

The poor fund as lender: church credit and agricultural production in the Cape Colony, 1685–1829*

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Abstract

Long before banks reached rural households, religious charities placed their assets with private borrowers. Historians can usually recover either an institution’s lending or the later economic lives of its debtors, but rarely both. This article links the credit register of a Reformed poor fund, and the account books from which it was compiled, to household tax censuses from the Cape Colony between 1685 and 1829. The median recipient stood at the 67th percentile of the settler wealth distribution. Recipients resembled one another much more closely than settler households in general. They seldom came from the poorest quartile, and their recorded wealth was already rising before they borrowed. Under the broad definition of receipt, agricultural production rises by a quarter of a standard deviation in the third and fourth years after a household first enters the register, with the increase concentrated in grain. Yet the account books show that many first entries were opening balances or debts transferred from other people. Restricting the analysis to 24 verified new advances leaves estimates in the same direction, but none can be distinguished from zero. Under either definition, the selection of borrowers cannot be separated from the credit itself. The firmer finding is institutional. A fund charged with relieving the destitute also financed the productive middle, and by the early 1720s its interest income nearly covered its annual relief bill.

Keywords: church credit; rural finance; religion and economics; Cape Colony; agricultural production

JEL codes: G21; N23; N27; Z12

*We thank Helena Liebenberg and Schalk Liebenberg (Tracing History Trust), Gustav Hendrich and Esté Kotzé for their invaluable work transcribing the original records. We are grateful to Christiaan van Bochove, Chris Colvin, Abe de Jong and Oscar Gelderblom for comments on an earlier draft. The project was supported by the Riksbankens Jubileumsfond (M20-0041). All remaining errors are our own. We used Claude Fable and Codex Sol for coding, editing and validation, and refine.ink for further validation. Cite this paper as: Fourie, Johan, and Jacques Theron. 2026. “The poor fund as lender: church credit and agricultural production in the Cape Colony, 1685–1829.” Working Paper, Department of Economics, Stellenbosch University.

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I Introduction

For a farmer in the early modern Cape, borrowing did not mean going to a bank. Credit came from neighbours, kin, merchants, notaries, orphan chambers and religious institutions. Their ledgers often survive. So do tax records, inventories and other traces of household activity. What rarely survives is a way to follow the same people through both archives. The lender's accounts sit in one repository and the household's economic life in another, and the names seldom line up without substantial reconstruction.

This article makes that connection for one institution and its debtors. Between 1685 and 1829, the poor fund of the Cape Dutch Reformed Church lent to settler households in Stellenbosch and Drakenstein. We reconstruct 797 dated entries from its credit register and read them against the deaconry's own cash books and journals. We then link the debtors we can verify to the *opgaafrolle*, the annual tax censuses. These list the composition of each settler household alongside its grain output and seed, wine, brandy, livestock, vines, vehicles and enslaved people. The two archives overlap for the same households across nearly a century and a half. The account books also reveal something the compiled register cannot: a household's first entry may be a new advance, an opening balance carried forward or a debt taken over from somebody else. We therefore use two definitions of receipt. The broad definition begins with a household's first verified link to the compiled register. The strict definition retains only the 24 onsets that a specific account-book row and an independently reviewed ledger year confirm as newly originated credit.

The first question is whom the fund chose to finance. The answer is the propertied middle of settler society. At fixed prices, the median recipient household held roughly ten times the taxable wealth of the median household that never borrowed. Yet recipients were much more alike than settler households as a whole, with a Gini coefficient of 0.62 rather than 0.78. Only one recipient in eighteen came from the poorest quartile, where the fund's own relief clients would have been found, while a third came from the richest. Their recorded wealth was also still rising when they entered the register. In household data, this is the pattern one would expect from a lender screening for repayment capacity and security.

The second question, what followed a first entry, gives a less certain answer. Under the broad definition, agricultural production rises by about a quarter of a standard deviation in the third and fourth years after entry, with grain accounting for most of the increase. Grain seed, our measure of capacity, rises after the same delay. Wine, vines, other livestock, vehicles and enslaved holdings show no common change. The estimates are smaller when averaged across all six follow-up years, and their intervals include zero. The strict definition leaves 24 verified new advances, of which 23 have adequate comparison households. Those estimates are also positive, but every interval includes zero. A sample this small could reliably detect only effects larger than about half a standard deviation. Neither definition separates the effect of credit from the selection of the households that received it.

Record linkage therefore does not solve selection. Its value is that it makes the selection visible, which is why we first establish whom the fund financed and only then ask what followed. The consistory's records name no rejected applicant, so we cannot compare borrowers with households

that applied and were refused. The censuses give the year of an entry but not its date within the year, and their calendar contains gaps that we leave as gaps. Every comparison excludes the entry year and requires the household to appear in an earlier census. Recipients are compared only with households enumerated in the same baseline and follow-up years. Within each comparison, we weight those households to match the recipient on seventeen recorded characteristics of the household and its farm. The confidence intervals allow for repeated observations of the same household.

The article joins two histories that are usually reconstructed from different records. The first is the history of credit beyond banks. Historians have used notarial deeds, probate inventories, court records and account books to recover this world (Hoffman, Postel-Vinay and Rosenthal, 2019; Ogilvie, Küpker and Maegraith, 2012; Dermineur, 2025; Lanzinger and Maegraith, 2025). Borrowing and lending were ordinary parts of rural life, and the same people often combined farming, trade and moneylending. The Cape was no exception. Probate inventories reveal an extensive private market in which settlers financed land and enslaved labour through bonds and personal obligations (Swanepoel and Fourie, 2018). Following van Bochove and Levy (2025), we treat these arrangements as non-bank credit systems in their own right, not as incomplete versions of banking. Such sources recover networks of claims and liabilities and show how deeply credit entered rural life (Ogilvie, Küpker and Maegraith, 2012; Lanzinger and Maegraith, 2025; Dermineur, 2025); probate evidence can also trace long-run changes in productive capacity (Falk, Bengtsson and Olsson, 2025). What they rarely provide is a named institutional debt followed by repeated measurements of the same borrower's output. That household time dimension is what the linked Cape records add.

The second history is that of charitable wealth. Religious and charitable institutions accumulated property and financial claims, then used the returns to meet later obligations. In the late-medieval Low Countries, asset income paid for much of their relief (van Bavel and Rijpma, 2016). Studies of charitable loan funds and Reformed deaconries show that lending was one way such institutions managed their capital (Svoboda, 2015; Zwarts, 2015). Here we can connect that financial practice to the people named as debtors and to what their households produced.

The same books also bring welfare and finance into one frame. Formal charity could command substantial resources, but its organisation, financing and reach varied sharply across place and time (van Leeuwen, 1994; Pullan, 2005; Solar, 1995; van Bavel and Rijpma, 2016). Confessional labels alone explain neither its form nor its scale (Pullan, 2005; Schaff, 2025; Kahl, 2005). Religion shaped economic institutions directly, not only through belief (Becker et al., 2025). At the Cape, one consistory allocated relief to some households and interest-bearing debt to others. By the early 1720s, interest on that debt was the largest single item in the fund's audited annual income. Reformed deaconries elsewhere in the Dutch colonial world managed accumulated resources in much the same way (Zwarts, 2015). Alms went to households judged unable to provide for themselves; credit went to middling households of demonstrated capacity. The deserving poor and the creditworthy were different people, distinguished in practice by the same body of men. This is the clearest result the linked records support. A fund charged with relieving the destitute also financed the productive middle, so welfare and rural credit became two uses of one charitable endowment (van Bavel and

Rijpma, 2016).

Section II reconstructs the fund's lending and its place in the Cape economy. Section III introduces the records and explains how they were linked, while Section IV identifies the households that received credit. Sections V to VII set out the comparison, present the estimates and mark the limits of what they can support. Section VIII concludes.

II From poor relief to financial intermediation

The Dutch East India Company (VOC) founded its Cape settlement in 1652. By the late seventeenth century, agricultural settlement had moved beyond Cape Town into Stellenbosch and Drakenstein. Farms there supplied grain, wine, livestock products and transport to Company and local markets. Wealth was distributed unevenly. Land, livestock, output and enslaved labour were concentrated among a settler elite, while many taxed households farmed on a much smaller scale (Fourie, 2013; Fourie and von Fintel, 2010).

Settlers had no bank, but they did not lack credit. Probate inventories record a substantial private market in which households borrowed from one another, chiefly through bonds used to finance land and enslaved labour. The participants came largely from the wealthier ranks of settler society (Swanepoel and Fourie, 2018). The poor fund lent within that market, and its choice of borrowers must be read against it. Our records describe the settler economy alone. They do not cover the full population of the Cape, and they reach the economic lives of Khoekhoe communities and enslaved people only where the tax censuses counted enslaved people within settler households.

Poor relief grew within an entanglement of church and Company rule. The Reformed minister drew a Company salary, civil authorities approved church office-holders, and a political commissioner normally sat with the Cape Town consistory (Gerstner, 1991, p. 178; de Boer, 2022, pp. 131, 136). This did not make every church decision a civil act, but it did place welfare resources under two authorities. In December 1687, the Council of Policy made the Cape Town elders and deacons caretakers of widows, orphans and other needy people (Böeseke, 1961, p. 183; Kotzé, 2024).

The arrangement belonged to a much wider world of welfare before national welfare states. Parishes, monasteries, confraternities, guilds, hospitals, municipal bodies and charitable foundations all supplied relief. Their systems were fragmented, but they were not necessarily small. In parts of western Europe, formal relief absorbed more than 2 per cent of output, and returns on accumulated assets paid for much of what these institutions spent (van Bavel and Rijpma, 2016). Doctrine shaped eligibility, yet Catholic and Protestant authorities alike distinguished the settled from the mobile poor, and the deserving from the undeserving (Pullan, 2005).

The Cape drew that distinction early, and government drew it as readily as the church. In March 1717, a visiting Company commissioner prompted the Council of Policy to ask whether the deaconry's monthly allowances went to people worthy of them. The Council resolved to withdraw support from anyone whose bad living or conduct made them unworthy (Western Cape Archives

and Records Service, Cape Town, Council of Policy C 41, Resolutions, 30 March 1717).¹ The church applied the same test. Cornelia Roelofse was an orphaned ward whom the deaconry boarded with foster parents. When she fell pregnant outside marriage in 1778, the church council censured her and withdrew support until she showed remorse (Groenewald, 2008, pp. 21–22). Alms relieved need, but they also disciplined conduct. The deaconry undertook to maintain people it judged unable to provide for themselves. It did not undertake to end poverty.

For a poor fund meant to endure, relief created a problem of time. Collections and bequests arrived intermittently; claims for alms continued. Administrators could hold the money as cash, buy property or public claims, or place it with private borrowers. Lending converted an accumulated stock into a stream of interest, while exposing charitable resources to default. Administrators who built church wealth through credit sought material profit alongside spiritual profit, and contemporaries in the Dutch overseas world saw no contradiction in that (Gerstner, 1991).

The combination of charity and credit was neither uniquely Cape nor uniquely Reformed. Reformed deaconries in Dutch settlements extended credit, and some Calvinist charities lent capital at interest and spent the returns on relief (Pullan, 2005, pp. 455–456; Zwarts, 2015). Charitable loan institutions elsewhere paired cheap credit with collateral and revolving capital (Svoboda, 2015). Nor was the practice confined to Christendom. Buddhist temples in early modern Japan placed accumulated funds at interest (Mitchell, 2025), while Ottoman craft guilds advanced credit from their common chests (Giakoumis, 2025). Theresian Carmelite houses in eighteenth-century Spain lent through a three-tier religious organisation that moved capital across informational and jurisdictional barriers (Milhaud, 2025). Swedish parish funds placed about 80–90 per cent of their capital in loans in 1857, and interest supplied 30–50 per cent of their annual income (Ulvång and Murhem, 2025). The Cape fund began lending much earlier. What makes it especially useful is not the practice itself, but a surviving register of debtors that can be read against their households' recorded production.

At the Cape, lending appeared almost from the beginning. A précis of the Council of Policy's journal reports that by 1674 the poor fund of the Cape Town congregation, then the colony's only one, had placed its accumulated capital in loans earning 6 per cent a year. The same entry records the administrators' concern about large sums secured only by houses. Fire might destroy a building and leave the land beneath it worth less than the debt. It also names borrowers who could pay neither their interest nor, still less, their principal (Leibbrandt, 1902, pp. 215–216). Later contracts need not all have carried the same rate or security. Even so, the basic financial problem is already visible in 1674: the administrators had to protect a charitable endowment by screening borrowers and valuing what they pledged.

An advance also required collective authority. An instruction of 1685 obliged the whole church council to approve a loan; no deacon could lend on his own (Theal, 1896, p. 46). Lending therefore sat within the governance of the consistory rather than within an individual deacon's discretion.

¹Resolutions of the Council of Policy are cited from the originals in the Western Cape Archives and Records Service, Cape Town, series C, using transcriptions made from the originals for this study. The resolutions of 1720–1723 are also published in de Wet (1968).

Neither the 1674 nor the 1685 document names a rejected applicant, gives a schedule of maturities or shows that every debtor signed the same contract.

The records also tell us what not to call the fund. An institution that demanded credible security was not giving alms to the people it relieved. Its borrowers were chosen partly for their ability to repay, even though the interest they paid would support somebody else's relief. Nor was the fund a bank. Its records show asset management, screening, secured credit and interest income. They show no deposit-taking, payments business, maturity transformation or market in transferable claims, the functions that would make the word 'bank' precise. We therefore call it a *financial intermediary*, meaning only an institution that accumulated resources from one set of flows and placed part of those resources as credit.

Over the next century, the fund accumulated substantial resources. Communion collections and legacies supplied part of its income (de Boer, 2022, pp. 128, 160); government assigned it shares of fines (Böeseken, 1962, p. 396); and from 1708 it drew guarantees and, later, fees from slave emancipations (Spoelstra, 1907, pp. 313, 607). By 1795, the last year of Company rule, the deaconry's account books record poor-fund capital above 357,000 guilders (Dutch Reformed Church Archive, Stellenbosch, GEM-K 1359, diaconate account books, 1795). Interest-bearing loans helped build that sum. Contemporaries placed the poor fund alongside the Orphan Chamber and private lenders among the colony's sources of personal credit (Groenewald, 2007).

An account laid before the Council of Policy in February 1723 reveals the scale of the fund's business in 1722. The Cape Town deaconry began the year with capital of 151,637 guilders and received 9,887 guilders during it. Interest contributed 4,411 guilders, more than alms collections, poor-boxes and legacies combined. Relief cost 4,933 guilders. The interest on the fund's loans therefore almost paid the entire relief bill. The deacons set aside expenditure on the church building as dead capital (*een dood capitaal*), leaving 110,556 guilders. Of this, 76,300 guilders, about seven guilders in every ten, stood in bonds and purchase-money mortgages (*obligatien en custingbrieven*). Cash accounted for 16,430 guilders. Overdue interest of 4,450 guilders appeared as a separate line. The remainder lay in church ornaments and loans to two country congregations. A political commissioner certified the account against the consistory's books (Council of Policy C 57, Resolutions, 9 February 1723). Most of the fund's capital was out on loan, and unpaid interest was a regular item in its accounts.

Some of the fund's borrowers were other congregations, and the resolutions record the terms. In March 1717, the Stellenbosch minister Henricus Bek petitioned the Council of Policy. His church had lain in ashes for six years. Services took place in a modest wine-press house at its owner's pleasure, while 3,500 guilders stood ready for rebuilding. The Council approved the work and directed the Cape Town deaconry to lend whatever the sum could not cover, against a proper receipt from the Stellenbosch minister and church council (Council of Policy C 41, Resolutions, 30 March 1717). The Cape Town consistory agreed the following January, but imposed a condition. Stellenbosch had to hand over as many of its own loan obligations as it could spare, keeping back 'soo veel penningen als er tot alimentatie van hare armen van noden zijn', as much money as the maintenance of its

own poor required (Council of Policy C 45, Resolutions, 25 January 1718).

Drakenstein borrowed on the same footing. In January 1719, the Cape consistory advanced its church council 2,000 guilders against obligations of equal value, formally transferred to the lender. The consistory declined to decide a request for a further unsecured sum and referred it to government. Government then authorised a loan of 4,000 guilders, still under written obligation (Council of Policy C 48, Resolutions, 24 and 31 January 1719). Further petitions in 1721 were held over (Council of Policy C 57, Resolutions, 7 October 1721). By the end of 1722, the deaconry’s account carried 4,000 guilders lent to the Stellenbosch church and 8,000 to Drakenstein. The audited account is the authority for those balances. The resolutions transcribed for this study authorise 2,000 guilders and a further 4,000 to Drakenstein, but do not record the act that raised its debt to 8,000. Even credit between congregations remained secured lending, and each poor fund retained what its own relief obligations required. Rural congregations continued to ask for money across the century (Spoelstra, 1907, pp. 276–308), while rural deaconries remained relatively poor (Biewenga, 2002, p. 203). They borrowed from Cape Town because its poor fund held the colonial church’s capital.

The institution outlived the regime that created it. VOC rule ended with the first British occupation in 1795; Batavian government followed briefly before the second British occupation in 1806. The welfare institution survived these changes, although its lending terms need not have done so. When the Cape Synod adopted its first church order in 1824, it instructed deacons to ‘continue’ caring for the congregational poor, suggesting that the local deaconry system of Company rule remained in place (Kotzé, 2024). The individual credit register continues to 1829. We can therefore follow one institutional form across the political transition: a deaconry poor fund that held assets and recorded debt. Continuity of form is not continuity of terms. We cannot assume that its legal status, interest rate, currency convention, collateral rule or approval process remained unchanged. We compare recipients only with households observed in the same census years, and estimate separately by period to test whether one political era drives the numerical pattern. Institutional continuity is a historical question here, not a warrant for identification.

III Data and record linkage

The analysis rests on three bodies of records: the compiled church credit register, the account-book transcriptions from which it was assembled, and the Stellenbosch and Drakenstein *opgaafrolle*.²

The original cash books and journals are held by the Dutch Reformed Church Archive in Stellenbosch, South Africa. Their deposited transcription covers 25 volumes, GEM-K 1341–1365, in 250 Word documents.³ Balanced debit and credit totals provide a check on the transcription. They

²The cleaned data and the full cleaning, linkage and estimation code will be made publicly available at <https://github.com/johanfourieza>.

³Dutch Reformed Church Archive, Stellenbosch, GEM-K 1341–1365; *Church Cash Books and Journals (1670–1825), Fifth and Final Progress Report*, 2022. Archive staff photographed the volumes in 2021, and a seven-person team transcribed approximately 616,000 words into designed templates. The report notes that some stuiver digits close to the binding could not be read, that repeated year labels were sometimes omitted, and that the account years 1760,

do not establish that a particular debtor row records a new loan.

The compiled register drawn from these books carries the shelfmarks GEM-K 1342–1365, one volume fewer than the transcription because GEM-K 1341 contains no individual debtor account. It gives names, years and nominal amounts, and for some entries also spouses, alternative names, probate references, notes and transfers of existing debt. We preserve the amounts exactly as compiled, but do not treat them as a continuous measure of credit. A figure may represent a residual balance, a mixture of cash and transferred debt, several advances at once or a transcription error. Guilders and stuivers were units of account in a multiple-currency system whose metallic equivalence and ‘heavy’ or ‘light’ convention could change (Wolters, 2008). Nominal amounts across the full period therefore do not measure constant purchasing power.

The register contains 800 individual entries. Of these, 797 are dated between 1685 and 1829. Two are undated, and one dated 1916 is excluded. The 797 dated entries map to 742 compiled borrower records because fifty records combine more than one register entry. A register entry is not necessarily a fresh cash advance, and a compiled borrower record is not necessarily one household. A complete re-audit of the tax-census source files produced 125,078 household-year rows. After the source-quality and linkage rules were applied, 119,934 rows remained in 35,582 conservative household segments.

Figure 1 first describes the surviving register without imposing any panel-linkage restriction. Recorded nominal amounts run from 25 to 22,400 in the register’s amount field. The first quartile is 800, the median 2,000 and the third quartile 4,800. Of the dated entries, 622 precede the occupation of 1795, sixty fall in 1795–1805 and 115 date from 1806 onwards. These are counts of surviving entries, not lending rates, because we do not know the population at risk, the rejected applications or the stock of outstanding obligations. The figure plots every dated entry on a logarithmic scale and joins decadal medians. It is not a real-value series. Currency use and money-of-account conventions changed over the period, so the apparent rise in recorded amounts cannot be interpreted as an increase in purchasing power without a separate monetary reconstruction (Wolters, 2008). The density of entries likewise reflects archival survival and transcription, not a known denominator of applications or loans.

The register’s own remarks make ‘loan’ an unsafe shorthand. In 119 dated entries, the named person ‘takes over’ somebody else’s debt; in twenty-four, the clerk later notes that the debt was transferred to another person. The register followed obligations and their holders, not simply the moments at which the fund advanced cash. Our broad treatment is therefore the household’s first verified *recorded exposure to church credit*. For eighteen of its fifty-five households, the first matched compiled entry records a debt takeover.

A single obligation shows both the promise and the danger of joining the records. On 7 August 1750, the GEM-K 1352 cash book debited 2,400 guilders to Jurgen Hendrik Engela’s account against cash. Interest for four months and 23 days brought the December balance to 2,457 guilders and 4 stuivers. The inventory of his deceased wife, Anna van Staaden, dated 19 May 1753, lists 1,180

1761, 1762 and 1765 are missing.

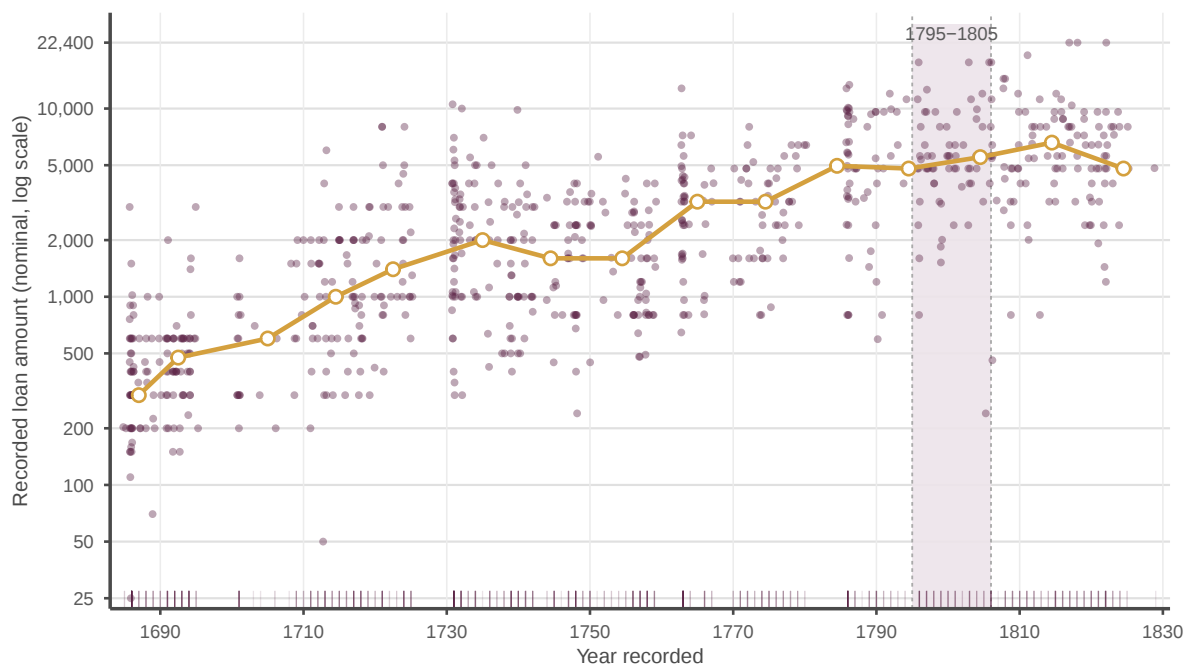


Figure 1: Timing and nominal amounts of individual credit-register entries. Each plum point is one dated entry and the gold line joins decadal medians. Amounts reproduce the register's amount field as transcribed, on a logarithmic scale, and are not adjusted for prices or changing money-of-account conventions. The shaded interval marks 1795–1805. Two entries with uncertain 1685 dates are retained; two undated entries and one 1916 entry are excluded.

rijksdaalders owed to the Cape deaconry for principal and interest under a formal *schepenkennis* obligation. Her estate contained three landed properties, 226 cattle, 1,831 sheep, 28 horses and nineteen enslaved people, yet its liabilities exceeded its assets by 414 rijksdaalders. A substantial church claim had followed an asset-rich but heavily indebted farm household into probate. The two amounts cannot be converted directly because the documents use different accounting units and refer to different dates. The case remains illustrative only. Engela’s candidate identity in the tax censuses did not meet our manual linkage standard, so the household is excluded from the panel and from every estimate.⁴

To distinguish new advances from inherited balances, we returned to the account books rather than simply discarding those eighteen rows. We extracted 44,038 table rows and 425,120 cells mechanically from the Word deposit, preserving the document, table, row and cell provenance of each. We then reconciled candidate evidence for all 233 linked event rows belonging to 213 supported debtor households. These are the same 233 register entries that Table A3 records as accepted to the safe panel. Each of the 57 event rows that determined the broad design’s 55 primary onsets received manual adjudication and an independent second review. At household level, the first events comprise 24 new-credit onsets, 17 debt takeovers or transfers, 11 opening balances, two unresolved entries and one event outside the transcribed years. These 17 account-book classifications need not equal the 18 takeover flags in the broad-design remarks. A compiled note about a takeover and an adjudication of the first account-book event are different objects.

The archive-verified treatment retains only the 24 onsets supported as a new cash advance or newly originated credit obligation, including the purchase-money *kusting* reported separately, and assigns the ledger year from the account itself. It excludes opening balances, transfers, succession, renewal and capitalised interest. The archive-verified design is defined by those decisions, not by the year or amount in the compiled spreadsheet.

The third source, the *opgaafrolle*, consists of tax enumerations rather than surveys designed for modern statistical analysis. They name the household head and, where present, a spouse, children and other household members, then list taxable agricultural quantities. The surviving forms include grain sown and harvested, wine and brandy production, cattle, horses, sheep, goats, pigs, vines, transport vehicles and enslaved people. The fields change across forms and years. The calendar also contains gaps: some censuses are incomplete or illegible, and years with inadequate preservation do not enter the linked panel. We neither interpolate these gaps nor treat the next observed record as the next calendar year.

The census files required substantial cleaning before they could be used to measure outcomes. We standardised names and locations, parsed quantities from transcribed strings, reconciled duplicate and shifted columns, and checked values against the structure of each source form. We corrected a field only when we could establish both the original entry and the direction of the transcription error. Thirty-three cells whose reading depends on context remain missing. An extreme value was

⁴DRC of South Africa Archives, GEM-K 1352, 1750, account 37; Western Cape Archives and Records Service, MOOC 8/7.30, inventory of Anna van Staaden, 19 May 1753. The register entry is `loan_raw_alpha_0188`.

not changed merely because it was large. We checked it against the household’s other observations, the structure of the source and possible column or digit errors. This keeps genuinely large farms in the panel while excluding unresolved transcription problems.

Blank cells require a separate rule. We code a blank as zero for a non-negative enumerated field that appeared on that year’s form, and only when the row has no unresolved parse, high-severity flag or conservative correction. A blank remains missing when the source did not record the field, when the row is excluded or when its meaning is unresolved. The primary sample contains the 119,934 rows that meet these rules. Section VII reports the results under alternative treatments of blanks.

Jacques Theron’s dissertation linked borrowers by hand to the longitudinal tax panel. We returned to the underlying Stellenbosch and Drakenstein census transcriptions and audited every proposed treated link against the transcribed name, spouse, year, place and neighbouring household records. Standardised name variants generated candidates, but no algorithmic score assigned treatment. The audit verified 213 register-linked debtor households with observations in the source files. Whenever a link across adjacent records remained uncertain, we divided the household into conservative segments rather than joining it.

The estimation sample is smaller because comparison requires timing as well as identity. A recipient must appear in a census before its first recorded exposure and in at least one census afterwards. Comparison households must appear in the same two years. Fifty-eight recipient households meet these timing requirements; fifty-five also have an adequate comparison group. We call a recipient, its pair of census years and its eligible comparison households a risk set. The fifty-five households contribute 216 post-entry comparisons across 138 risk sets.

The archive-verified design begins with the 24 clean onsets, all of which have timing support. Jan van Oldenburgh’s account-book year, however, is 1687 rather than the compiled 1686, and his corrected cohort fails the rebuilt overlap rule. The remaining 23 households appear in 85 post-entry comparisons across 53 risk sets, against 1,110 distinct comparison households. We also remove from the comparison group 273 safe-panel segments linked to account-book debtors without a verified clean onset. This excludes known debtors whose obligations were already outstanding or whose starting dates cannot be resolved. Table 1 records both sample constructions. Neither design covers every person in the credit register, and the archive-verified design is deliberately narrower.

Table A3 separates the linkage stage from the later timing and common-support restrictions. The 233 entries linked to the safe census panel have the same median year as all 797 dated entries and almost the same pre-1795 share. Their median nominal amount is lower, and a larger share records a debt takeover. The 58 timing-eligible and 55 primary households enter later than the 213 verified panel-linked households and are also more likely to have a takeover remark on their first entry. These comparisons show the direction in which the observed sample changes. They do not show that linkage or retention is random, and the nominal amounts are not comparable in real terms.

Table 2 compares the baseline composition and productive holdings of borrowers in the broad

Table 1: Sample construction in the broad and archive-verified designs

Stage	Unit	Number
Credit register	Individual entries	800
Dated register, 1685–1829	Register entries	797
Dated entries mapped to compilation	Compiled borrower records	742
Clean transcribed panel	Tax-unit observations	125,078
Safe cleaned panel	Tax-unit observations	119,934
Safe cleaned panel	Conservative household segments	35,582
<i>Broad compiled-register design</i>		
Verified register-to-panel links	Debtor households	213
Verified register-to-panel links	Linked safe segments	330
Pre- and post-exposure timing support	Recipient households	58
Pre- and post-exposure timing support	Treated baseline–follow-up pairs	244
Initial risk-set design	Baseline-by-follow-up risk sets	160
Initial risk-set design	Treated and control membership rows	88,024
Primary common support	Recipient households	55
Primary common support	Treated follow-up memberships	216
Primary common support	Risk sets	138
Primary common support	Distinct control households	2,181
Primary common support	Control memberships	9,369
<i>Archive-verified limited secondary design</i>		
Account-book event reconciliation	Linked event rows	233
Account-book primary adjudication	Event rows	57
Archive-verified clean onsets	Recipient households	24
Archive-verified control exclusions	Safe debtor segments	273
Archive-verified primary common support	Recipient households	23
Archive-verified primary common support	Treated follow-up memberships	85
Archive-verified contributing outcome design	Risk sets	53
Archive-verified primary common support	Distinct control households	1,110

Notes: A register entry is one individual record in the transcribed credit register; it is not necessarily a fresh advance. The 797 dated entries exclude two undated rows and one row dated 1916. They map to 742 compiled borrower records because fifty records contain multiple entries. The safe panel excludes rows and links that fail conservative source-quality rules. The first analytical block describes the broad compiled-register design. The archive-verified block retains only new-credit onsets supported by a specific account-book row and reviewed ledger year, and excludes other known archive debtors from controls. Timing support requires a baseline no more than six calendar years before the first verified register exposure and an outcome one to six years after it. Primary common support additionally requires a weighted control effective sample size of at least ten and a maximum within-risk-set control share no greater than 0.25. The same household may appear in more than one risk set; headline estimates give each recipient equal total weight.

design with those of eligible comparison households, before and after weighting. The weights bring the groups into near-perfect agreement on all seventeen pre-loan characteristics; the largest remaining difference is negligible (Table 2). That agreement is imposed by the weighting procedure, not discovered in the records. It is therefore not independent evidence that the groups are comparable, and it says nothing about unobserved influences on borrowing or production.

IV Who received church credit

Who, then, did the consistory choose to finance? We compare the 213 verified recipient households with roughly 35,000 never-recipient household segments in the safe cleaned panel. These are descriptive comparisons. They use the general panel without the timing, overlap or weighting restrictions of the outcome designs, and show who received credit rather than what credit did. Household wealth is measured by valuing taxable assets at fixed mid-eighteenth-century unit prices (Pretorius, 2025). The valuation includes grain, wine, brandy, livestock, vines, vehicles, weapons and enslaved people. It is broader than the outcome indexes in Section V, which exclude categories recorded too unevenly for comparison over time. The figures therefore reflect recorded quantities, not price changes. Blanks are coded as zero only under the quality-screened rule in Section III, while structural non-recording remains missing.

The contrast with the ordinary settler household is large. In the census years before their first recorded entry, recipient households held a median of 454 rijksdaalders in taxable assets at fixed prices, roughly ten times the median among never-recipients (Table 3). Ninety-five per cent had positive recorded wealth before entry, compared with 75 per cent of never-recipients. Half reported grain output, against one fifth. The median recipient kept ten cattle; the median never-recipient kept none. The share holding enslaved people was 60 per cent among recipients, compared with 44 per cent among never-recipients. On every recorded margin of productive standing, the fund's future debtors were already substantial households.

Yet the fund did not simply lend to the colonial elite. Wealth was much less unequally distributed among recipient households than among never-recipients, with Gini coefficients of 0.62 and 0.78 respectively. Figure A3 plots the Lorenz curves. Within the never-recipient distribution, the median recipient stood at the 67th percentile. Only 5.5 per cent came from the poorest quartile, the stratum towards which the fund's relief mandate pointed. One third came from the richest quartile, and three fifths from the middle half. Even recipient vine-growers were modest by comparison: their median vineyard held 9,000 vines, against 29,000 among vine-holding never-recipients. The clientele consisted largely of grain and stock farmers operating at a middling scale, below the great wine estates.

Their earlier histories also differed. Figure 2 follows recorded wealth around the first register entry in the general panel, without matching or weighting. It compares each recipient household with itself over time and uses never-recipients to absorb colony-wide changes from year to year. Ten years before entry, recipients' recorded wealth stood well below its level in the year immediately

before entry, relative to never-recipients. It then rose towards the entry year. A linear summary of that rise is clearly distinguishable from zero, although the nine individual pre-entry estimates, tested jointly, are too imprecise to reject a flat path. The fund’s debtors were households of established or improving standing, first recorded while they were still gaining ground. That is the clientele suggested by the concern with security in 1674 and the collective-approval rule of 1685.

The counts differ because each descriptive exercise requires different support. All 213 households are verified links between the register and the panel. Of these, 119 have usable pre-entry wealth for Table 3, Panel A, and 200 have positive pooled wealth for Panel B. Another 133 have sufficient event-window support for Figure 2, while 92 have an eligible baseline for the later attrition analysis. These are not competing totals for a single estimation sample.

The consistory’s papers preserve neither a list of applicants nor a written decision rule. The pattern is nevertheless consistent with the familiar problem of lending under incomplete information. Raising the interest rate can change both who applies and how borrowers behave, so a lender may prefer to refuse some applicants rather than raise the rate until demand equals supply (Stiglitz and Weiss, 1981). A demand for security sorts applicants in its own right, because those expecting to repay are more willing to pledge collateral (Bester, 1985). The administrators’ concern in 1674 with what borrowers pledged was therefore a screen as well as a safeguard. Rural cooperatives later addressed the same problem through local information and inexpensive sanctions (Guinnane, 2001). The consistory possessed similar advantages. It lent to households it knew through congregational life, the kind of accumulated information associated with relationship lending (Boot, 2000), while the 1685 rule of collective approval made one body the delegated monitor of the community’s charitable capital (Diamond, 1984). The fund could also assess household holdings and histories, the same dimensions that now appear in the censuses. What does not survive is the consistory’s own assessment or the applications it rejected. We observe the pattern, not the rule that produced it.

This distinction matters in two ways. First, one institution separated households deserving alms from households deserving credit within the same community. The sorting reveals the fund’s dual purpose. An endowment managed for material as well as spiritual profit placed its capital where principal looked safest, and that is where we find it: among established and improving farmers, not among the poor whom the fund relieved. Second, the same sorting determines the empirical design. A comparison with the settler population at large would confound the loan with the standing that attracted it. The preceding comparison is therefore descriptive only, and no estimate in Sections VI or VII rests on it. Each recipient is instead compared with households observed in the same census years and balanced on baseline standing, holdings and observation history.

The middling clientele was not peculiar to the Cape. In the nineteenth-century Netherlands, microfinance-type institutions often failed to reach the very poor because those households lacked the cash or collateral needed to use them (Deneweth, Gelderblom and Jonker, 2014). Dutch help banks made small loans viable through co-signers, weekly instalments and low-cost assistance from local elites (de Vicq and van Bochove, 2024). London’s Limehouse Savings Bank likewise served a broader constituency than the ‘industrious poor’ named by its promoters (Perriton and Henderson,

Table 2: Covariate balance and overlap diagnostics, broad design

Diagnostic	Before weighting	After weighting / primary sample
Number of predetermined numeric covariates	17	17
Maximum absolute standardised mean difference	0.3240	0.000026
Mean absolute standardised mean difference	0.1287	0.000009
Primary risk sets	–	138
Minimum control effective sample size within a retained risk set	–	11.33
Median control effective sample size within a retained risk set	–	44.46
Maximum control weight share within a retained risk set	–	0.2493

Notes: Balance is assessed for pooled treated memberships and eligible controls. The seventeen covariates cover baseline agricultural quantities, adult and child composition, marital structure, productive holdings, valued holdings, observation history, place and source regime. Risk sets are exact on baseline and follow-up source year. Close balance on recorded covariates does not imply balance on unobserved borrower characteristics.

Table 3: Recipient households and the general settler population

	Recipient households	Never-recipient households
<i>Panel A: standing before first recorded exposure</i>		
Fixed-price wealth, median (rijksdaalders)	454	46
Share with positive recorded wealth (per cent)	95	75
Share recording grain output (per cent)	50	21
Cattle, median head	10	0
Share holding enslaved people (per cent)	60	44
Households	119	35,231
<i>Panel B: pooled wealth distribution</i>		
Gini coefficient	0.624	0.777
Median household wealth (rijksdaalders)	784	257
Households with positive wealth	200	26,452
<i>Panel C: recipients' position in the never-recipient distribution</i>		
Median recipient percentile		67
Share below the 25th percentile (per cent)		5.5
Share between the 25th and 75th percentiles (per cent)		61.5
Share above the 75th percentile (per cent)		33.0

Notes: Descriptive comparisons in the general safe panel; no matching, weighting or timing restrictions are applied. Wealth values recorded taxable assets at fixed circa-1750 unit prices (Pretorius, 2025). Panel A compares household means over pre-exposure census years for verified recipients with household means over all observed years for never-recipient conservative segments; counts for individual rows vary with each field's recorded years. Panel B pools each household's observed years and restricts to positive recorded wealth, one value per household. Panel C locates each recipient household's pooled mean wealth within the never-recipient distribution. Recipient units are verified households; never-recipient units are conservative household segments.

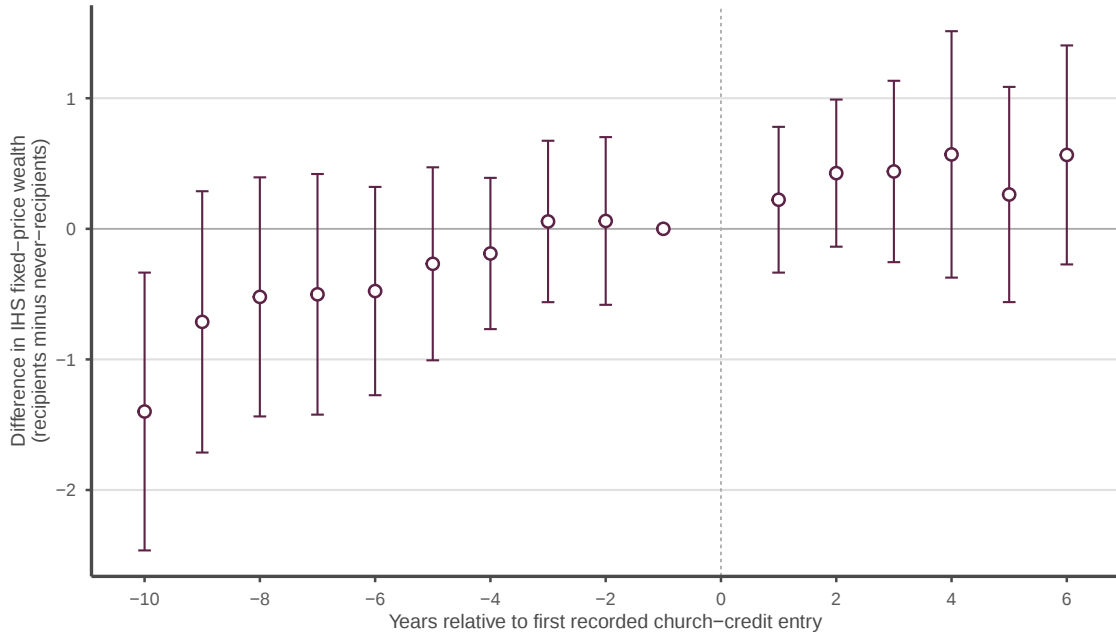


Figure 2: Recorded household wealth around the first credit-register entry in the general panel. Points are coefficients from an unweighted event study comparing 133 verified recipient households with roughly 35,000 never-recipient household segments, from a regression of inverse-hyperbolic-sine fixed-price wealth on household and census-year fixed effects. Bars are pointwise 95 per cent confidence intervals clustered by household. The year before the entry is the reference and entry-year observations are excluded. The comparison is descriptive: it applies none of the matching, weighting or timing restrictions of the outcome designs.

2022). Irish *monts-de-pi  t  * sought to lower borrowing costs and finance welfare, but depended for their viability on scale, privileged funding and cross-subsidy (McLaughlin, 2013). The Cape differed because interest-bearing debt to middling farmers and alms to destitute households were two uses of one endowment, not two services for the same clients. The division follows the logic of wealth-dependent credit: when a loan must be secured, households with assets can borrow and households without them cannot, whatever their prospects (Banerjee and Newman, 1993).

V Comparing recipients with other households

The historical question is straightforward; the comparison is not. Once a household first appeared in the credit register, how did its production and holdings change, compared with how they would have changed had it not appeared? We estimate the average of that difference among the recipients. For each group of households first recorded in a given calendar year, and for each later census year, we compare the change from a pre-entry baseline with the change among similar households that were not, or not yet, in the register.

We make the comparison under two definitions of receipt. In the broad design, the entry date is the first manually verified link to the compiled register, whether the obligation began as a cash advance, an opening balance or a transfer. In the archive-verified design, it is the independently reviewed ledger year of a newly originated cash advance or credit obligation. The second design does not repair the first. It asks the same question of a narrower and cleaner set of households, and its answer applies to that narrower group.

The headline estimates average the comparisons across calendar years one to six after the first recorded exposure. Each recipient household carries the same total weight, regardless of how many follow-up censuses contain it. Later loans are treated as part of what followed the first. Separate specifications either stop following a household at its second loan or retain only households that borrowed once. In the archive-verified sample, the only household with repeated verified advances received both in the same excluded entry year, so the two restrictions select the same households.

The register gives a calendar year but no date within it, so all observations in the entry year are excluded. For each first-exposure cohort, the baseline is the latest available census strictly before entry and no more than six calendar years earlier. Follow-up observations must fall one to six calendar years after entry. Event time is therefore elapsed calendar time, not the number of observed censuses.

A comparison household must appear in exactly the same baseline and follow-up census years as the recipient. It may never enter the register, or it may enter only after the follow-up year. A household already in the register cannot serve as a comparison. In the archive-verified design, we also remove every known account-book debtor without a verified clean onset; households with verified onsets serve only until their own entry. Comparing households within the same pair of census years absorbs colony-wide shocks and changes in the tax form that affected both groups.

Within each risk set, we retain the fifty households that most closely resemble the recipient

before entry. We then weight them until their averages match the recipient’s on seventeen pre-loan characteristics. These include the baseline outcome, adult and child composition, marital structure, productive holdings and their value, previous appearances in the records, district and tax regime. A risk set enters the primary analysis only if the weighting does not depend too heavily on a few households. The weighted comparison group must contain the equivalent information of at least ten independent households, and no one household may carry more than a quarter of the weight. A further regression adjustment accounts for any remaining pre-loan differences. Confidence intervals come from 9,999 bootstrap replications that preserve the grouping of observations within households and comparison segments.

For the archive-verified design, we also recorded how similar the candidate comparison households were before weighting. They were not similar enough. We had set a ceiling of 0.20 on the largest standardised pre-loan difference across the seventeen covariates, and no candidate pool met it. The five nearest neighbours leave a largest difference of 0.22; the figure rises to 0.24 with ten neighbours, 0.30 with twenty and 0.39 with fifty. We chose the fifty-neighbour pool because it contains the most information, accepting weaker raw balance in exchange. As prespecified, we then weighted these pools into balance. The result is a weighted comparison group equivalent to more than a thousand independent households in total. The near-perfect agreement on recorded characteristics is therefore imposed by the weighting.

One assumption makes this comparison informative, and the archive cannot prove it. Had recipients not entered the register, their outcomes must, on average, have changed like those of their weighted comparison households observed in the same census years. By matching on pre-loan standing, holdings and history, we ask less than we would by assuming that recipients moved in parallel with the settler population at large. Section IV shows that broader assumption to be false. Even so, the adjustment remains incomplete. The records contain no rejected applications, borrower intentions, land quality or full measure of household liquidity. The positive pre-loan differences reported in Section VII show that some of what distinguished recipients survives the weighting. The design also requires each recipient to have comparable households in the first place.

We specified two outcome indexes in advance. The agricultural-production index combines the available comparable measures of grain output, wine and brandy. The productive-capacity index combines grain seed, livestock, transport and vines. Enslaved holdings are reported separately and are not called capital. These quantities are highly skewed and often zero, so each is transformed by the inverse hyperbolic sine, a logarithm-like function defined at zero, and standardised against the comparison households in its risk set. The estimates are therefore expressed in standard deviations of changes in outcomes. They are not elasticities or monetary rates of return. Component estimates are also reported in their original units.

Using two indexes limits the number of headline tests. We group the dynamic estimates into years one and two, three and four, and five and six after entry. For each, we report a conventional 95 per cent confidence interval and a wider simultaneous interval that remains valid when all three periods are considered together. The component estimates receive the same kind of correction for

the number of comparisons.

We also decided in advance what different sample sizes could support. Thirty independently verified households would permit a confirmatory result; twenty would permit limited secondary evidence; anything smaller would be treated as exploratory. The 24 verified onsets cross the lower threshold, and 23 have adequate comparison households. We therefore describe the archive-verified estimates as limited secondary evidence. The middle and late follow-up periods, with 19 and 14 households respectively, are exploratory.

Five sets of checks assess the two designs. First, we ran the estimator on artificial data that preserve the actual census calendar, cohort sizes, missing observations and household groupings but contain no true effect. Before any real estimate was computed, the archive-verified version had to satisfy criteria set in advance for negligible bias and correct error rates. Second, we constructed a fixed-composition sample in which the same households support the pre-loan comparison and all three post-loan periods. Third, sensitivity intervals measure how far the parallel-trends assumption would have to fail, relative to the pre-loan difference we can observe, before an estimate would be overturned. Fourth, we estimated whether receiving credit predicts later appearance in the censuses and reweighted the sample under a stated assumption. Finally, we varied the comparison pool, overlap criteria, linkage rules, treatment of blank cells, baseline window, handling of repeat loans and set of influential households. For the archive-verified design, we also rebuilt the analysis three times: without the mixed cash-and-transfer onset, without the purchase-money bond (*kusting*), and without both.

VI What followed a first recorded entry

Table 4 places the two designs side by side. For each outcome and follow-up period, it reports the estimate in standard deviations of outcome changes, the conventional 95 per cent confidence interval, the wider simultaneous interval for reading the three follow-up periods together, a bootstrap p -value and the number of recipient households behind the estimate. Panel A gives the broad compiled-register design as originally specified. Panel B gives the archive-verified design.

Across all six follow-up years, neither estimate in Panel A can be distinguished from zero. Agricultural production is 0.116 standard deviations, with a conventional interval from -0.076 to 0.304 . Productive capacity is 0.018, with an interval from -0.153 to 0.184 . Both estimates use 55 recipient households.

The six-year average conceals a difference in timing. Production changes little in years one and two, at 0.067, and again in years five and six, at 0.069. In years three and four, it rises by 0.246 standard deviations among 43 recipient households. The conventional interval runs from 0.032 to 0.462 and the bootstrap p -value is 0.023. The simultaneous interval, which allows for the fact that we examined three follow-up periods, runs from -0.009 to 0.501. Read alone, the estimate differs from zero; read with the three periods together, it does not. Productive capacity shows the same timing more weakly: 0.165 in years three and four, with an interval from -0.013 to 0.342.

Panel B points in the same direction, with less precision. Across the six follow-up years, production is 0.191 standard deviations among 23 recipient households, with an interval from -0.108 to 0.485 . Capacity is 0.015. The production estimate for years three and four is larger than in Panel A, at 0.361, but its interval runs from -0.013 to 0.720 and it rests on 19 households. Under the thresholds set out in Section V, that estimate is exploratory. The years-five-and-six estimate uses only 14 households, too few for the adjustment regression to include all seventeen control variables. No interval in Panel B excludes zero.

Panel B is not a corrected version of Panel A. It changes the treated population, some entry years, the comparison exclusions and the weights. Its narrower message is that returning to the account books leaves the direction of the production estimates intact but removes the precision suggested by the broad design. Capacity remains close to zero in both designs, with wide intervals.

The simulations show what either design could have detected reliably. A sample the size of the broad design would find production effects of about one third of a standard deviation four times in five, and somewhat smaller capacity effects. For the archive-verified sample, under our more cautious treatment of dependence among comparison households, the corresponding figures are about one half of a standard deviation for production and one third for capacity. These calculations use the pooled index and analytic inference, so they are not exact counterparts of the binned bootstrap estimates in Table 4. The implication is nevertheless clear. Neither design has much power for effects of the size estimated here, and an estimate that becomes significant in a sample this small will tend to exceed the effect that generated it. We therefore read the delayed production estimate as evidence about direction and timing, not as a reliable measurement of size. On artificial data containing no true effect, the archive-verified estimator rejects a true null at close to the nominal five per cent rate, its intervals cover the truth at close to their stated rate, and its bias is small. These trials show that the estimator was implemented as intended. They cannot show that recipients and comparison households would have moved in parallel, and they provide no separate reassurance for the periods containing 19 and 14 households.

Figures 3 and 4 display the follow-up pattern under the two designs. Section VII then disaggregates the broad design. The archive-verified figure is included to show what remains after stricter examination of the onset evidence, not to make the dynamic result appear stronger.

Several uses of credit could produce a delayed increase in output. A household might have bought seed, hired labour, financed cultivation or released other resources for production. Neither the compiled register nor the aggregate index tells us how the money was used. We therefore turn first to the prespecified components of the broad design before interpreting the timing.

VII Components, sensitivities and interpretation

VII.1 The broad design: components and sensitivities

Table 5 separates the broad design’s estimate for years three and four into its components. Grain output rises by 0.442 standard deviations, the largest of the three production components. It is

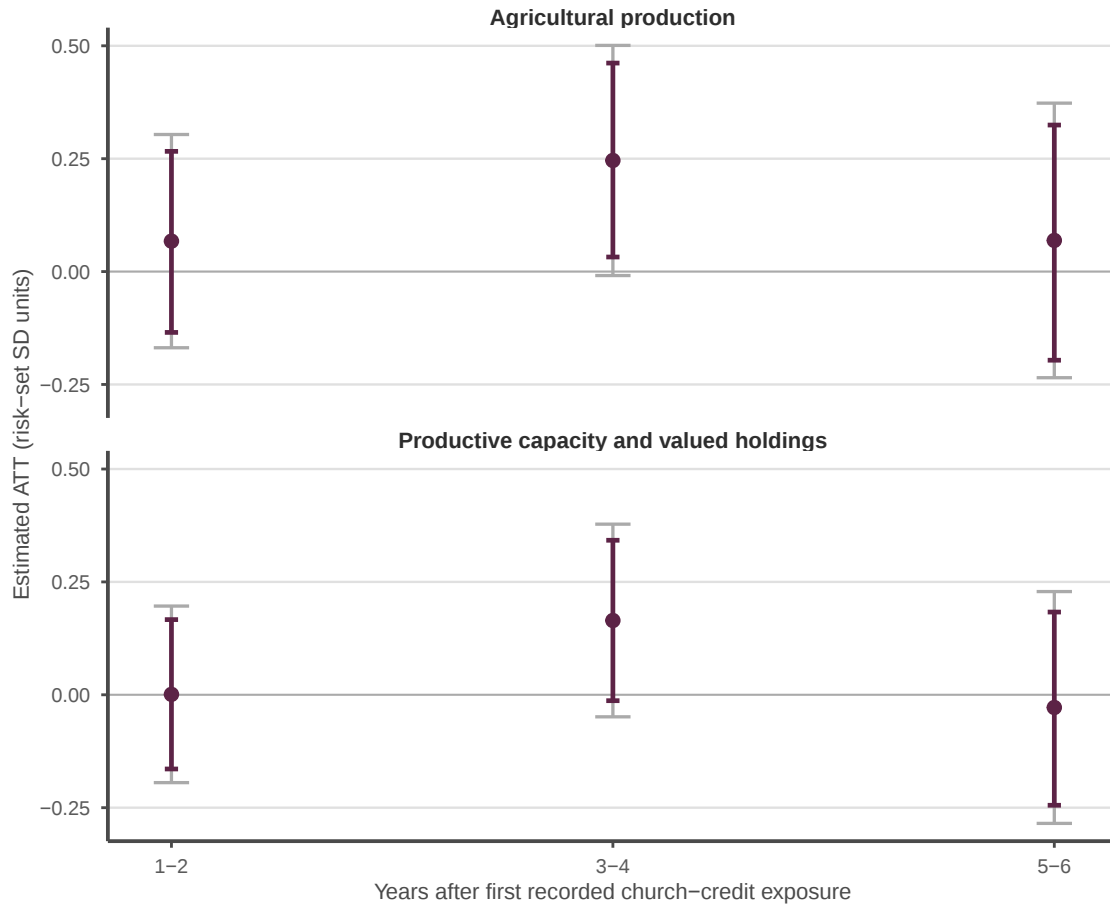


Figure 3: Broad compiled-register design. Agricultural production and productive capacity after first recorded church-credit exposure. Points show equal-recipient-weighted estimates by elapsed calendar-year group; thick bars are pointwise 95 per cent confidence intervals and thin bars are domain-wise simultaneous 95 per cent bands. Exact estimates are reported in Table 4, Panel A.

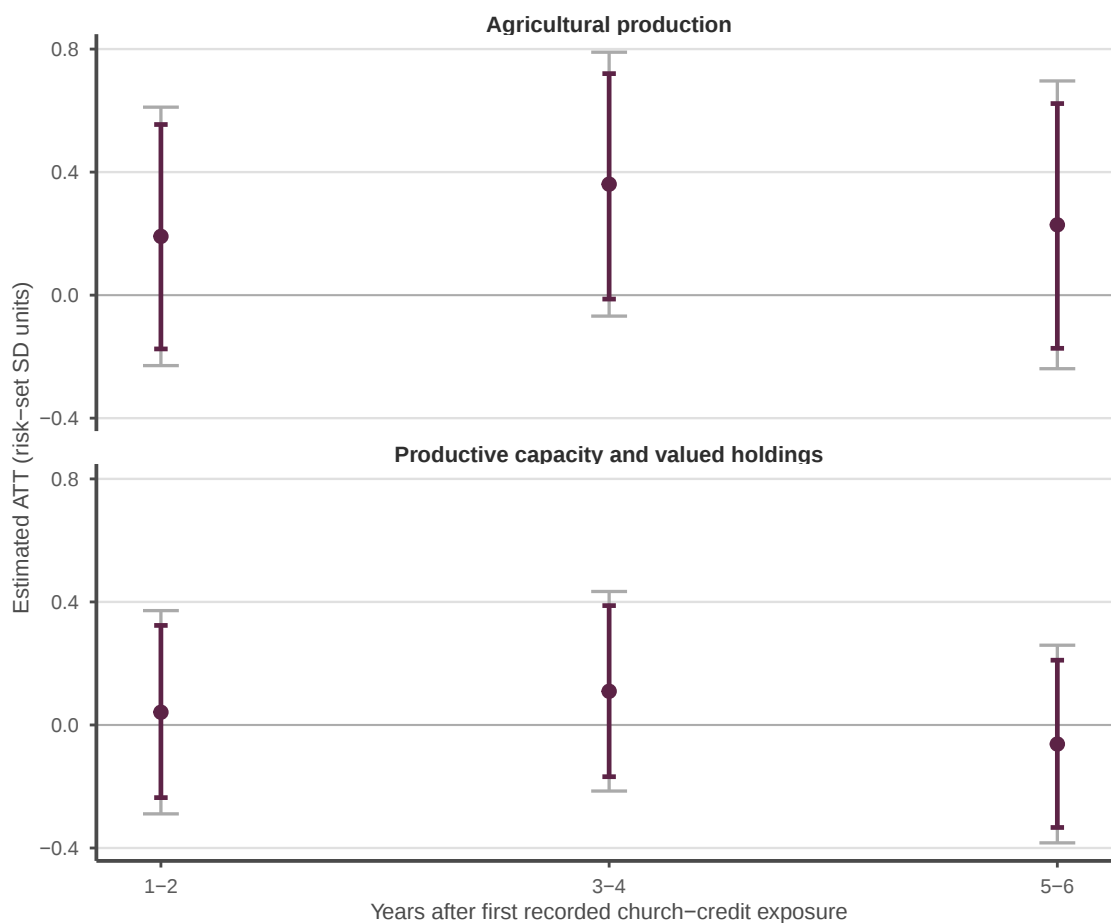


Figure 4: Archive-verified limited secondary design. Treatment is a new-credit onset supported by a specific account-book row and reviewed ledger year. Thick bars are pointwise 95 per cent confidence intervals and thin bars are domain-wise simultaneous 95 per cent bands. The middle and late bins have 19 and 14 treated households and are exploratory. Exact estimates are reported in Table 4, Panel B.

also the only component to survive correction for the three production tests considered together. Even the widest interval, which covers every production component and every follow-up period at once, remains above zero. The wine estimate is small and imprecise. Brandy is negative, but only thirteen recipient households support that estimate, so it is exploratory.

Grain seed rises over the same two years. The estimate narrowly survives correction for the eight capacity components tested at that horizon, although the widest interval across every capacity component and period includes zero. Cattle are positive and imprecise. Horses, sheep, pigs, goats, vines and vehicles show no common pattern. Enslaved holdings also rise, but zero lies well within the interval. These data do not show that church credit increased households' holdings of enslaved people.

Table 4: Broad compiled-register and archive-verified secondary estimates

Outcome domain and period	Estimate (SD)	Pointwise 95% CI	Simultaneous 95% CI	Bootstrap p	Recipients
<i>Panel A: broad compiled-register design</i>					
Agricultural production, years 1–6	0.116	[−0.076, 0.304]	–	0.249	55
Productive capacity, years 1–6	0.018	[−0.153, 0.184]	–	0.846	55
Agricultural production, years 1–2	0.067	[−0.135, 0.266]	[−0.169, 0.304]	0.519	44
Agricultural production, years 3–4	0.246	[0.032, 0.462]	[−0.009, 0.501]	0.023	43
Agricultural production, years 5–6	0.069	[−0.196, 0.325]	[−0.235, 0.373]	0.609	44
Productive capacity, years 1–2	0.001	[−0.164, 0.166]	[−0.195, 0.196]	0.991	44
Productive capacity, years 3–4	0.165	[−0.013, 0.342]	[−0.049, 0.378]	0.073	43
Productive capacity, years 5–6	−0.028	[−0.245, 0.183]	[−0.285, 0.229]	0.808	44
<i>Panel B: archive-verified limited secondary design</i>					
Agricultural production, years 1–6	0.191	[−0.108, 0.485]	–	0.210	23
Productive capacity, years 1–6	0.015	[−0.236, 0.266]	–	0.907	23
Agricultural production, years 1–2	0.191	[−0.175, 0.555]	[−0.229, 0.611]	0.312	20
Agricultural production, years 3–4	0.361	[−0.013, 0.720]	[−0.068, 0.790]	0.055	19
Agricultural production, years 5–6	0.229	[−0.173, 0.623]	[−0.239, 0.696]	0.288	14
Productive capacity, years 1–2	0.041	[−0.236, 0.324]	[−0.289, 0.372]	0.776	20
Productive capacity, years 3–4	0.110	[−0.168, 0.388]	[−0.215, 0.434]	0.455	19
Productive capacity, years 5–6	−0.062	[−0.333, 0.211]	[−0.383, 0.259]	0.671	14

Notes: Estimates are equal-recipient-weighted averages measured in within-risk-set standard deviations of outcome changes. Panel A defines treatment as the first linked compiled-register exposure. Panel B uses only newly originated cash advances or credit obligations supported by an account-book row and reviewed ledger year; it is limited secondary evidence. Production combines grain output, wine and brandy. Capacity combines grain seed, livestock, transport and vines. Treatment-year observations are excluded. Simultaneous intervals cover the three dynamic estimates within a domain. Inference uses 9,999 household-or-segment-block multiplier-bootstrap draws. Panel B’s years 3–4 and 5–6 rows are exploratory because fewer than twenty treated households remain. Panels A and B have different treated populations, onset years, control exclusions and weights and are not estimates of the same treated-population parameter.

Table 5: Broad-design component estimates in years three and four after first recorded exposure

Domain	Component	Estimate (SD)	Pointwise 95% CI	Holm-adjusted p	Family 95% CI	Recipients	Support
Production	Grain output	0.442	[0.129, 0.757]	0.014	[0.074, 0.811]	43	Primary
Production	Wine	0.054	[−0.233, 0.345]	0.720	[−0.289, 0.397]	38	Primary
Production	Brandy	−0.169	[−0.342, 0.009]	0.124	[−0.381, 0.043]	13	Exploratory
Capacity	Grain seed	0.475	[0.123, 0.832]	0.050	[0.012, 0.937]	43	Primary
Capacity	Cattle	0.268	[−0.014, 0.554]	0.461	[−0.113, 0.649]	43	Primary
Capacity	Horses	0.106	[−0.226, 0.428]	1.000	[−0.325, 0.537]	43	Primary
Capacity	Sheep	0.111	[−0.276, 0.495]	1.000	[−0.403, 0.625]	29	Limited
Capacity	Goats	0.038	[−0.293, 0.362]	1.000	[−0.405, 0.482]	16	Exploratory
Capacity	Pigs	0.049	[−0.158, 0.261]	1.000	[−0.230, 0.327]	43	Primary
Capacity	Vines	−0.054	[−0.321, 0.217]	1.000	[−0.417, 0.308]	38	Primary
Capacity	Transport vehicles	0.253	[−0.328, 0.829]	1.000	[−0.519, 1.026]	13	Exploratory
Separate outcome	Enslaved people	0.240	[−0.150, 0.635]	0.256	[−0.153, 0.632]	44	Primary

Notes: Components are standardised inverse-hyperbolic-sine changes. Holm adjustments and family simultaneous intervals are calculated within the prespecified outcome family and period. Enslaved holdings form a separate family and are not included in productive capacity. “Primary” denotes at least thirty independent recipients, “Limited” twenty to twenty-nine, and “Exploratory” fewer than twenty. The full simultaneous interval across all production components and dynamic periods remains above zero for grain output [0.033, 0.852]. The analogous full interval for grain seed includes zero [−0.024, 0.973]. Enslaved people are a separate outcome and therefore have 44 supporting recipients rather than the paired domain’s 43.

In their original recorded units, the changes in grain output and seed are positive but imprecise, and both intervals include zero. The inverse-hyperbolic-sine transformation reduces the influence of exceptionally large farms. The standardised estimates therefore tell us more about relative change among small and medium producers than about total physical volume. Parallel trends is an assumption about the scale on which outcomes are measured, so precision on the transformed scale does not carry over to physical quantities.

Grain seed and grain output rise together, as an expansion of grain cultivation would imply. Their timing is more consistent with finance for working inputs than with the accumulation of fixed holdings. But the censuses record seed as a quantity, not as a purchase paid for by the loan, and a single unobserved household decision could have moved both measures.

Who remains visible in the censuses also matters. In the broad design, recipients reappear in later censuses more often than other households. Among all 92 verified recipients with an eligible pre-loan baseline, counted without conditioning on a later observation, recipients are about ten percentage points more likely to appear in an available census during the next six years. The gap is distinguishable from zero. Credit might cause it, but so might durable household status or the mechanics of record linkage. In either case, the complete cases cannot be treated as representative of all baseline-eligible recipients.

We therefore reweighted the sample, giving more influence to the kinds of household least likely to reappear. The resulting production and capacity estimates remain close to those in Table 4 at both horizons. This correction requires a further assumption: once treatment, risk set, baseline holdings and observation history are taken into account, reappearance in a census must be unrelated to how outcomes would otherwise have changed. It also treats the fitted reappearance model as fixed and does not carry uncertainty in that model into the intervals. These estimates are sensitivity checks, not substitutes for the primary results.

The fixed-composition exercise is less reassuring. Requiring the same household to contribute to the pre-loan comparison and all three post-loan periods leaves thirty recipients. Before borrowing, these households were already growing faster in production than their comparison households. The difference is too imprecisely estimated to rule out a real divergence in underlying trends. In this sample, the production estimate for years three and four falls and its interval includes zero. Every prespecified sensitivity interval also includes zero, even when recipients' post-loan trend is allowed to diverge by only half the pre-loan difference we observe. The censuses provide one common pre-loan comparison. We cannot apply methods that require a longer sequence of pre-loan estimates without imposing a time structure that the source does not contain.

Elsewhere within the broad design, the numerical pattern is stable. Across the prespecified variations in controls, overlap, repeated loans, linkage, outliers and the baseline window, the six-year and years-three-and-four production estimates remain within a narrow band and keep the same sign. Excluding every comparison household that was a strong but manually rejected borrower candidate changes little. The same is true when Hendrik Cloete, the most prominent outlier check, is removed. No leave-one-recipient-out estimate materially shifts the result. Censoring at the second loan and

retaining only one-loan recipients also leave the pattern in place.

Alternative rules for missing values produce much the same broad estimates when they leave adequate support. Coding every historically enumerated blank as zero, the earlier panel convention, barely changes either estimate. Treating every blank as missing reduces the sample to eleven production households and five capacity households, too few for a useful test. The results therefore depend on the documented interpretation of historically enumerated blanks, but not on the small difference between the quality-screened and general zero conventions.

We also ran the preregistered trials for the broad estimator on 1,000 artificial datasets that preserve the actual calendar, cohort sizes, missing observations and household groupings but contain no true effect. Bias was negligible in both domains; the estimator rejected a true null at close to the nominal five per cent rate; and the intervals covered the truth at close to their stated rate. Section VI explains both what these trials establish and what they cannot.

VII.2 Archive-verified transaction and trend sensitivities

An earlier practice was to discard compiled entries whose remarks mentioned a takeover. The account-book design replaces that shortcut. For each alternative definition of a qualifying transaction, we rebuild the samples, comparison pools, weights, simulation checks and estimates from the beginning. Excluding Hofman’s mixed cash-and-transfer onset, De Vos’s purchase-money *kusting*, or both leaves the six-year production estimate positive, with zero inside its interval, just as in the all-clean sample (Table A2). Capacity remains close to zero. All three alternative samples pass the same pre-set simulation checks as the main archive-verified sample, and none changes the strength of the evidence the design can support.

For years three and four, production remains positive under all four definitions, from 0.274 to 0.429 standard deviations, with 18 or 19 households in each case (Table A2). Whether an interval excludes zero depends on the definition. Zero is inside the interval for the all-clean and De Vos samples, and outside it when Hofman’s onset is excluded, either alone or together with De Vos’s. We report these intervals rather than conceal them, but draw no conclusion from their differences. The samples overlap heavily, so separate intervals do not test whether the estimates differ, and the paired comparison needed for such a test is unavailable. Removing one household also re-estimates the weights for an entire cohort. That can move an unrelated risk set across the prespecified overlap threshold and alter which comparisons enter a period. Every sample remains below the prespecified threshold of twenty households for secondary evidence.

The repeated-advance restrictions fail the validation checks. Limiting the sample to households with a single verified advance leaves 23 verified households and 21 with adequate comparisons. Stopping at the second advance leaves the same numbers because the one household with two advances received both in the same excluded entry year. On artificial data with no true effect, these restricted samples produce bias beyond the tolerance set in advance, whereas the main sample remains within it. We therefore stop before calculating an effect. The failure is reported rather than hidden by relaxing the threshold or presenting an unvalidated estimate as a robustness result.

The fixed-composition exercise is thinner still in the archive-verified design. Only eleven households support both domains, all three post-entry periods and a usable earlier observation. Before borrowing, these households were already growing faster than their comparison households in both production and capacity. Both differences are estimated far too imprecisely to distinguish them from zero. They are nevertheless positive, consistent with the rising pre-entry wealth in Section IV and in the same direction as the post-entry estimates. They do not support the parallel-trends assumption. The formal sensitivity intervals include zero throughout and do not provide informative bounds. There is only one common pre-loan comparison, drawn from a census as much as six years before the selected baseline, and the reported endpoints reach the limits of the search procedure. The exercise shows how thin the support becomes; it is not a test of parallel trends.

VII.3 Interpretation

A pattern that recurs in both sign and timing does not amount to a secure causal result. In the broad design, the delayed grain estimates are not driven by one large borrower, one rule for blank cells or one definition of the comparison pool. In the archive-verified design, production remains positive across transaction definitions, but precision falls and failed simulations prevent the two intended repeated-advance analyses. The checks most directly concerned with sample composition and differing trends are either uninformative or include zero. We can therefore describe the timing and sectoral concentration of what followed a first recorded entry. We cannot establish an average causal effect of church credit. The books preserve no rejected applications, and neither design provides the sequence of common pre-loan comparisons required for a stronger test of parallel trends.

The selection evidence and the outcome estimates form one historical picture. A lender screening for repayment capacity will choose households whose production might have grown without the loan. Rising output among borrowers is therefore what successful screening can produce whether or not the credit itself added anything. Our results fit that interpretation. Recipients' recorded wealth rose before entry, and grain output rose in the third and fourth years afterwards, on a scale that the design cannot separate from selection. The credit may have financed the expansion, or it may merely have accompanied it. In either case, the linked records reveal a consistory able to identify, within one rural community, households about to expand. That is evidence about the quality of the screen, and it does not depend on how the causal question would ultimately be resolved.

VIII Conclusion

The poor fund of the Cape Dutch Reformed Church used one endowment for two purposes. It relieved households judged unable to support themselves, and it lent at interest to the productive middle of settler society. No surviving list names the recipients of alms, so the two clienteles cannot be matched household by household. The debtors were nevertheless plainly not the poor. The same consistory decided who belonged in each group.

The scale of the second activity is the greater surprise. In 1722, the Cape Town deaconry received

4,411 guilders in interest and spent 4,933 guilders on the poor. Interest on its loans therefore came close to paying for the year’s relief. About seven guilders in every ten of its investable capital stood in bonds and purchase-money mortgages. Lending at that scale was not an incidental part of charity. Nor were the borrowers poor: the median recipient household held roughly ten times the taxable assets of the median household that never borrowed, and only one recipient in eighteen came from the poorest quartile of the settler population.

This changes how a church poor fund should be read. It belongs to the history of premodern welfare, but equally to the history of rural credit before banks. One stock of capital served both purposes; the accounts record the tension between preserving that capital and spending its returns. Historians have long known that rural households borrowed widely outside banks. The Cape records add the named debtors of one institution and repeated observations of what their households produced. They allow the welfare question and the credit question to be asked of the same set of books.

What the credit did to those households remains uncertain. In the broad design, production rises by about a quarter of a standard deviation in the third and fourth years after the first recorded entry, with the increase concentrated in grain. The estimate differs from zero when read alone, but not when the three follow-up periods are considered together. Only 24 of the broad design’s 55 households have an account-book row confirming newly originated credit. The 23 with adequate comparisons produce positive estimates, but every interval contains zero. A compiled first entry may be an opening balance or a transferred obligation, and even a verified advance does not say what the money bought. The consistory left no list of rejected applicants. Comparison households resemble recipients only after weighting, and the weights cannot remove everything that distinguished households already growing faster before they borrowed. These limits apply to the production estimates. They do not weaken the independent evidence about whom the fund financed.

A register of rejected applications would identify the households that sought credit but did not receive it, the comparison this design lacks most. A direct reconstruction of approvals, repayment schedules, collateral and stated purpose would allow the uses of a loan to be tested rather than inferred. Both may yet be recoverable: the synod’s welfare structures continued to develop for two centuries after this register ends (Kotzé, 2024). For now, the Cape case shows both the reach and the limit of joining a lender’s books to repeated household measurements. The records identify who was chosen and where those people stood. They do not measure what the credit did for them.

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Appendix: supplementary descriptive and robustness material

This appendix gives the distribution of register entries across political periods, compares the broad and archive-verified treatments, reports the disaggregated production and capacity estimates discussed in Section VII, and plots the Lorenz curves behind the inequality comparison in Section IV. It adds no identifying assumptions. Table A3 separately records the disposition of event links and the successive household stages in the broad design.

Table A1. Credit-register entries and nominal amounts by political period

Period	First year	Last year	Entries	25th percentile	Median	75th percentile
VOC rule	1685	1794	622	600	1,500	3,000
Political transition	1796	1805	60	4,000	4,800	8,000
British rule	1806	1829	115	4,000	5,600	8,000

Notes: Amounts reproduce the register’s nominal amount field as transcribed and are not adjusted for prices or changes in money of account. The first and last year columns give the earliest and latest entry observed within each period, not the period’s bounds: the transition period runs from 1795 to 1805 and its earliest surviving entry falls in 1796. Period counts describe surviving dated entries, not application rates, outstanding credit or real lending volumes. Two uncertain 1685 dates are retained.

Table A2. Archive verification and transaction-subtype comparisons

Sample	Outcome	Period	Estimate (SD)	Pointwise 95% CI	Recipients
<i>Panel A: broad versus archive-verified treatment</i>					
Broad compiled register	Agricultural production	Years 1–6	0.116	[−0.076, 0.304]	55
Broad compiled register	Agricultural production	Years 3–4	0.246	[0.032, 0.462]	43
Broad compiled register	Productive capacity	Years 1–6	0.018	[−0.153, 0.184]	55
Broad compiled register	Productive capacity	Years 3–4	0.165	[−0.013, 0.342]	43
Archive-verified all clean	Agricultural production	Years 1–6	0.191	[−0.108, 0.485]	23
Archive-verified all clean	Agricultural production	Years 3–4	0.361	[−0.013, 0.720]	19
Archive-verified all clean	Productive capacity	Years 1–6	0.015	[−0.236, 0.266]	23
Archive-verified all clean	Productive capacity	Years 3–4	0.110	[−0.168, 0.388]	19
<i>Panel B: archive-verified transaction subtypes, years 1–6</i>					
Exclude Hofman mixed onset	Agricultural production	Years 1–6	0.254	[−0.046, 0.549]	22
Exclude Hofman mixed onset	Productive capacity	Years 1–6	0.062	[−0.183, 0.307]	22
Exclude De Vos <i>kusting</i>	Agricultural production	Years 1–6	0.112	[−0.145, 0.370]	22
Exclude De Vos <i>kusting</i>	Productive capacity	Years 1–6	−0.023	[−0.273, 0.231]	22
Exclude both	Agricultural production	Years 1–6	0.175	[−0.088, 0.443]	21
Exclude both	Productive capacity	Years 1–6	0.025	[−0.227, 0.272]	21
<i>Panel C: exploratory archive-verified production estimates, years 3–4</i>					
All clean onsets	Agricultural production	Years 3–4	0.361	[−0.013, 0.720]	19
Exclude Hofman mixed onset	Agricultural production	Years 3–4	0.429	[0.086, 0.768]	19
Exclude De Vos <i>kusting</i>	Agricultural production	Years 3–4	0.274	[−0.051, 0.589]	18
Exclude both	Agricultural production	Years 3–4	0.359	[0.067, 0.658]	18

Notes: The broad compiled-register and archive-verified designs have different treated populations, onset years, control exclusions and weights. Their estimates are not two versions of one quantity. We rebuild every Panel B cohort from the raw design inputs, and each meets the simulation criteria we fixed in advance. The two prespecified repeated-advance cohorts exceed our null-bias tolerance, so we report no effect estimates for them. Archive-verified results are limited secondary evidence, and rows with nineteen recipients or fewer are exploratory. In Panel C the simultaneous intervals for excluding Hofman’s mixed onset [0.029, 0.829] and for excluding both [0.020, 0.697] also lie above zero. Both cohorts fall below the twenty-household threshold for secondary evidence. Panel C rows share most of their households, and each rebuild re-estimates the cohort’s weights, so the differences between them are not tests of the transaction definitions.

Table A3. Linkage disposition and broad-design sample stages

Sample	Entries/households	First-year Q1	Median year	First-year Q3	Pre-1795 (%)	Median amount	Takeover (%)
<i>Panel A: dated register entries</i>							
All dated register entries	797	–	1748	–	78.0	2,000	14.9
Accepted to safe panel	233	–	1748	–	77.7	1,440	19.3
Verified, no uploaded source observation	73	–	1731	–	98.6	1,000	1.4
Not linked or unresolved	491	–	1758	–	75.2	2,500	14.9
<i>Panel B: verified households at successive broad-design stages</i>							
All verified panel-linked households	213	1711	1748	1791	77.5	1,500	20.2
Timing-eligible households	58	1695.75	1763	1807.75	63.8	1,833	31.0
Primary weighted-design households	55	1701	1770	1810	61.8	1,920	32.7

Notes: Panel A classifies all 797 dated register entries using the frozen event-linkage disposition. ‘Verified, no uploaded source observation’ identifies a borrower adjudicated in the register but absent from the uploaded tax-census source. Panel B begins with the 213 verified debtor households observed in the safe panel, then applies the timing and common-support restrictions of the broad design. Household characteristics refer to the first mapped register entry. Amounts reproduce the nominal register field and are not comparable in real terms across the full period. A takeover is identified from a non-empty register remarks field. The table is descriptive and makes no assumption that linkage is random.

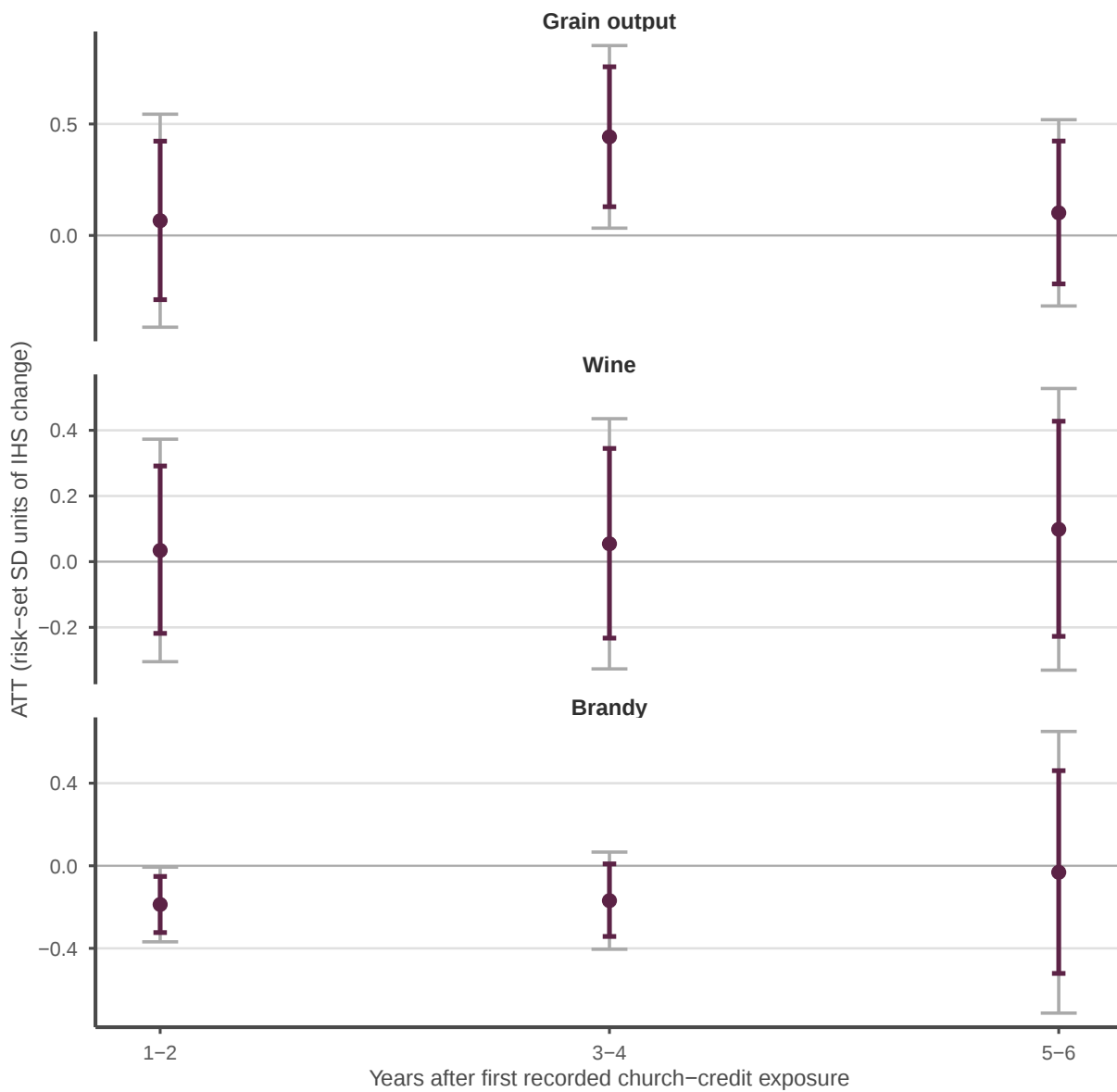


Figure A1. Broad-design dynamic production-component estimates. Thick intervals are pointwise 95 per cent confidence intervals; thin intervals are simultaneous 95 per cent bands across all production components and dynamic periods.

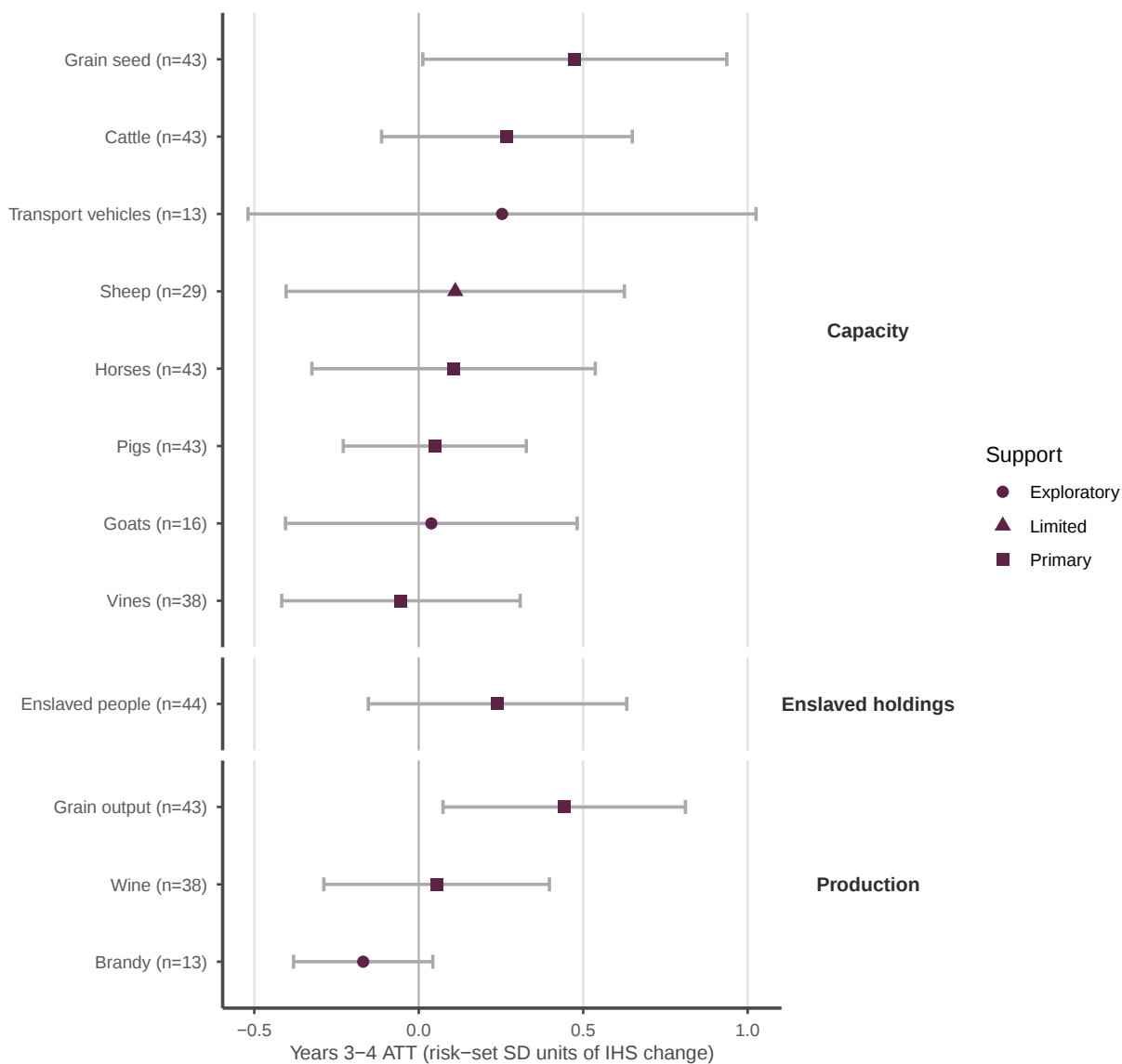


Figure A2. Broad-design component estimates in years three and four. Intervals control family-wise error across components within each prespecified domain. Shapes distinguish primary, limited and exploratory support.

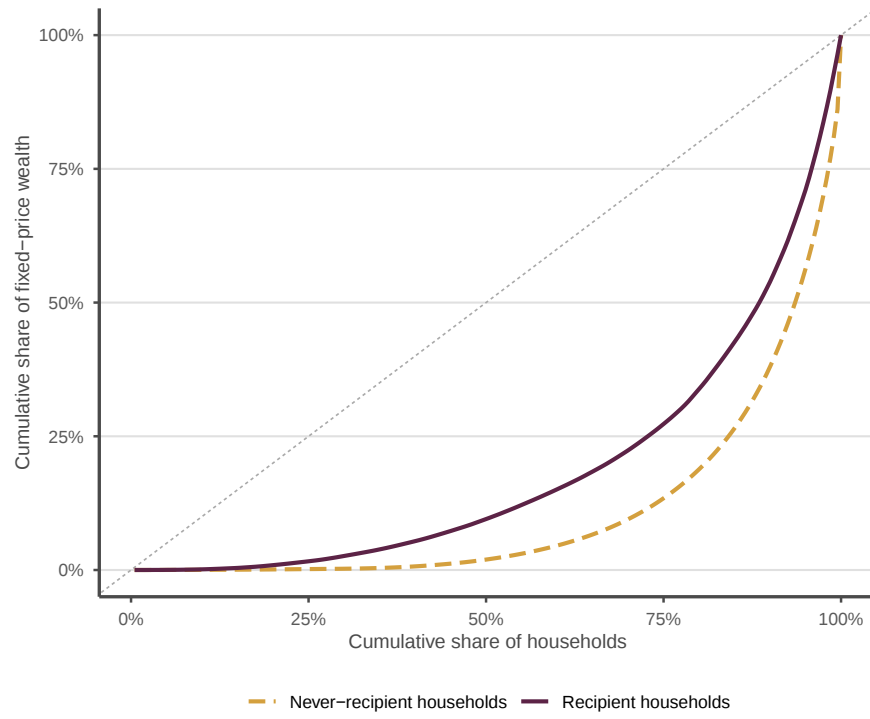


Figure A3. Lorenz curves of pooled fixed-price household wealth for verified recipient and never-recipient households. The recipient distribution is markedly more compressed: a Gini coefficient of 0.624 among 200 recipient households against 0.777 among 26,452 never-recipient segments. Sample and construction follow Table 3, Panel B.