These two results together suggest that the tax holiday may indeed help beneficiary firms scale production and expand market activity.

Tax holiday boosts firm growth but weakens exports and local supply linkages. The broader developmental transmission is much weaker. The same firms do not show convincing causal gains in exports, and local input use actually appears to deteriorate relative to controls. In other words, the holiday seems more effective at

supporting firm expansion than at generating domestic supply-chain deepening or export transformation. That is a major policy distinction.

Tax holidays boost sales but are limited, un-sustained tax revenue, creating fiscal trade-offs. Strategic-sector beneficiaries experience strong sales growth, yet the increase in taxes paid is only brief and then plateaus. That pattern is economically significant. It suggests that the incentive may be successful in expanding gross activity, but much less successful in converting that growth into fiscal returns. The most plausible interpretation, and the one the paper itself points toward, is that the tax holiday lowers the effective tax burden enough that profit expansion does not translate proportionately into revenue. Put differently, the state may be subsidizing scale without securing a durable fiscal payoff. For a revenue-constrained country, that is a serious trade-off. It implies that growth effects alone are not sufficient grounds for maintaining a generous tax holiday unless those gains are large, persistent, and clearly linked to broader structural transformation.

Exporter incentives show modest gains but may reflect stronger firms rather than true policy effects. Beneficiary exporters display stronger CAPEX levels, higher export levels, modest sales gains, and a moderate increase in taxes paid overtime relative to matched controls. Unlike the strategic-sector case, the exporter regime seems more consistent with the idea that incentives may help firms sustain external-market participation and scale activity enough to enlarge the tax base, even if rates are reduced. But the paper also warns that exporters appear to have been stronger firms to begin with. The pre-treatment divergence noted in several exporter outcomes points to selection: beneficiaries may already have been larger, more dynamic, or more growth-oriented firms. This weakens any strong causal interpretation and suggests that part of the observed exporter "success" may reflect who got the incentive, not only what the incentive did.

Tax holidays create jobs, but there is a scope for improving job quality and true additional employment. The 10-year tax holiday appears to support substantial job creation. More than 12,600 jobs are associated with CITE 4 recipients and about 4,700 with CITE 5 recipients. However, the quality of jobs likely differs sharply across sectors. Jobs in manufacturing and agro-processing are more likely to be formal, productive, and linked to skills accumulation and supplier development. Jobs in construction and some services may be temporary, low-skill, and weakly connected to long-term transformation. In addition, some employment may reflect labor reallocation across firms rather than net economy-wide job creation. This means that the employment case for the incentive is strongest only when jobs are productive, durable, and additional, not merely numerous.

Beneficiaries meet local hiring rules but may not increase Ugandan employment relative to non- benefitting firms. The paper finds that beneficiaries generally satisfy the legal threshold requiring a high share of Ugandan employees or wage allocation to nationals. That is important from a compliance perspective. However, beneficiaries may employ fewer Ugandans than comparable controls while remaining above the legal threshold. Therefore, there is a scope for a large causal increase in local employment intensity.

Tax incentives raise output but do not strengthen local sourcing or domestic linkages. One of the most policy-relevant findings in the paper is the weak performance on local sourcing. For qualifying sectors, treated firms remain below the 70 percent local-input benchmark and appear to reduce local input use after the policy relative to controls. For exporters, effects are small. This matters because local sourcing is one of the main channels through which incentives are supposed to generate domestic spillovers. If firms respond to the incentive mainly by expanding imported machinery and imported intermediate-input use, then the policy may still raise firm-level activity while generating weaker domestic

multiplier effects than intended. This seems especially plausible in sectors where domestic supplier networks are thin or unable to meet quality and scale requirements. Therefore, the 10- year tax holiday may be increasing output without proportionately strengthening local production links. That weakens the structural-development case for broad-based tax holidays.

5. Policy Options

The evidence supports replacing broad tax holidays with targeted, performance-based incentives focused on high-impact sectors and fiscal efficiency. The evidence strongly supports moving from broad, untargeted tax holidays to a selective, performance-based, and fiscally disciplined incentive regime that prioritizes sectors with demonstrable spillovers and aligns incentives with Uganda's structural transformation goals.

Shift from broad tax holidays to targeted incentives. The current system performs well only where there are strong value chains and productivity spillovers. Retain incentives only in high-return sectors such as manufacturing and export-oriented agriculture, where BCRs are significantly above 1 and strong spillovers exist. Reform incentives in low-return sectors such as construction, transport, and some services where evidence shows near-zero returns and deadweight loss.

Replace long tax holidays with reduced time-bound rates. The evidence shows strong sales growth but weak and un-sustained tax revenue, implying excessive erosion of the tax base. Introduce declining benefit structures, where there is front-loaded support tapering over time. Link incentives to performance conditions such as:

i. Export growth (verified export ratios)
ii. Local input sourcing thresholds
iii. Employment quality (formal, skilled jobs)

iv. Value addition benchmarks

Strengthening domestic supply chains instead of subsidizing imports. Given that firms expand using imported inputs rather than deepening domestic linkages, weakening multiplier effects, there is a need to complement incentives with:

i. Supplier development programs
ii. Local content financing schemes
iii. Industrial clustering and logistics support

i. Introducing Sector-Specific Incentive Design. Given that sector heterogeneity is large, a uniform policy is inefficient. There is a need to:
ii. Retain and possibly deepen support for Manufacturing and agro-processing given its high spillover potential
iii. Remove fiscal incentives for Construction and transport and focus on regulatory efficiency instead
iv. Justify support for social sectors on social returns, not fiscal returns

Improve Targeting and Selection Mechanisms. Evidence shows strong selection effects, where beneficiaries are often already high-performing firms. Therefore, there is a scope to introduce transparent eligibility criteria based on:

i. Productivity potential
ii. Export capability
iii. Investment additionality
iv. Use ex-ante scoring models rather than discretionary approval

Strengthening monitoring, evaluation, and exit rules. Current incentives persist even when outcomes local sourcing and tax revenue do not materialize. There is a scope for institutionalizing:

i. Annual performance audits
ii. Mid-term reviews (e.g., after 3–5 years)

iii. Automatic withdrawal for non-performance

Shift from Tax Incentives to Structural Reforms for Export Promotion. This is because the study shows tax holidays are ineffective tools for export promotion in qualifying sectors. Therefore, there is a need to address:

i. Trade logistics
ii. Standards and certification
iii. Access to finance
iv. Infrastructure

Focus on quality, not just quantity of jobs. While employment gains exist, they may reflect low-quality or reallocated jobs rather than net structural transformation. Therefore, introduce incentives tied to:

i. Wage levels
ii. Skill intensity
iii. Job permanence

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Appendix 1

Table A1: Cost Benefit Analysis of CIT Tax Holidays by ISIC Sector for period 2021/22 to 2024/25

Cost - Benefit Analysis Variable	All CIT - Recipients	A- Agriculture, forestry and fishing	C- Manufacturing	F- Construction	G- Wholesale and retail trade; repair of motor vehicles and motorcycles	H- Transportation and storage	Q- Human health and social work activities	S- Other service activities
CITE 4 - Strategic Investors	46	7	25	1	7	1	3	2
CITE 5 - Exporters	18	8	9		1			
Employment (CITE 4)	12602	2103	5880	23	667	8	3853	68
Employment (CITE 5)	4731	2357	2358		16			
BCR (CITE 4)	2.49	2.26	5.49	0.02	0.95	0.00	0.23	0.42
BCR (CITE 5)	1.854	4.784	1.592		0.003			
BCR (Overall)	5.03	7.05	7.08		0.96			
Benefits (CITE 4) (UGX Billions)								
PAYE	32.38	4.43	17.50	0.003	1.84	0.03	8.22	0.36
Domestic VAT	59.43	0.45	56.00	0.28	2.37	0.06	0	0.28
Import VAT	219.92	2.60	183.10	0	32.46	0	0.20	1.56
Excise Duty	0.36	-	0.36	-	-	-	-	-
Capital Stock (Plant and Machinery)	1,003.78	145.70	817.20	0.027	12.46	0	14.64	13.75
Benefits (CITE 5)								
PAYE	6.00	4.03	1.93		0.04			
Domestic VAT	5.71	1.06	4.64		0			
Import VAT	3.91	2.63	1.28		0			
Excise Duty	0.00	-	-		0			
Capital Stock (Plant and Machinery)	236.06	130.2	105.80		0.062			
Costs (CITE 4) (UGX Billions)								
CIT Forgone	101.22	9.14	68.00	0.01	8.71	0.41	15.2	-2.43E-01

Cost - Benefit Analysis Variable	All CIT - Recipients	A- Agriculture, forestry and fishing	C- Manufacturing	F- Construction	G- Wholesale and retail trade; repair of motor vehicles and motorcycles	H- Transportation and storage	Q- Human health and social work activities	S- Other service activities
Admin. Cost	290.07	46.10	36.47	14.31	31.05	62.00	62.00	38.15
Compliance Cost	2.21	0.223	0.50	0.002719	0.11	0.01	1.30	0.07
Interest paid on additional Debt	15.18	1.37	10.20	0.00	1.31	0.06	2.28	-0.04
Development Expenditure on Infra. to accommodate debt	120.00	10.84	80.62	0.01	10.33	0.48	18.02	-0.29
Per capita cost (CIT Forgone divided by employment) UGX Billions	0.8%	0.4%	1.2%	0.0%	1.3%	5.1%	0.4%	-0.4%
Per Capita PAYE as % of per capita cost	6.12%	6.55%	8.94%	0.02%	3.57%	0.05%	8.32%	0.94%
Costs (CITE 5) (UGX Billions)								
CIT Forgone	25.51	4.44	19.40		1.67			
Admin. Cost	66.23	16.69	18.53	-	31.00			
Compliance Cost	0.19	0.07	0.12		0.01			
Interest paid on additional Debt	3.83	0.67	2.91	-	0.25			
Development Expenditure on Infra. to accommodate debt	40	6.96	30.42	-	2.62			
Per capita cost (CIT Forgone divided by employment)	0.5%	0.19%	0.82%		10.44%			
Per Capita PAYE as % of per capita cost	4.42%	13.98%	2.70%		0.12%			

Source: Authors calculations using URA Data

Appendix 2: Methodology for Estimating the Effect of the Strategic Sector CIT Policy

The 2018/19 policy timing allows a clear before–after comparison using difference-in-differences. The strategic sector policy introduced in FY2018/19 provides a discrete and observable treatment timing. Unlike the exporter tax holiday case, this setting allows for a credible before–after comparison, enabling the use of a difference-in-differences (DiD) framework. The baseline specification is:

$$Y_{it} = \alpha_i + \lambda_t + \beta(Treat_i \times Post_t) + \varepsilon_{it}$$

where:

a. $Treat_i$ =1if firm i qualifies for the strategic sector incentive,
b. $Post_t$=1for t≥2018,
c. Y_{it} denote firm outcomes (e.g., CAPEX, exports, employment, value added, tax paid).
d. α_i: firm fixed effects,
e. λ_t: year fixed effects,
f. β: average treatment effect on the treated (ATT).

The study uses an event-study model to track changes over time. To capture adjustment dynamics and assess identification, the study estimates a dynamic event-study model:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k \neq -1} \beta_k \, (Treat_i \times 1[t = k]) + \varepsilon_{it}$$

where:

i. k indexes event time relative to 2018 (e.g., $k = -3, -2, -1, 0, 1, 2, \ldots$),
ii. $k = -1(2017)$ is the omitted baseline,
iii. $\beta_k$traces outcome differences between treated and control firms before and after policy implementation.

The coefficients for the period before the policy are used to test whether treated and control firms were following similar

trends. The coefficients for the period after the policy show how the impact of the policy evolves over time.

i. Pre-policy coefficients ($k < 0$): test for parallel trends.
ii. Post-policy coefficients ($k \geq 0$): estimate dynamic treatment effects.

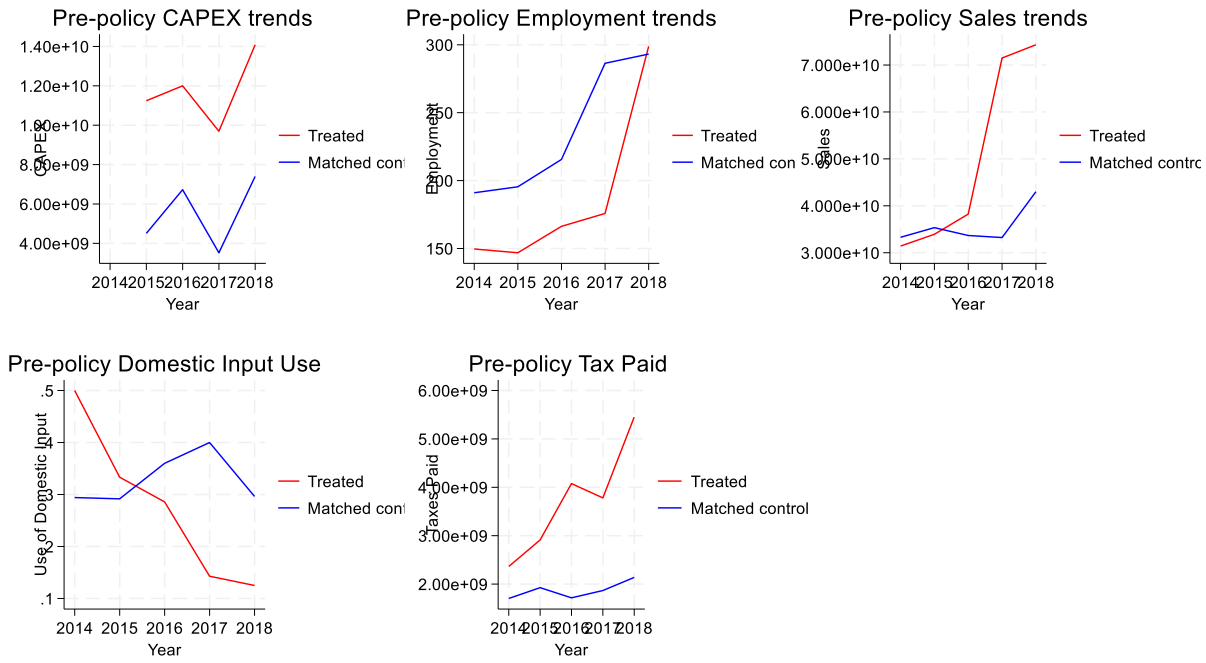
Strategic firms are selected on key traits, so we match similar firms using pre-2018 characteristics. Strategic sector firms are selected based on capital expenditure, exports, sales, employment, value added, profitability, domestic input use and tax payments. To improve comparability, the control group is constructed using Mahalanobis matching using pre-2018/19 outcomes. The results on Table A2 and Figure A1 suggest that treated (beneficiaries) and control (non-beneficiaries) where very similar on outcomes before 2018/19, with exception of local wages, value addition and taxes paid, where treated firms were doing much better. The results for local wages, value addition and taxes paid are significant.

Table A2: Parallel Trends Formal Results

	CAPEX (millions)	**Employment**	**Value Addition (millions)**	**Sales (Millions)**	**Domestic Inputs**	**Local Employment**	**Taxes Paid (million)**	**Local Wages**
Coefficient	-123.00	-21.58	41,700**	8,720.00	-0.051	-0.002	647**	0.000251***
[p-value]	[0.894]	[0.642]	[0.024]	[0.175]	[0.334]	[0.332]	[0.035]	[0.00]

Source: Authors calculations using URA Data

Figure A1: Parallel Trends Visuals



Source: Authors Calculation using URA data

The interpretation of β is the average post-2018 impact, while β_k shows timing and persistence. The estimated coefficient βcaptures the average change in outcomes for strategic-sector firms after 2018/19, relative to similar non-beneficiary firms. Dynamic coefficients $\beta_k$provide timing of response (immediate vs delayed), persistence of effects, evidence on whether policy affects levels or growth rates.

The limitation is that unobserved factors, weak trends, and spillovers may bias results. Selection into treatment may still depend on unobservable (e.g., political prioritization, firm quality). Also, imperfect parallel trends may bias estimates if not adequately addressed. In addition, spillovers (e.g., competition, supply chains) may violate stable unit treatment assumptions.

Appendix 3: Methodology for Estimating the Effect of the CIT Tax Holiday for Exporters

The exporter CIT tax holiday was introduced in 2008, but firm-level data are only available from 2012 onward. This creates an important identification problem. Treated firms are already exposed to the policy by the time they first appear in the dataset. As a result, the study cannot recover the full causal effect of introducing the tax holiday in 2008, because no observed pre-treatment period exists for the treated firms.

Accordingly, the empirical strategy must be framed more narrowly. Rather than estimating the effect of the initial policy introduction, the analysis estimates the medium-run post-2012 effect of being a tax holiday beneficiary, relative to comparable non-beneficiary firms. In other words, the design identifies whether treated firms perform differently from similar untreated firms from 2012 onward, not the total effect from 2008.

In this case, the study estimates how do firms benefiting from the exporter CIT tax holiday perform after 2012 relative to similar firms that did not receive the holiday. The main observation is the average effect on benefitting firms (Average Treatment of the Treated (ATT)) for the observed post-2012 period. The outcomes are like those of qualifying sectors.[4]

The estimates should be interpreted as incremental differences in post-2012 trajectories. Because treatment is already active when observation begins, these estimates should be interpreted as incremental differences in post-2012 trajectories, not the causal impact of the original 2008 adoption.

Results show differences after 2012, not the original 2008 policy effect. Because we lack earlier data and have few firms, we compare similar firms from 2012 onward to estimate the policy's impact. Given the absence of observed pre-2008 data for benefitting

[4] capital expenditure, exports, sales, employment, value added, profitability, domestic input use and tax payments

firms and the small number of benefitting firms (18 firms), the most appropriate methodology is a matched panel design with firm fixed effects, and event-time interactions anchored at the first observed year (2012).

We compare beneficiary firms to similar non-beneficiaries to ensure a fair comparison. We identify non- benefitting firms (control group) that resemble benefitting firms (treated group) as closely as possible. The selected non-benefitting firms are drawn from firms that operate in similar sectors; exporters or potential exporters; are of similar size; have similar asset base, turnover, employment, and profitability; and were eligible or near eligible for the incentive but did not receive it.

We match similar firms using key characteristics to ensure fair comparison. Because tax holiday receipt is unlikely to be random, simple treated-versus-untreated comparisons would suffer from selection bias. Mahalanobis matching is therefore used to improve comparability. Given the small-treated sample, a practical approach is to combine with nearest-neighbour matching on baseline firm characteristics measured in 2012. The matching covariates include log assets, log sales, sector, and location.

We then compare how benefitting firms and similar non-benefitting firms evolve after 2012. After matching, the main model compares the evolution of outcomes for treated and control firms from 2012 onward. Since all treated firms are already treated in 2012, treatment itself does not vary over time. However, one can still test whether treated firms experience different post-2012 outcome paths relative to controls. The baseline specification is:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k=2013}^{T} \beta_k \left(Treat_i \times 1[t = k]\right) + \varepsilon_{it}$$

where:

i. $Y_{it}$is the outcome for firm $i$in year t,
ii. $\alpha_i$are firm fixed effects,
iii. $\lambda_t$are year fixed effects,

iv. $Treat_i$is an indicator equal to 1 for tax holiday beneficiaries,
v. $1[t = k]$is a year dummy,
vi. $\beta_k$captures the additional outcome change for treated firms in year k, relative to the control group and relative to the baseline year 2012.

Results compare post-2012 trends, not treatment entry effects. The coefficients $\beta_k$do not measure the effect of entering treatment in year k. Instead, they measure whether treated firms' outcomes evolve differently from those of matched untreated firms after 2012, taking 2012 as the reference year. This is therefore a post-treatment comparative panel design, not a standard pre/post difference-in-differences design.