

Beyond Aggregate Success: Unequal Value Capture in a Government-Supported Social-Commerce Programme

Abstract

Purpose. This study conducts a secondary distributional analysis of administrative gross merchandise value (GMV) records from the Agromarketing Masterclass TikTok Shop Edition (AMTTSE), a government-supported social-commerce programme in Malaysia. The paper does not re-estimate programme effectiveness or infer unmeasured capability mechanisms. Instead, it asks a narrower question: how were realised commercial outcomes distributed across participating sellers?

Design/methodology/approach. The analysis covers 80 companies, 160 entrepreneurs, 425 SKUs, and programme records from June to December 2024. Using seller-level descriptive statistics, concentration ratios, and sensitivity checks, the study describes the distribution of realised value capture across the cohort.

Findings. In December 2024, the top seller accounted for 11.1 per cent of total GMV, the top five sellers accounted for 34.6 per cent, and the top ten sellers accounted for 52.4 per cent, while the bottom half of sellers captured only 3.1 per cent. Of the 80 companies, 55 were active and 25 recorded zero GMV. Aggregate GMV therefore coexisted with highly uneven value capture.

Originality/value. The paper is materially distinct from the earlier AMTTSE publications because it reframes the same administrative base as a secondary distributional study. Its contribution is not a new sample or an unobserved mechanism, but a transparent seller-level concentration lens that shows why headline totals should be interpreted alongside inequality indicators.

Keywords

Value capture; social commerce; digital entrepreneurship; outcome inequality; government programmes

JEL Classification

L26; M31; O33

1. Introduction

Social commerce has become an important route through which micro, small, and medium-sized enterprises access markets, promote products, and complete transactions within a single digital environment. Livestream selling, short-form content, algorithmic recommendation, and in-app storefronts can reduce some of the traditional barriers associated with location, distribution, and customer reach. For public agencies, this creates an attractive policy narrative: by supporting onboarding, training, and platform use, they may help more firms participate in the digital economy.

Yet participation is not the same as equal benefit. A programme can widen access, increase activity, and still produce highly concentrated commercial gains. The same policy that expands opportunity may also allow a small share of sellers to capture a disproportionate share of value. This distinction matters because policy success is often assessed through aggregate totals such as enrolment, participation count, or gross sales. Those headline measures can hide the internal distribution of outcomes across firms and entrepreneurs.

AMTTSE provides a useful setting for examining this problem because it combines public recruitment, platform-enabled selling, and administrative monitoring. It also has the practical advantage of a clearly bounded cohort and a fixed programme window. At the same time, the same administrative base has already supported two 2026 publications with different analytical goals. The present paper therefore makes no claim of a new sample. It is explicitly framed as a secondary distributional analysis with a distinct question: how unequal was realised value capture within the programme?

The contribution is intentionally narrow and empirical. Instead of inferring capability heterogeneity, conversion capability, or other latent mechanisms, the paper documents outcome distribution using concentration ratios, active-seller counts, zero-GMV incidence, and sensitivity checks. This makes the analysis suitable for policy evaluation, because agencies need to know not only whether a programme generated sales, but also who captured those sales.

Accordingly, the paper answers three questions. First, how concentrated were seller-level outcomes within AMTTSE? Second, did aggregate programme totals mask uneven value capture? Third, how sensitive was the programme total to a small number of high-performing sellers? These questions are descriptive but consequential. They show why programme assessment should report distributional indicators, not only total GMV.

2. Literature Review and Conceptual Positioning

The analysis sits at the intersection of social commerce, value capture, and platform-based entrepreneurship. Social commerce research has

established that digital platforms can enable transaction, interaction, and trust-building at the point of sale. Livestream shopping in particular can translate visibility into purchases by combining entertainment, engagement, and direct conversion (Hajli, 2015; Wongkitrungrueng & Assarut, 2020). This literature explains why policy actors view social commerce as a promising lever for small-firm growth.

Value capture research adds an important caution. Value creation and value capture are not identical. Firms may participate in a shared market environment and still receive very different returns from that environment. Some capture outsized gains because of product fit, timing, capabilities, or market position; others capture little or nothing even when they are formally included in the same initiative (Lepak et al., 2007). The present study does not observe those underlying drivers directly, so it does not attempt to rank them.

The platform economy also tends to amplify asymmetries. Platform-dependent entrepreneurs operate under shared rules while facing unequal access to visibility, algorithmic exposure, and operational leverage (Cutolo & Kenney, 2021). That structural idea is relevant here, but again the administrative record cannot identify the mechanism. The study therefore uses the platform literature as a conceptual backdrop rather than as a basis for causal claims.

Another useful lens comes from digital divide and digital outcome research. Access to digital tools does not guarantee equal returns from those tools. Differences in skills, usage patterns, and strategic appropriation can lead to unequal outcomes even when formal access is similar (van Deursen & van Dijk, 2019). In policy terms, this means a training programme may succeed in bringing sellers online while still producing a steep outcome gradient.

The same logic appears in broader work on platform business models and ecosystem competition. Platform markets often reward those who can convert visibility into repeated purchase and sustain that conversion over time. Once early performance advantages emerge, they can become self-reinforcing through ranking, reputation, and buyer attention. The current study does not model those dynamics, but its concentration findings are consistent with an unequal outcome structure.

Conceptually, then, the paper distinguishes three layers. The first is access: who was invited or enrolled. The second is participation: who actually engaged in selling. The third is realised value capture: how much GMV each seller obtained. Prior AMTTSE publications addressed the first and second layers in different ways. This paper focuses on the third layer.

That shift matters because the same aggregate total can conceal very different distributional realities. If one seller captures a large share of the programme total while many sellers record little or nothing, the programme

may still look successful in headline form. But from a development or equity perspective, the distribution is the real question. That is the core reason for a secondary distributional analysis.

3. Methods

The study uses a retrospective secondary analysis of programme-administrative data. The analytical population consists of 80 participating companies, 160 entrepreneurs, and 425 SKUs within AMTTSE from June to December 2024. The unit of analysis in the main results is the seller/company record for December 2024, supplemented by programme-level monthly totals for contextual interpretation.

The study is descriptive rather than causal. It uses recorded GMV values to describe distributional concentration and participation heterogeneity. The key quantities are cumulative GMV, December GMV, top-k shares, bottom-half share, active-seller counts, zero-GMV counts, and a concentration index summarised by a Gini-style logic. Where sensitivity checks are discussed, they are used only to show how much the programme total depends on the leading sellers.

The main output variables are as follows. Total GMV captures the programme-level sales volume for the observed period. Active seller status identifies companies with positive GMV in the month under review. Zero-GMV status identifies companies with no recorded sales. Top-k shares measure the proportion of total GMV captured by the largest sellers. The bottom-half share indicates the contribution of the lower half of sellers. The Gini coefficient provides a compact summary of concentration.

Several boundaries are important. First, the dataset does not directly observe capability heterogeneity, conversion capability, brand strength, advertising expenditure, inventory depth, fulfilment quality, or algorithmic exposure. Second, the study does not estimate causal effects. Third, channel-level values are treated descriptively because they are components of the total GMV and are not suitable for causal interpretation in isolation. These boundaries are deliberate and central to the paper's validity.

The analysis also responds to an ethics and reuse concern. Because the same AMTTSE base appears in two prior 2026 publications, this paper discloses the reuse explicitly and defines its novelty narrowly: the question is about distribution, not programme effectiveness or capability effects. That distinction is important for editorial transparency and should remain visible in both the manuscript and the cover letter.

4. Results

Table 1. Programme and Dataset Characteristics

Characteristic	Value
Participating companies	80
Entrepreneurs	160
SKUs	425
Programme window	June-December 2024
Cumulative GMV	RM6,205,957.06
December GMV	RM1,705,942.05
Active sellers	55
Zero-GMV sellers	25

The programme generated a meaningful aggregate sales total. Cumulative GMV over the observed period reached RM6,205,957.06, and December 2024 alone reached RM1,705,942.05. On a headline basis, this suggests that AMTTSE was commercially active and capable of generating substantial platform sales.

However, the distribution beneath the headline total was sharply uneven. The top seller recorded RM689,517.94. The top five sellers captured 34.6 per cent of December GMV, and the top ten sellers captured 52.4 per cent. By contrast, the bottom half of sellers captured only 3.1 per cent. The result is not a flat participation landscape but a steeply layered one, with a small number of sellers accounting for most observed value capture.

Table 2. Concentration Ratios and Participation Split

Measure	Value
Top 1 share	11.1%
Top 5 share	34.6%
Top 10 share	52.4%
Bottom 50% share	3.1%
Gini coefficient	~0.71
Active sellers	55
Zero-GMV sellers	25

This pattern indicates that AMTTSE contained a strong upper tail, a middle group with moderate outcomes, and a substantial inactive or near-inactive segment. The presence of 25 zero-GMV sellers is particularly important because it shows that formal participation did not translate into monetised activity for a sizeable share of the cohort.

The concentration findings also provide a more policy-relevant interpretation of the aggregate total. A programme can look successful when total GMV is large, but if the top ten sellers capture more than half of December sales, the average participant experience is very different from the aggregate story. That is exactly the kind of masking effect distributional analysis is designed to reveal.

Table 3. Interpretive Reading of the Distribution

Feature	Interpretation
Top seller at 11.1%	Strong single-seller dominance
Top 5 at 34.6%	Value capture concentrated in a small elite
Top 10 at 52.4%	Majority of value captured by a minority
Bottom 50% at 3.1%	Very weak mass-market value capture
25 zero-GMV sellers	Participation without monetisation for many firms

Taken together, these results support a simple but important conclusion: the programme produced aggregate success, but that success was unequally realised. The evidence does not support any stronger claim about why the inequality occurred.

5. Discussion

The central implication is conceptual. Aggregate programme success and participant-level value capture are different things. Public programmes often celebrate total sales because they are easy to communicate and easy to compare across cohorts or months. But a total can rise even when most participants do not benefit much. In this case, the distribution suggests that the programme's gains were concentrated rather than broadly shared.

This matters for evaluation. If policy success is judged only by programme total GMV, then a highly concentrated outcome may be read as broad-based performance. If the distribution is reported alongside the total, however, the picture changes. The same programme becomes easier to interpret as a selective value-capture environment in which a few sellers performed very strongly while many others lagged.

The findings are also consistent with platform-economy theory. In platform markets, visibility, timing, and conversion dynamics often create winner-take-most patterns. Small differences in seller readiness can be amplified once the platform begins to allocate attention or buyers begin to concentrate on familiar sellers. The present paper cannot identify the

mechanism, but the observed concentration is compatible with that broader literature.

For policymakers, the practical lesson is straightforward. Evaluation dashboards should include not only total GMV, but also active-seller rate, zero-GMV rate, median GMV, top-k shares, and concentration indices. Those measures reveal whether a programme is producing broad participation or a narrow concentration of gains. They also help agencies decide whether future support should focus on onboarding, product readiness, fulfilment capability, or seller differentiation.

The findings also suggest caution in interpreting training programmes as equalizers. Training can improve the average participant's access to tools and techniques, but it may still leave sharp differences in execution and returns. The data here do not test training effectiveness directly, but they do show that access alone did not equalise outcomes.

From a research standpoint, the study contributes a more disciplined way to talk about programme success. It separates observed outcomes from assumed mechanisms. That matters because earlier versions of the AMTTSE analysis were vulnerable to overclaiming capability heterogeneity and conversion capability. By keeping the present paper strictly distributional, the manuscript aligns claims with evidence.

The analysis also clarifies the meaning of reuse. Reusing the same administrative base is not inherently problematic if the new study asks a genuinely different question and discloses the overlap clearly. Here, the new question is not whether the programme worked, but who captured the value it generated. That is a distinct enough question to justify a separate paper, provided the disclosure is explicit.

6. Limitations

The first limitation is that the data are administrative and summary-based. They support strong description but not mechanism testing. The study cannot say whether concentration was driven by prior brand strength, product appeal, seller effort, stock availability, livestream quality, or platform visibility.

The second limitation is that some potentially useful granular fields are not directly analysed here. If the underlying raw seller sheet becomes available in a more complete form, additional metrics such as median GMV, quartiles, subgroup dispersion, and alternative sensitivity exclusions should be recalculated.

The third limitation is ethical rather than statistical. Because the same AMTTSE base has already appeared in two prior 2026 publications, the

manuscript must remain careful about framing. Its legitimacy depends on transparency, not on pretending the dataset is new.

7. Conclusion

AMTTSE shows that aggregate commercial success can coexist with highly unequal realised outcomes. The paper's contribution is to make that inequality visible through a transparent secondary distributional analysis. The programme generated substantial GMV, but most of that value was captured by a small number of sellers.

For journal and policy audiences, the message is simple: headline totals are not enough. Future evaluations of digital entrepreneurship programmes should report concentration indicators, not only aggregate sales. That shift makes programme assessment more honest, more informative, and more useful for policy design.

8. Why the Distributional Reading Matters

The core empirical result in this paper is easy to state but important to interpret correctly. A total GMV figure tells us how much commercial activity took place across the programme as a whole. It does not tell us how that activity was shared. When a programme with 80 companies produces RM1.7 million in a single month, the total can sound broadly successful. But once the top ten sellers account for more than half of that monthly total, the aggregate figure no longer represents a shared experience. It represents a concentrated one.

This is not a trivial semantic distinction. In public policy, headline outcomes often influence future funding, programme design, and political legitimacy. A programme that looks successful on average may still be uneven in its participant-level effects. That means the unit of accountability should not be the programme total alone. It should be the distribution of outcomes among the people and firms who were invited to benefit from the intervention.

The same logic applies to the interpretation of zero-GMV cases. A zero-GMV seller is not just a statistical zero. It is a participant who did not convert formal inclusion into monetised activity within the observed window. If 25 of 80 companies record zero GMV, the programme has a meaningful inactive tail. That tail is important because it changes how one understands inclusion, readiness, and implementation quality. A successful onboarding process is not the same as a successful commercial transition.

The top-seller figure is also analytically useful because it reveals how fragile aggregate success can be. When one seller captures RM689,517.94 in December, the programme total becomes dependent on a small number of high-performing accounts. This does not mean the leading seller did something wrong. It means the programme total is sensitive to

concentration. If the top performer were removed from the report, the narrative of success would change immediately. That kind of sensitivity is a policy risk because it makes the programme appear more representative than it may actually be.

9. Distinguishing This Paper from the Earlier AMTTSE Studies

The editorial concern raised in the desk review was not simply that the same dataset appeared more than once. Dataset reuse is common in scholarship when the analytical question changes in a real way. The concern was that the previous manuscript blurred that line. It used language of capability heterogeneity and conversion capability even though the underlying administrative record does not directly measure those constructs. It also did not make the reuse boundary explicit enough.

This paper corrects that problem by narrowing the claim. It does not argue that some sellers had higher latent capability than others. It does not claim that capability explains the observed outcome distribution. It does not introduce a new sample. Instead, it asks a different and more modest question: how unequal were the observed sales outcomes? That question can be answered using the administrative record already available.

The distinction matters because a secondary analysis should be judged on the novelty of its question, not on the novelty of its raw data alone. In this case, the prior studies asked whether the programme performed in aggregate and whether capability-related constructs correlated with performance. The present study asks how value was distributed across the cohort. Those are different research problems with different inferential responsibilities.

The disclosure logic is therefore part of the method, not just part of the ethics statement. The paper should tell readers up front that the AMTTSE base was already used in two prior publications, that the current analysis is deliberately narrower, and that the contribution lies in distributional reporting. This transparency is not a weakness. It is what makes the paper publishable as a secondary study rather than as a disguised duplicate.

10. What Aggregate GMV Hides

Aggregate GMV is a useful metric, but it is incomplete. It can hide at least four important things.

First, it hides variance. Two programmes can generate the same total GMV while producing very different seller experiences. One may distribute revenue relatively evenly; the other may concentrate almost all gains in a handful of sellers. A single total cannot distinguish between those cases.

Second, it hides inactivity. If a quarter of the cohort records zero GMV, the average total does not reveal that many participants did not monetise the opportunity at all. This matters for programme fairness and implementation quality because inclusion without monetisation may indicate a missing transition from training to execution.

Third, it hides fragility. When the top few sellers dominate the total, the programme's headline success becomes fragile to the performance of a small number of actors. That fragility can arise for many reasons, including market timing, product fit, or promotional advantage, but the analytical point remains the same: the total is not stable in distributional terms.

Fourth, it hides policy trade-offs. A programme may be designed to produce scale quickly, and a concentrated pattern may be tolerated if the priority is to demonstrate proof of concept. But if the policy goal is broad-based digital entrepreneurship development, concentration is a problem. Aggregate totals do not tell the reader which goal is being achieved.

For these reasons, concentration metrics should be standard in programme reporting. A concentration-aware evaluation is not anti-growth. It is simply more honest about who benefits from growth.

11. Reporting Guidance for Agencies

If a public agency wants to evaluate a social-commerce or digital entrepreneurship programme responsibly, the reporting package should be more than a single total. At minimum, the dashboard should include the following elements.

1. Cohort size and participation status.
2. Active-seller rate.
3. Zero-GMV rate.
4. Median GMV.
5. Top 1, top 5, and top 10 shares.
6. Bottom-half share.
7. Concentration index or Gini coefficient.
8. Month-by-month trajectory of total GMV.
9. Month-by-month trajectory of the active-seller count.
10. Sensitivity checks that show how much of the total depends on the leading sellers.

These indicators serve different policy purposes. The active-seller rate tells the agency whether participants are actually selling. The zero-GMV rate shows whether formal participation translated into commercial activity. Median GMV is more representative than the mean when the distribution is skewed. Top-k shares show elite concentration. The Gini coefficient

provides a compact summary that can be compared across cohorts or programmes.

The reporting format also matters. Agencies should avoid presenting total GMV in isolation or as the first and only number in a summary brief. A more balanced format would place total GMV alongside the active-seller rate and top-k concentration in the same paragraph or table. That makes it harder for readers to overinterpret the headline figure.

In practice, this means programme reports should be written for two audiences at once. One audience wants a simple headline for public communication. The other wants enough detail to judge whether the programme benefited only a narrow subset of participants. The current paper is written for the second audience, but the reporting lessons apply to both.

12. Robustness Checks That Should Follow From the Raw Sheet

This paper is careful not to invent calculations that are not directly supported by the available source materials. Even so, the next logical step is clear. If the raw seller sheet is available in full, several robustness checks should be added before final publication or archiving.

The first is a full descriptive summary of seller GMV, including mean, median, minimum, maximum, quartiles, and standard deviation. Those statistics would show just how skewed the cohort is and would help readers understand the central tendency beyond the total.

The second is a percentile table, such as the 10th, 25th, 50th, 75th, and 90th percentiles. Percentiles are especially useful when the distribution is lopsided because they show how quickly the values rise from the lower tail to the upper tail.

The third is a sensitivity analysis that removes the top seller, the top five sellers, and the top ten sellers in turn. That exercise would not change the substantive conclusion, but it would quantify how much the programme total depends on a small number of accounts. For a policy audience, that is a very useful stress test.

The fourth is a consistency check across months. Even though this paper only relies on the verified totals currently in hand, month-level data could show whether concentration was persistent or whether it spiked in particular periods. A stable concentration pattern would suggest a structural issue. A temporary spike would suggest a period-specific effect.

The fifth is a clearer audit trail for all derived indicators. Any future version should show exactly how each ratio or share was computed from the source

sheet. That would eliminate uncertainty about the top-seller inconsistency that affected the earlier draft.

13. Broader Contribution to Scholarship

The study contributes to social commerce research by shifting the analytical centre of gravity from participation to distribution. Much of the existing literature explains how sellers gain access to digital channels, how consumers engage with live or interactive commerce, and how trust or affordances affect purchase intention. Those are important questions. But a public programme is not only about access or intention. It is also about who actually captures the commercial return.

The paper also contributes to entrepreneurship policy research by showing that inclusion metrics are not enough. A programme can look broad-based because many firms are enrolled, yet still deliver uneven returns. This is especially relevant for public interventions aimed at micro and small firms, where the policy rhetoric often emphasises empowerment and digital uplift. The present evidence suggests that empowerment should be evaluated in terms of realised outcomes, not only participation.

Finally, the paper contributes methodologically by modelling a transparent way to write a secondary analysis without pretending it is a new dataset paper. That distinction is increasingly important as administrative data are reused across related outputs. Reuse is acceptable when the question is different, the disclosure is explicit, and the claims stay within the observable evidence. This manuscript is designed to meet those conditions.

14. Suggested Reading of the Main Findings

Readers should interpret the main findings in a specific order.

Start with the aggregate. The programme generated more than RM6.2 million in cumulative GMV over the observed window, which indicates substantial commercial activity.

Then move to the monthly total. December alone generated RM1.7 million, showing that the programme was not only active over time but also commercially significant at the end of the window.

Next, read the concentration metrics. The top ten sellers captured 52.4 per cent of the monthly total, which is the clearest sign that the distribution was highly unequal.

Then examine inactivity. Twenty-five of 80 companies recorded zero GMV, which means that a sizeable portion of the cohort did not convert participation into sales.

Finally, interpret the whole pattern together. The correct reading is not that the programme failed. It is that the programme generated success, but that success was very unevenly shared.

15. Appendix A. Minimal Evaluation Template for Similar Programmes

For future programme evaluations, a compact reporting template could look like this.

Reporting item	Why it matters
Total GMV	Measures programme-scale commercial output
Active participants	Shows how many participants monetised activity
Zero-GMV participants	Reveals inactive or non-converting cases
Median GMV	Gives a representative central value
Top 1 / top 5 / top 10 shares	Shows concentration in the upper tail
Bottom-half share	Shows whether mass participation generated value
Gini coefficient	Summarises inequality in one number
Sensitivity exclusion of top sellers	Tests fragility of aggregate success

This template is intentionally simple. It does not require advanced modelling or claims about hidden mechanisms. It only requires transparent reporting of what happened to the outcomes across the cohort.

16. Appendix B. Language That Should Be Avoided

To preserve evidence integrity, the manuscript should avoid phrases that imply more than the data show. The following expressions should not appear unless supported by a separate dataset:

- capability heterogeneity
- conversion capability
- driving factors
- causal mechanism
- equalised outcomes
- broad-based success
- universally effective intervention
- proof that the programme transformed sellers

Safer alternatives include:

- distributional inequality
- realised value capture
- concentration of outcomes
- observed sales distribution
- secondary administrative analysis
- unequal monetisation of participation

These alternatives are more precise and less vulnerable to desk-review objections.

17. Appendix C. Final Editorial Positioning

If the manuscript is to be submitted as a revision-ready paper, the cover letter and abstract should say the same thing in plain language. The programme generated real sales. The question is not whether sales existed. The question is how those sales were distributed. The answer is that they were concentrated.

That positioning is important because it signals both novelty and restraint. It tells the editor that the paper is not a disguised duplicate of the earlier AMTTSE studies, and it tells reviewers that the claims do not exceed the evidence. For a secondary analysis, that is exactly the right balance.

18. Data Lineage and Provenance Notes

The trustworthiness of a secondary analysis depends heavily on provenance. Readers should be able to see where the numbers came from, how they were selected, and what was excluded. In this case, the authoritative figures used in the manuscript are the cohort counts, the cumulative and December GMV totals, and the seller concentration values already cross-checked against the source materials available during the reconstruction.

That means the paper should avoid drifting into speculative reconstruction of fields that were not directly verified. If a raw seller sheet, a monthly export, or a fuller administrative table becomes available later, the manuscript can be updated with a more complete audit trail. Until then, the defensible position is to report only the values that have been verified and to label derived indicators as derived.

This kind of discipline matters because secondary studies are often criticised less for what they say than for what they silently assume. A transparent provenance note closes that gap. It gives the reader a map of the evidence hierarchy: source sheet first, verified totals second, derived ratios third, and interpretation last. That sequence helps keep the analysis honest.

The paper can also state, without drama, that the same AMTTSE base has already appeared in two prior 2026 publications. That fact is not a defect if it is disclosed and if the new question is genuinely distinct. In scholarly practice, the ethical issue is concealment, not reuse per se. The manuscript therefore treats reuse as a disclosure item and not as a problem to be hidden.

19. How to Read the Reuse Disclosure

Some editors and reviewers are cautious when they see the same programme or cohort appear in multiple papers. That caution is understandable. The concern is usually that the author is slicing one dataset into multiple pieces without adding genuine insight. The way to address that concern is not to deny reuse. It is to show the analytical boundary clearly.

In this paper, the boundary is simple. The earlier AMTTSE papers were concerned with aggregate effectiveness and capability-related explanation. This paper is concerned with distributional inequality in realised value capture. Those questions do not yield the same findings, and they do not require the same inferential logic.

If the editor asks why the paper should exist separately, the answer is that public programmes are often evaluated through multiple lenses: effectiveness, mechanism, equity, and distribution. One dataset can legitimately support more than one lens, provided each paper has a different core question and the overlap is disclosed. The present paper is meant to stand on that principle.

The disclosure also protects the final manuscript against misreading. A reviewer who sees the phrase secondary distributional analysis can immediately understand that the purpose is not to claim a new sample or a new intervention effect. The paper is a measurement and interpretation exercise. It uses existing records to make a different policy point.

20. Future Work Agenda

The present study is intentionally cautious, but it suggests several future research directions.

First, if granular seller records become available, a full distributional decomposition could be conducted. That would allow researchers to ask whether the concentration pattern is driven by a small elite throughout the programme or by a few outliers in particular months. Such a study could separate persistent advantage from temporary spikes.

Second, a comparative design could be developed across multiple programmes. AMTTSE would then be one case among several government-supported digital entrepreneurship interventions. That would allow

researchers to compare whether concentration is unusually high, typical, or context-specific.

Third, if more detailed operational data exist, future work could connect distributional outcomes to programme features such as seller support, content cadence, product type, or fulfilment speed. That would move the discussion from descriptive inequality to mechanism tracing. The present paper does not make that leap because the data available here do not support it.

Fourth, the policy field could benefit from standardised concentration reporting. If more programmes adopted the same metrics, benchmarking would become easier. Agencies could compare not only total sales but also how evenly those sales are shared. That would improve accountability and help identify which support structures are most equitable.

Fifth, the same analytical logic could be extended beyond social commerce to other public programmes where headline totals conceal uneven participant outcomes. Training grants, market access schemes, and digital adoption programmes all face the same reporting problem: aggregate success can hide internal inequality. The contribution of this paper is to make that problem visible in a concrete case.

Declarations

Data availability: [AUTHOR TO CONFIRM] access conditions with FAMA.

Conflict of interest: [AUTHOR TO CONFIRM].

Funding: [AUTHOR TO CONFIRM].

Ethics statement: secondary administrative data only; no human-subject intervention.

Related-publication disclosure: the same AMTTSE base was used in two prior 2026 publications; the present study is a distinct secondary distributional analysis.

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