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Tuition Subsidies and Tertiary Education Participation: Evidence from a System with Deferred Costs

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Evidence from a System with Deferred Costs

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Abstract

This paper examines the impact of tuition subsidies on tertiary education outcomes in a setting where financial barriers are already substantially reduced. We study the introduction of New Zealand's Fees-Free policy, which eliminated first-year tuition fees, in a system where tuition is financed through widely accessible student loans that require no upfront payment and are interest-free for borrowers who remain in the country, and where means-tested allowances provide non-repayable support toward living costs for some students. Using administrative data and a cohort-based empirical strategy, we estimate effects on participation, retention, and completion. We find little evidence that the policy increased participation or improved progression outcomes, and socioeconomic gaps in participation do not narrow. These findings are consistent with a setting in which tuition costs are not the primary constraint on tertiary enrolment. Instead, opportunity costs and prior academic preparation appear more important in shaping participation decisions. Our results identify a boundary condition for the effectiveness of tuition subsidies: in systems where upfront costs are deferred and borrowing costs are low, tuition subsidies primarily reduce future student debt rather than altering constraints at the point of enrolment, and may therefore have limited effects on access and may not improve, and may even worsen, equity, with implications for the cost-effectiveness of universal tuition subsidies in such settings.

Keywords: tertiary education, tuition subsidies, free tuition, difference-in-differences, New Zealand, Fees-Free, educational inequality, administrative data

JEL codes: I22, I23, I28, H52

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1 Introduction

Policies that reduce or eliminate tuition fees are widely used to increase participation in tertiary education, particularly among under-represented groups. The underlying assumption is straightforward: lowering the private cost of education should increase enrolment. However, the empirical evidence is mixed, and estimated effects vary substantially across institutional settings and policy designs (Dynarski, 2003; Deming and Dynarski, 2010; Gurantz, 2019; Gándara and Li, 2020). Understanding when and why tuition subsidies are effective therefore remains an open question. This distinction also has implications for the cost-effectiveness of such policies, as reducing costs that are not binding at the point of decision may have limited behavioural impact despite substantial fiscal cost.

This paper evaluates the impact of New Zealand's Fees-Free policy, introduced in 2018, which eliminated first-year tuition fees for eligible students. The policy appears to substantially reduce the price of education, but its effect on the financial constraints faced by students is less clear. Prior to Fees-Free, tuition was already financed through widely accessible government-backed student loans that do not require upfront payment and are interest-free for borrowers who remain in the country. These loans can be used to cover the full cost of tuition as well as some study-related and living expenses. In addition, means-tested student allowances provide non-repayable support for living costs. Government funding also accounts for the majority of tertiary education costs, with students contributing less than one-fifth on average (New Zealand Productivity Commission, 2017). As a result, tuition costs in New Zealand are largely deferred, and policies that reduce fees primarily lower future repayment obligations rather than altering the financial cost structure faced at the point of enrolment.

These institutional features imply that Fees-Free altered the structure of costs in a more limited way than its headline design suggests. In particular, the policy largely converted a deferred cost into no cost, rather than relaxing an upfront financial constraint for most students. This distinction is important because theoretical and empirical work suggests that liquidity constraints, rather than the present value of costs, are often the primary barrier to participation (Carneiro and Heckman, 2002; Dynarski, 2003). In a system where upfront costs are already minimal and borrowing costs are low, further reductions in tuition fees are therefore expected to have limited effects on enrolment decisions, although some marginal individuals may respond to reductions in future repayment obligations. Participation may instead be shaped by opportunity costs—particularly foregone earnings—as well as differences in academic preparation and other non-financial barriers (Sakellaris and Spilimbergo, 2000; Bettinger et al., 2012). In this context, even a policy with a large reduction in fees may have only modest behavioural effects if it does not alter the constraints faced at the point of enrolment.

The New Zealand case therefore provides a useful test of whether reducing tuition fees affects behaviour when financial barriers are already attenuated. Using linked administrative data from Stats NZ's Integrated Data Infrastructure (IDI), this paper implements a cohort-based empirical design comparing school-leaver cohorts before and after the introduction of Fees-Free. The analysis examines participation, composition, retention, and completion, and explores heterogeneity by socioeconomic background.

A key empirical challenge is that the policy was introduced during a period of declining tertiary participation that predates the reform. Failing to account for this trend risks attributing pre-existing changes in enrolment behaviour to the policy itself. To address this, the analysis explicitly models cohort trends and implements placebo tests to assess the plausibility of the identifying assumptions.

The results show that once pre-existing trends are accounted for, the effect of Fees-Free on participation is small and only marginally statistically significant. There is no consistent evidence of aggregate effects on composition, retention, or completion. Moreover, the policy does not appear to reduce socioeconomic gaps in participation, and in some specifications these gaps widen slightly. These findings are consistent with the view that, in institutional contexts where upfront costs are already low and borrowing is subsidised, further reductions in tuition fees have limited effects on access. They also suggest that such policies may primarily operate as transfers to students who would have enrolled in the absence of the subsidy, rather than as mechanisms for expanding participation.

This paper contributes to the literature on higher education finance by highlighting the distinction between upfront costs and deferred repayment obligations as a key determinant of policy effectiveness. While existing evidence shows that reducing tuition can increase participation in settings where financial constraints are binding, we provide quasi-experimental evidence from a context in which such constraints are substantially mitigated. In New Zealand, tuition can be financed without upfront payment through widely accessible, interest-free loans, and some living costs are supported through means-tested allowances. In this setting, tuition subsidies primarily reduce future student debt rather than relaxing constraints at the point of enrolment, and have limited effects on participation, retention, and completion. Moreover, because tertiary participation is concentrated among relatively advantaged groups, universal tuition subsidies direct a substantial share of public funding toward students who would have enrolled in the absence of the policy. This implies that the cost per additional enrolment may be high relative to alternative policy instruments that more directly target binding constraints. While this paper focuses on New Zealand, the mechanism highlighted is relevant to other settings with similar student finance systems, including the United Kingdom, Australia, and parts of Canada, where tuition costs are largely deferred and borrowing constraints are limited.

2 Literature and Conceptual Framework

This section situates the paper within the existing literature and introduces a simple conceptual framework to guide the empirical analysis. We first review evidence on the effects of tuition subsidies and the mechanisms through which they operate. We then present a framework that distinguishes between upfront costs and deferred repayment obligations, which is central to the interpretation of the policy evaluated in this paper.

2.1 Literature

A large empirical literature examines the effects of tuition subsidies and financial aid on tertiary education outcomes. A central finding is that reducing the private cost of education can increase participation, particularly among disadvantaged students (Dynarski, 2003; Deming and Dynarski, 2010; Fack and Grenet, 2015). However, the magnitude of these effects varies substantially across institutional settings, and in some cases tuition reductions have limited or no impact on enrolment (Declercq and Verboven, 2015; Ponce and Loayza, 2012; Gurantz, 2019). While some policies generate sizeable enrolment gains, particularly in settings where financial constraints are strong, estimated impacts are often small in systems with extensive financial support. Understanding the sources of this heterogeneity is an important focus of the literature.

One explanation emphasises the role of credit constraints. In standard human capital models, individuals may fail to invest in education even when returns are high if they are unable to finance upfront costs (Carneiro and Heckman, 2002). Consistent with this, policies that relax liquidity constraints—such as grants or simplified access to financial aid—have been shown to increase participation (Dynarski, 2003; Bettinger et al., 2012). In these settings, tuition subsidies are effective because they reduce costs that must be paid at the point of enrolment. This is consistent with evidence that the marginal effect of tuition subsidies is smaller in systems where students already have access to loan financing (Hemelt and Marcotte, 2011).

A second explanation focuses on the broader structure of student finance systems. When tuition can be financed through loans, particularly where borrowing is widely accessible and repayment conditions are favourable, the importance of upfront costs is reduced. In such settings, tuition is effectively a deferred cost, and the enrolment decision may depend less on the level of tuition fees and more on expected returns and other factors. Empirical evidence from low-tuition or heavily subsidised systems suggests that participation gaps are often driven by differences in prior achievement and preferences rather than price sensitivity (Declercq and Verboven, 2015; Ponce and

Loayza, 2012).

A related strand of the literature highlights programme choice and substitution effects. Tuition subsidies may alter not only whether students enrol, but also where they enrol, with some evidence that subsidies shift students across institutions or programme types rather than increasing overall participation (Cohodes and Goodman, 2014). In such cases, aggregate enrolment effects may be modest even when individual responses are substantial.

Another explanation emphasises the role of opportunity costs. When foregone earnings are large relative to tuition costs, reductions in tuition may have limited effects on enrolment decisions. Cross-country evidence supports this distinction. Using data from OECD and non-OECD countries, Sakellaris and Spilimbergo (2000) show that tertiary enrolment is countercyclical in OECD economies – consistent with opportunity costs being the dominant margin – while enrolment is procyclical in non-OECD settings, where credit constraints are more likely to bind. This suggests that in settings where credit markets function reasonably well and tuition is a smaller share of total costs, policies that reduce tuition may not materially alter the trade-offs faced by prospective students.

More recent work also highlights the importance of behavioural and informational frictions. Complexity in financial aid systems, lack of information, and the salience of costs can all influence enrolment decisions (Bettinger et al., 2012). These findings suggest that the perceived cost of education may differ from its financial cost, and that policy effectiveness depends not only on the level of subsidies but also on how costs are presented and understood.

Existing New Zealand research provides complementary evidence on participation patterns and financial constraints. Studies using linked administrative data show that gaps in tertiary participation are largely explained by differences in prior achievement and socioeconomic background rather than tuition costs (Meehan et al., 2017; Earle, 2018). Evaluations of related policies suggest that financial support can matter at specific margins, such as loan access for continuing students (Chu and Cuffe, 2020). However, there is limited causal evidence on the effects of universal tuition subsidies. This paper contributes by providing a quasi-experimental evaluation of the Fees-Free policy using population-level administrative data.

This paper contributes to this literature by emphasising the distinction between upfront costs and deferred repayment obligations. While this distinction is implicit in much of the existing work, it has received less explicit attention in empirical evaluations of tuition subsidies. In particular, we highlight that the effectiveness of tuition subsidies depends on whether they reduce costs at the point of enrolment or primarily affect future repayment obligations. Using evidence from New Zealand, a setting in which tuition can be financed without upfront payment and borrowing costs are low, we provide a useful empirical test of this mechanism. Our findings suggest that when financial constraints at the point of enrolment are weak, tuition subsidies have limited effects on participation and are largely inframarginal.

The following subsection formalises this distinction and clarifies the conditions under which tuition subsidies are expected to affect enrolment decisions.

2.2 Conceptual framework

This framework builds on standard models of human capital investment with credit constraints, but emphasises the distinction between upfront costs and deferred repayment obligations, which is central to modern student loan systems.

We consider a simple model of the decision to enrol in tertiary education. An individual chooses whether to enrol ($E = 1$) or not ($E = 0$) by comparing the expected lifetime utility of each option. Let utility from enrolment be:

$$U^E = R - L - C^{\text{upfront}} - \delta C^{\text{future}} \tag{1}$$

and normalise the utility from non-enrolment to zero. Here, R denotes the expected return to education, L represents the opportunity cost of enrolment (e.g. foregone earnings), C^{upfront} captures costs that must be paid at the point of enrolment, and C^{future} represents costs that are repaid in the future through student loans. The parameter $\delta \in (0, 1]$ captures the effective weight placed on future repayment obligations. This reflects both standard time-preference discounting and features of the student loan system, such as interest rates, repayment conditions, and the perceived burden of future debt. That is, δC^{future} represents the present value or perceived cost of future repayments.

Enrolment is desirable if:

$$U^E \geq 0 \tag{2}$$

In addition to this utility condition, individuals must satisfy a financing constraint:

$$C^{\text{upfront}} \leq A + B \tag{3}$$

where A denotes available resources and B represents borrowing capacity.

This framework highlights a distinction between the level of costs and their timing. When tuition must be paid upfront and borrowing is limited, C^{upfront} is large and may both reduce utility and bind the financing constraint. In this case, tuition subsidies reduce upfront costs and can have substantial effects on participation.

In contrast, when tuition can be financed through student loans that do not require upfront payment, C^{upfront} is zero or close to zero for most individuals. Tuition costs are instead shifted into C^{future}, which is repaid over time. In such settings, the financing constraint is unlikely to bind, and the enrolment decision is governed primarily by the utility condition. Because future repayments are discounted and may be less salient, reductions in C^{future} are expected to have smaller effects on behaviour.

A tuition subsidy reduces total tuition costs, but when tuition is already fully financed through loans, the subsidy primarily reduces C^{future} rather than C^{upfront}. As a result, the policy does not materially relax the financing constraint at the point of enrolment. Unless individuals are highly sensitive to future debt, the effect on participation is therefore expected to be limited.

This framework implies the following.

Implication 1 (Desirability of enrolment). Individuals will choose to enrol in tertiary education when the expected utility from enrolment is positive, i.e. $R - L - C^{\text{upfront}} - \delta C^{\text{future}} \geq 0$

Implication 2 (Role of financing constraints). Even when enrolment is desirable, individuals will not enrol if they cannot meet upfront costs. In particular, if $C^{\text{upfront}} > A + B$, the financing constraint binds and enrolment is not feasible.

Implication 3 (Effects of tuition subsidies under deferred-cost systems). When tuition can be financed without upfront payment and borrowing is widely accessible, $C^{\text{upfront}} \approx 0$ and the financing constraint is unlikely to bind. In this case, tuition subsidies primarily reduce future repayment obligations (C^{future}) rather than relaxing constraints at the point of enrolment. As a result, effects on participation are expected to be limited, unless individuals place substantial weight on future repayment obligations.

Implication 4 (Inframarginal incidence). When participation in tertiary education is concentrated among relatively advantaged individuals, universal tuition subsidies are largely inframarginal and primarily benefit individuals who would have enrolled in the absence of the policy.

These implications guide the empirical analysis that follows, particularly in assessing whether the Fees-Free policy operates through changes in upfront constraints or through reductions in future repayment obligations.

Extension to Additional Outcomes The framework above focuses on the participation decision as the primary margin through which tuition subsidies operate. The empirical analysis also considers programme choice, retention, and completion as secondary outcomes. These can be interpreted as downstream consequences of the initial enrolment decision.

In particular, if tuition subsidies are largely inframarginal and do not substantially alter participation, then effects on composition, retention, and completion are also expected to be limited. However, these outcomes may respond differently if the policy affects marginal students with different characteristics or alters the allocation of students across programmes. The empirical analysis therefore examines these outcomes to provide a more comprehensive assessment of policy effects.

3 Institutional Background and Policy Context

This section provides the institutional context needed to interpret the empirical analysis. It describes the structure of New Zealand's tertiary education system, the student finance arrangements that preceded the Fees-Free policy, the design and implementation of the policy, and the broader trends in tertiary participation. Throughout, the discussion links institutional features to the conceptual framework in Section 2, particularly the distinction between upfront costs, deferred repayment obligations, and opportunity costs.

3.1 The New Zealand Tertiary Education System

New Zealand's tertiary education sector comprises four main types of providers: universities, institutes of technology and polytechnics (now consolidated under Te Pūkenga), wānanga (Māori tertiary institutions), and private training establishments (PTEs). There are eight universities, which offer qualifications primarily at the bachelor's level and above (Levels 7–10 on the New Zealand Qualifications and Credentials Framework, NZQCF; New Zealand Qualifications Authority, 2024). Te Pūkenga institutions and PTEs deliver a broader range of qualifications, from certificates (Levels 1–4) through diplomas (Levels 5–6) to degrees. Wānanga provide programmes grounded in *mātauranga Māori* (Māori knowledge) across a range of levels.

Qualifications are classified on a ten-level framework (NZQCF, formerly NZQF). Certificates at Levels 1–3 are typically short-duration, foundation-level qualifications. Levels 4–6 include trade and vocational certificates, national certificates, and diplomas. Bachelor's degrees are at Level 7, with postgraduate qualifications at Levels 8–10. The distinction between sub-degree (Levels 1–6) and degree-level (Level 7 and above) programmes is important for this paper's analysis of programme composition. Because the Fees-Free policy covered NZQF Level 3 and above, and Levels 1–2 are equivalent to senior secondary qualifications, our empirical analysis restricts the composition variable to enrolments at Levels 3–7 (see Section 4.3). These institutional features are important for interpreting programme-level responses, but they do not directly determine the financial constraints associated with enrolment, which are shaped primarily by the student finance system described below.

Tertiary participation rates in New Zealand peaked in the mid-2000s and have since declined steadily, particularly in sub-degree programmes (Ministry of Education, 2024b). Ministry of Education data show that the age-standardised participation rate fell from approximately 13.2 per cent of the 15–64 population in 2005 to 10.4 per cent by 2024, with most of the decline concentrated in certificate and diploma enrolments. Degree-level participation has been comparatively stable. This long-run decline predates the Fees-Free policy by more than a decade and is an important contextual factor for the analysis.

3.2 Student Finance Prior to Fees-Free

Prior to 2018, New Zealand operated a mixed student finance system with three main components: tuition fees, student loans, and student allowances.

Tuition fees. Domestic students at public tertiary providers paid regulated tuition fees subject to an annual maximum fee movement (AMFM) cap. In 2017, the year before Fees-Free was introduced, average annual tuition fees for a full-time domestic student ranged from NZD 6,000 to NZD 7,000, depending on the programme and provider (Ministry of Education, 2024b). Universities typically charged higher fees than polytechnics or wānanga. However, these fees represent only a minority share of the total cost of tertiary education, with government funding covering the majority of provision costs. Estimates suggest that students contribute approximately 15–20 per cent of total costs through tuition fees, implying that tertiary education in New Zealand was already heavily subsidised prior to the introduction of Fees-Free (New Zealand Productivity Commission, 2017). As a result, Fees-Free reduced a cost that was already both deferred and heavily subsidised.

Student Loan Scheme. The Student Loan Scheme, administered by Inland Revenue, provides income-contingent loans for tuition fees, course-related costs, and living expenses (New Zealand Government, 2011; Inland Revenue, 2024). Since 2006, student loans have been interest-free for borrowers who remain resident in New Zealand. Repayments are deducted at a rate of 12 per cent of income above a threshold (NZD 22,828 per annum as of 2023/24). The interest-free feature substantially reduces the effective burden of future repayment obligations, lowering the weight placed on C^{future} in the enrolment decision. It has been described as a key reason why tuition fees are a relatively weak barrier to participation in New Zealand compared with systems that charge interest on student debt (Ministry of Education and Inland Revenue, 2023). In addition, because loans can fully cover tuition and are paid directly to providers, most students do not face upfront payment requirements, implying that $C^{\text{upfront}} \approx 0$ for a large share of potential entrants.

Student Allowances. Student allowances are non-repayable grants available to eligible students based on parental income (for those under 24) or personal circumstances (StudyLink, 2024). Allowance rates in 2017 were approximately NZD 230 per week for a single student living away from home. Not all students qualify: a significant share are ineligible due to parental income thresholds, and postgraduate students lost allowance eligibility in 2013 (New Zealand Treasury, 2012). For eligible students, allowances further reduce reliance on borrowing for living costs, partially offsetting opportunity costs, although these costs remain substantial for many students.

The combination of interest-free student loans and means-tested allowances meant that, by 2017, New Zealand already had one of the more generous student finance systems among OECD countries (OECD, 2022). Students could borrow the full cost of tuition without facing real interest charges, and lower-income students received additional grant support. This implies that the incremental financial relief provided by Fees-Free was limited in terms of both upfront constraints and the effective cost of future repayments.

3.3 The Fees-Free Policy

The Fees-Free tertiary education policy was a central commitment of the Labour-led government that took office in October 2017. The policy was announced as part of the government's first 100 days programme and implemented on 1 January 2018.

3.3.1 Design and Eligibility

The policy provided eligible first-time tertiary learners with up to one equivalent full-time student (EFTS) year of fee-free study, or two years of industry training, up to a cap of NZD 12,000 (including GST). Eligibility was restricted to domestic students enrolled at NZQCF Level 3 or above who had completed no more than half a full-time equivalent year of prior tertiary study. There were no age restrictions: both school leavers and adult learners with limited prior tertiary experience were eligible. The policy covered tuition fees at universities, polytechnics, wānanga, and eligible PTEs (Tertiary Education Commission, 2024).

The policy was administered by the Tertiary Education Commission (TEC), which verified eligibility and made payments directly to providers on behalf of students. Students did not need to apply separately; eligibility was automatically assessed based on enrolment records (Tertiary Education Commission, 2024).

3.3.2 Policy Objectives

The stated objectives of the Fees-Free policy were to: (i) increase participation in tertiary education, particularly among under-represented groups; (ii) reduce financial barriers to entry; (iii) reduce student debt; and (iv) support workforce development by enabling more people to gain qualifications. The government initially planned to extend the policy to two years of fee-free study by 2021 and three years by 2024 (New

Zealand Government, 2017), but this expansion was paused indefinitely in 2020 due to COVID-19 fiscal pressures.

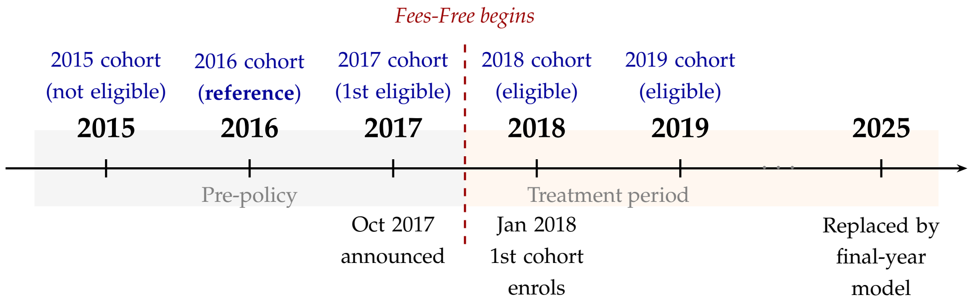
These objectives faced a demanding empirical test in the New Zealand context. Because the Student Loan Scheme already eliminated the upfront cost of tuition (i.e. $C^{\text{upfront}} \approx 0$) through interest-free borrowing (Section 3.2), the Fees-Free policy converted a deferred cost into no cost. However, it did not materially change the immediate cash-flow position of most students. The participation and equity objectives therefore required the policy to shift behaviour among students for whom the psychological or informational salience of "free" exceeded the financial relief already available through loans. In terms of the conceptual framework, the policy primarily reduced C^{future}, with effects depending on how strongly individuals value future repayment obligations. This institutional context is important for interpreting the results that follow.

The design of the policy was subsequently modified by the National-led government elected in 2023. From 2025, the Fees-Free entitlement was shifted from the first year of study to the final year, conditional on successful completion or progression. This change shifts the policy from targeting the participation margin to targeting completion. It does not affect the cohorts analysed in this paper, which entered tertiary study under the original first-year Fees-Free design.

3.3.3 Implementation Timeline

The policy timeline is relevant for defining the treatment period in the empirical analysis. Figure 1 summarises the key dates and maps school-leaver cohorts to their Fees-Free eligibility status.

Figure 1: Policy Timeline and Cohort Mapping



Note: Cohort year = year of school exit; students typically enrol in year $t+1$. Dashed line marks Fees-Free introduction (January 2018). Shaded areas indicate pre-policy (grey) and treatment (orange) periods. The policy covered approximately 40,000–65,000 learners per year at a cost of NZD 347M in 2022/23.

Source: Authors' elaboration based on TEC (2024) and Treasury (2023).

For this paper, "cohort year" refers to the year a student left secondary school, not the year of tertiary enrolment; the standard pathway is enrolment in year $t + 1$. This timing reflects the New Zealand education calendar: the secondary school year generally runs from February to November/December, while the main tertiary academic year begins in February or March. The treatment period begins with the 2017 school-leaver cohort (the first eligible for Fees-Free upon entering tertiary study in 2018). The 2016 cohort is the natural reference for the event study, and the 2015 cohort provides a pre-trend test (Figure 1).

Two caveats apply. First, some students do not follow the standard one-year gap pathway; a small proportion defer or enrol in the same year they leave school. We measure participation as enrolment in year $t + 1$, which captures the modal pathway. Second, the policy was announced in October 2017 and implemented in January 2018. Because the announcement came near the end of the 2017 secondary school year and only a few months before the 2018 tertiary year started, there was limited scope for earlier cohorts to anticipate the policy. The first treated cohort may also have had less time to adjust post-school plans in response to the policy than later treated cohorts.

3.4 Broader Context: Participation Decline and Labour Market Conditions

The Fees-Free policy was introduced against a backdrop of declining tertiary participation that had been underway since at least the mid-2000s. The most important contextual factor is the labour market. New Zealand experienced sustained economic growth and falling unemployment between 2012 and 2019, with youth unemployment (15–24 years) dropping from approximately 15 per cent in 2012 to under 10 per cent by 2017 (Stats NZ, 2024). Because tertiary enrolment is countercyclical—when labour markets tighten, the opportunity cost of foregone earnings rises, and marginal students are drawn towards direct employment (Dynarski and Scott-Clayton, 2013)—the pre-existing decline in participation is consistent with rising opportunity costs (L) in the enrolment decision.

Other potential confounds are less concerning. Total school-leaver cohort sizes remained relatively stable during the 2015–2019 period (approximately 49,000–52,000 per year), and the proportion achieving the National Certificate of Educational Achievement (NCEA) Level 2 or higher remained broadly constant. NCEA is New Zealand's main senior secondary school qualification; Level 2 is usually completed in the penultimate year of school and represents a common threshold of senior-secondary attainment. This suggests that neither demographic shifts nor changes in academic preparation explain the decline in participation across these five cohorts.

A final consideration is the COVID-19 pandemic. The 2019 school-leaver cohort enrolled in tertiary study from 2020 onwards and was the first to experience pandemic disruptions during its first year. The decision to participate was largely made before New Zealand's first lockdown in March 2020, but subsequent outcomes—particularly retention and completion—may be affected. The 2017 cohort was the last for which a three-year bachelor's degree could have been completed entirely before COVID-19, while the 2018 cohort's third year fell in 2021. The completion analysis should be interpreted with this in mind.

These contextual factors affect the utility margin of the enrolment decision and provide an important alternative explanation for the declining participation trend. The trend-adjusted model presented in Section 5 is designed to separate the pre-existing secular decline from any policy-specific effect.

3.5 Summary and Link to Conceptual Framework

These institutional features closely align with the conceptual framework outlined in Section 2. In particular, the availability of student loans that eliminate upfront tuition costs implies that $C^{\text{upfront}} \approx 0$ for most students, while the interest-free nature of borrowing reduces the effective burden of future repayment obligations. As a result, the Fees-Free policy primarily reduces C^{future} rather than relaxing constraints at the point of enrolment. This provides a useful setting for examining whether tuition subsidies affect behaviour when financial constraints are already attenuated.

4 Data and Sample Construction

This section describes the data and sample construction used to evaluate the impact of the Fees-Free policy. The design focuses on consecutive school-leaver cohorts around the policy introduction, allowing for the assessment of pre-existing trends and the identification of policy effects within a cohort-based framework.

4.1 Data Source

This study draws on de-identified administrative microdata from Statistics New Zealand's Integrated Data Infrastructure (IDI). The IDI is a large research database linking person-centred records from government agencies, surveys, and non-government organisations through probabilistic and deterministic matching to a central spine of unique identifiers (Stats NZ, 2025). All data access and analysis are conducted within the Stats NZ Data Lab, a secure computing environment that prevents the release of identifiable information.

The primary data collections used in this chapter are the Ministry of Education (MoE) schooling records, which capture enrolment spells, school characteristics, and the circumstances under which students leave secondary school; and the MoE tertiary education records, which capture enrolments, programme characteristics, and completion events at all New Zealand tertiary education organisations. These collections are linked at the individual level, enabling us to track each school leaver's transition into tertiary education and subsequent academic trajectory. This longitudinal linkage is central to the empirical design, as it allows outcomes to be measured consistently across cohorts before and after the policy change.

4.2 Analytical Sample

The analytical sample is constructed from five consecutive school-leaver cohorts: those who completed secondary school between 2015 and 2019. The 2017 cohort is the first eligible for Fees-Free tuition upon entering tertiary study in 2018, providing the treatment group. The 2015 and 2016 cohorts (pre-policy) and the 2018 and 2019 cohorts (continuing under the policy) enable both pre-trend assessment and the examination of medium-term effects. This cohort structure aligns closely with the timing of the policy and provides a natural setting for an event-study design.

Two restrictions improve the internal validity of the cohort-based comparison. First, the sample is limited to school leavers whose recorded reason for leaving is 'End of Schooling' (MoE code 20678), excluding those who transferred, were excluded, or emigrated. Second, the sample is restricted to students who achieved at least NCEA Level 2 or an equivalent qualification. This ensures comparability across cohorts in terms of academic preparedness and focuses the analysis on students for whom tertiary enrolment is a realistic margin of choice.[1]

After applying these restrictions, the final sample comprises 251,329 school leavers, with cohort sizes ranging from approximately 49,000 to 52,000 per year. Table 1 reports the sample composition. Because the analysis conditions on school leavers with sufficient academic attainment, the estimates should be interpreted as effects on the margin of tertiary participation among academically eligible students rather than the full population.

[1] The NCEA attainment codes retained are 25–27 (Level 2), 30–37 (Level 3 and University Entrance), 40 and 43 (Level 4 certificates), and 55–72 (equivalent qualifications under the National Qualifications Framework).

Table 1: Sample Composition by Cohort

	2015	2016	2017	2018	2019
N	49,833	50,117	51,869	50,471	49,039
Enrolled	28,564	27,165	27,615	26,277	25,591
Tertiary participation rate	57.3%	54.2%	53.2%	52.1%	52.2%

Note: Sample restricted to school leavers exiting via End of Schooling (code 20678) with NCEA Level 2 or equivalent. Total $N = 251{,}329$. Participation is measured on the full sample. Retention and completion are conditional on enrolment ($N = 135{,}212$). Composition is further restricted to students enrolled at NZQF Levels 3–7, excluding postgraduate enrolments ($N = 120{,}406$). All counts subject to Stats NZ random rounding (base 3).

Source: Authors' elaboration based on Stats NZ IDI (IDI_Clean_202510).

Cohort sizes are stable across the period, ranging from 49,039 (2019) to 51,869 (2017), indicating that the school-leaver population did not change substantially in size over these five years. Participation rates, however, declined monotonically from 57.3 per cent for the 2015 cohort to 52.2 per cent for the 2019 cohort. Notably, the largest single-year decline (-3.1 percentage points) occurred between 2015 and 2016—before the Fees-Free policy was introduced—foreshadowing the pre-trend that is examined formally in Section 6.3. This pattern underscores the importance of accounting for pre-existing trends when identifying policy effects.

4.3 Outcome Variables

The outcome variables are chosen to map directly to the mechanisms outlined in Section 2, capturing both the decision to enrol and subsequent educational trajectories. Four binary outcome variables are constructed to capture the full mechanism from access through to qualification attainment. Each is measured at the individual level.

Participation. A binary indicator equal to one if the school leaver enrolled in any tertiary programme in the year following school exit (i.e., leaving in year t, enrolled by $t+1$). Base rate: 54.2 per cent (2016). This captures the extensive margin of enrolment and corresponds to the primary decision margin in the conceptual framework.

Composition. Conditional on participation, a binary indicator equal to one if the student enrolled in a Bachelor's degree (NZQF Level 7), zero if sub-degree (Levels 3–6). Sample restricted to Levels 3–7 enrolments ($N = 120{,}406$), consistent with the Fees-Free eligibility threshold of Level 3. Base rate: 64.0 per cent (2016). This captures programme choice and allows for the possibility of substitution across qualification types.

Retention. Conditional on participation, a binary indicator equal to one if the student was still enrolled two years after leaving school ($t+2$). Sample restricted to enrolled students ($N = 135{,}212$). Base rate: 81.0 per cent (2016). This outcome assesses whether any participation effects translate into sustained engagement.

Completion. A binary indicator equal to one if the school leaver completed any tertiary qualification within six years of leaving school. This is measured among enrolled students ($N = 135{,}212$) and has a base rate of approximately 76 per cent for the 2016 cohort. This outcome captures longer-term attainment and allows assessment of whether initial enrolment effects translate into qualification completion. An important caveat applies to the 2019 cohort, which has had only five years of follow-up data (2020–2024) at the time of analysis; the 2019 coefficient should therefore be interpreted with caution due to right-censoring.

4.4 Control Variables and Socioeconomic Status

All models include a set of individual-level controls drawn from administrative records. Gender is measured as a binary indicator for female. Age at school leaving is centred at 17 (the modal leaving age). Ethnicity is captured through three non-mutually-exclusive indicators for Māori, Pacific peoples, and Asian, with New Zealand European and Other as the reference category. NCEA attainment is controlled through a set of indicator variables for the student's highest recorded attainment code at the time of leaving school, which accounts for the heterogeneity in academic preparation within the NCEA Level 2+ restriction.

The socioeconomic status (SES) of each student is proxied by the decile of the last secondary school attended. School deciles in New Zealand rank schools from 1 (most deprived catchment) to 10 (least deprived) based on a composite of census meshblock indicators, including household income, parental occupation, household crowding, educational qualifications, and receipt of means-tested benefits. A binary low-SES indicator is defined as deciles 1–4.

Linking school decile information in the IDI requires careful handling, as the underlying administrative table contains overlapping records for some schools. To obtain a unique decile per student, we retain the most recent record valid at the time of school exit. Schools classified as special schools (decile code 99) are excluded from the SES analysis.[2]

Because school decile is an area-level measure rather than an individual-level indicator, it introduces measurement error into the heterogeneity analysis. In a standard attenuation-bias framework, this pulls interaction coefficients toward zero, suggesting that the SES effects reported in Section 6.5 are likely *lower bounds* of the true socioeco-

[2] School decile ratings were reviewed periodically by the Ministry of Education. A school's decile reflected the socioeconomic characteristics of its catchment area, not the school's quality or performance. The decile system was in place for all cohorts in our study, but was replaced by a new Equity Index in 2023.

nomic gradient. These controls also enable the analysis of heterogeneous effects across demographic and socioeconomic groups, which is central to evaluating the equity implications of the policy.

Table 2 reports summary statistics for the control variables by cohort. The covariates are remarkably stable across the five cohorts, supporting the assumption that the composition of the school-leaver population did not change materially over this period and reinforcing the comparability of pre- and post-policy cohorts. The female share is approximately 50 per cent throughout. Mean age at school exit is constant at 17.5 years. The Māori share rises modestly from 20.9 to 21.7 per cent; the Pacific share increases from 12.0 to 13.0 per cent; and the Asian share rises from 13.3 to 14.0 per cent, consistent with gradual demographic shifts rather than abrupt compositional breaks. The proportion of students from low-decile schools (decile 1–4) is stable at approximately 23–24 per cent.

Table 2: Summary Statistics by Cohort

Cohort	Female (%)	Mean Age (years)	Māori (%)	Pacific (%)	Asian (%)	Low Decile (%)
2015	49.7	17.5	20.9	12.0	13.3	23.8
2016	49.8	17.5	21.8	12.2	13.3	24.1
2017	50.0	17.5	22.0	12.3	13.7	23.2
2018	49.9	17.5	21.9	12.7	14.0	23.5
2019	50.4	17.5	21.7	13.0	14.0	22.8

Note: Sample restricted to school leavers exiting via End of Schooling (code 20678) with NCEA Level 2 or equivalent. Cohort sizes: 2015 ($N = 49{,}833$), 2016 ($N = 50{,}117$), 2017 ($N = 51{,}869$), 2018 ($N = 50{,}471$), 2019 ($N = 49{,}039$). Total $N = 251{,}329$. Ethnicity indicators are non-mutually-exclusive (percentages may sum to more than 100). Low Decile = school decile 1–4. All statistics subject to Stats NZ confidentiality rules. Source: Stats NZ IDI (IDI_Clean_202510).

5 Empirical Strategy

The identification strategy exploits the introduction of the Fees-Free policy in 2018, which created a sharp discontinuity in the cost of first-year tertiary study for successive school-leaver cohorts. Identification comes from comparing outcomes across adjacent cohorts before and after the policy, under the assumption that, absent the reform, cohort-level differences would have evolved smoothly over time. The research design proceeds in four stages: a baseline event study to characterise the cohort-level dynamics; a discussion of identification assumptions; placebo- and trend-adjusted specifications to assess robustness; and heterogeneity analysis across socioeconomic and demographic subgroups.

Primary outcome and preferred specification. Our primary outcome is tertiary participation (enrolment within one year of school exit), and our preferred specification is the trend-adjusted model (Equation 6), which controls for the linear pre-existing cohort trend. This is pre-declared because participation most directly tests the policy's stated objective of expanding access, and the strong pre-trend documented in the placebo test (Section 6.3) necessitates trend adjustment for credible inference. The remaining three outcomes—composition, retention, and completion—are treated as secondary. For these, the event study specification (Equation 4) is preferred where the placebo test confirms that the parallel trends assumption holds. Accordingly, the choice of specification varies across outcomes depending on the validity of the identifying assumptions. Heterogeneity analyses across four subgroups and four outcomes are exploratory; we report all estimated interactions without correction for multiple comparisons and interpret patterns rather than individual coefficients (see Section 7).

5.1 Baseline Event Study

The baseline specification is a cohort-level event study that estimates year-specific deviations from a reference cohort (see Roth et al., 2023, for a general discussion of event study designs in difference-in-differences settings). We estimate the following linear probability model:

$$Y_i = \alpha + \sum_{c \neq 2016} \beta_c \cdot \mathbf{1}(\text{Cohort}_i = c) + \mathbf{X}_i' \gamma + \varepsilon_i \tag{4}$$

where Y_i is the binary outcome for individual i (participation, composition, retention, or completion); the sum runs over cohorts $c \in \{2015, 2017, 2018, 2019\}$ with 2016 as the omitted reference year; $\mathbf{X}_i$ is a vector of individual controls (gender, centred age, ethnicity indicators, and NCEA attainment fixed effects); and ε_i is a heteroskedasticity-robust error term. All models are estimated using OLS with robust (HC1) standard errors.

The 2016 cohort is chosen as the reference year because it is the last pre-policy cohort—students leaving school in 2016 entered tertiary study in 2017, the year immediately before Fees-Free took effect. The coefficient β_{2015} tests whether there was a pre-existing difference between the 2015 and 2016 cohorts. A statistically significant β_{2015} would indicate that outcomes were already changing before the policy, raising concerns about the parallel trends assumption. The coefficients β_{2017}, β_{2018}, and β_{2019} capture the post-policy dynamics for the first, second, and third Fees-Free cohorts, respectively.

This cohort-specific structure is useful because the first treated cohort faced a compressed implementation timeline, while later treated cohorts had more time to incorporate Fees-Free into post-school planning. If the limited response among the first treated cohort reflected late announcement rather than weak policy effects, we would expect larger or more positive effects for the 2018 and 2019 school-leaver cohorts.

The choice of a linear probability model (LPM) over a logistic or probit specification is motivated by three considerations. First, the LPM provides directly interpretable marginal effects without requiring transformation or evaluation at specific covariate values. Second, with a binary treatment variable and binary outcomes, the LPM with robust standard errors produces consistent estimates of average marginal effects (Angrist and Pischke, 2009). Third, the LPM facilitates straightforward interpretation of interaction terms in the heterogeneity analysis, whereas interaction effects in nonlinear models require additional computation and are not equivalent to the cross-derivative (Ai and Norton, 2003).

5.2 Identification Assumptions

The causal interpretation of the event study coefficients rests on the assumption that, absent the Fees-Free policy, outcomes for post-2016 cohorts would have followed the same trajectory as the 2015–2016 pre-period—the parallel trends assumption. In a standard two-period difference-in-differences framework, this assumption cannot be tested directly, but the availability of multiple cohorts allows us to assess its plausibility (Roth, 2022; Roth et al., 2023).

A key feature of this setting is that each individual belongs to exactly one cohort and has only one opportunity to transition from school to tertiary education. There is no within-individual variation over time. The design, therefore, relies on comparing outcomes across adjacent cohorts, assuming that cohort-level differences in unobserved characteristics are small. The inclusion of rich individual-level controls—particularly the NCEA attainment fixed effects, which capture granular variation in academic preparation—strengthens this assumption by absorbing observable sources of cohort heterogeneity.

One potential threat to identification arises from concurrent changes in the labour market or broader economy that may have differentially affected the opportunity cost of tertiary study across cohorts. For instance, improving labour market conditions between 2015 and 2019 could have drawn marginal students away from tertiary education independently of the fee change. We address this concern using the trend-adjusted models described below, which account for any linear cohort trend in outcomes. Given the presence of a pre-existing decline in participation, controlling for cohort trends is necessary for credible identification.

A related concern is that the trend-adjusted model assumes a *linear* cohort trend, and this assumption is doing significant analytical work. The descriptive data show that the participation decline was larger between 2015 and 2016 (-3.1 pp) than between subsequent adjacent cohorts (-1.0 to -1.1 pp), raising the possibility that the true pre-existing trend was nonlinear. If so, a linear trend adjustment could either over- or under-control for the secular decline, biasing the residual policy estimate in either direction. With only two pre-treatment cohorts, we cannot test for nonlinear trends or estimate higher-order specifications. This is acknowledged as a limitation of the design, and the event study estimates—which impose no functional form on the trend—serve as a complementary check. Accordingly, results should be interpreted with caution and viewed as conditional on the assumed trend specification.

A second concern relates to the COVID-19 pandemic, which may have affected outcomes for the 2017 and 2018 cohorts differently. Students in the 2017 cohort (entering tertiary study in 2018) could have completed a three-year bachelor's degree before the pandemic began in early 2020, while the 2018 cohort could not. This is primarily a concern for the completion outcome; for first-year outcomes (participation) and second-year outcomes (retention), pandemic effects are minimal because these are measured in 2018–2019 and 2019–2020, respectively, with only the latter partially overlapping with the pandemic's onset.

5.3 Placebo Tests and Trend-Adjusted Models

The event study for participation reveals a large and statistically significant coefficient for the 2015 cohort (+3.2 percentage points, $p < 0.001$), indicating that participation was already declining before the Fees-Free policy was introduced. This pre-trend complicates the causal interpretation of the post-policy coefficients, as part of the observed post-2016 decline may reflect a continuation of the pre-existing downward trend (Freyaldenhoven et al., 2019; Roth, 2022).

To formally test for pre-trends, we implement a placebo test that assigns treatment status to the 2016 cohort and restricts the sample to the 2015–2016 cohorts only:

$$Y_i = \alpha + \delta \cdot \text{Placebo}_i + \mathbf{X}_i'\gamma + \varepsilon_i \tag{5}$$

where $\text{Placebo}_i = 1$ if the student belongs to the 2016 cohort. A significant δ confirms that outcomes were changing between pre-policy cohorts, which would violate the parallel trends assumption required for a naive comparison of pre- and post-policy cohorts.

For participation, the placebo test yields a coefficient of -3.2 percentage points ($p < 0.001$), confirming the pre-existing decline. This motivates the use of a trend-adjusted specification that explicitly accounts for cohort trends. The trend-adjusted model is specified as follows:

$$Y_i = \alpha + \theta \cdot \text{FeesFree}_i + \lambda \cdot \text{Trend}_i + \mathbf{X}_i'\gamma + \varepsilon_i \tag{6}$$

where FeesFree_i is a binary indicator for cohorts eligible for the policy (2017–2019), and Trend_i is a linear function of the cohort year (centred at 2016). The parameter θ captures the policy effect net of any secular trend, while λ captures the annual rate of change in the outcome. This specification is more demanding than the naive event study, as it attributes to the policy only those changes that exceed what the pre-existing trend would predict.

For participation, the trend-adjusted model reduces the estimated Fees-Free effect by approximately 80 per cent—from -3.8 percentage points in the model without trend control to -0.8 percentage points with the linear cohort trend included ($p = 0.030$). The estimated annual trend is -1.2 percentage points per year. The trend-adjusted specification is our preferred model for participation. Incorporating a cohort trend for participation substantially attenuates the estimated policy effect, consistent with the presence of a strong pre-existing decline.

The placebo and trend-adjusted results for the remaining three outcomes are summarised in Table 3. For composition, the placebo coefficient is -0.3 percentage points ($p \approx 0.32$), confirming that the parallel trends assumption is satisfied. The trend-adjusted model yields a Fees-Free coefficient of -1.4 percentage points ($p < 0.001$) with a positive cohort trend of $+0.4$ percentage points per year; however, because the parallel trends assumption holds, the event study estimates are the preferred specification for this outcome.

For retention, the placebo coefficient is -0.5 percentage points ($p \approx 0.10$), not statistically significant, confirming that the parallel trends assumption is broadly satisfied. The trend-adjusted model yields a Fees-Free coefficient of -1.8 percentage points with a positive annual trend of $+0.7$ percentage points per year. The event study remains preferred.

Completion presents a more complex picture. The placebo coefficient is -1.1 percentage points ($p < 0.001$), confirming the small but significant pre-trend. The trend-adjusted model estimates a strong negative cohort trend of -2.4 percentage points per year and a Fees-Free coefficient that reverses sign to $+3.0$ percentage points. This sign reversal is driven by right-censoring of the 2019 cohort (five years of follow-up instead of six) and is not credible as a policy effect; see Section 6.3 for discussion.

Table 3: Placebo and Trend-Adjusted Estimates: All Outcomes

Outcome	Model	FF Effect (pp)	Trend/yr (pp)	p(FF)	N
Participation	Without trend	−3.8	—	< 0.001	251,329
	With trend	−0.8	−1.2	0.030	251,329
	Placebo	−3.2	—	< 0.001	99,950
Composition	Without trend	−0.5	—	< 0.05	120,406
	With trend	−1.4	+0.4	< 0.001	120,406
	Placebo	−0.3	—	0.32	—
Retention	Without trend	0.0	—	0.91	135,212
	With trend	−1.8	+0.7	< 0.001	135,212
	Placebo	−0.5	—	0.10	—
Completion	Without trend	−3.0	—	< 0.001	135,212
	With trend	+3.0	−2.4	< 0.001	135,212
	Placebo	−1.1	—	< 0.001	55,729

Note: pp = percentage points; — = not applicable. All models estimated as linear probability models with HC1 robust standard errors and full controls. Placebo tests restrict the sample to 2015–2016 cohorts with treatment assigned to 2016. Placebo N for composition and retention not shown as the test is estimated on enrolled students from the 2015–2016 cohorts only. The completion trend-adjusted result is unreliable due to right-censoring of the 2019 cohort; see text for discussion.

5.4 Heterogeneity Analysis

A central policy question is whether the Fees-Free policy differentially affected population groups that it was partly intended to benefit—particularly students from low socioeconomic backgrounds and Māori (the Indigenous population of New Zealand) and Pacific students. Māori and Pacific peoples are key groups of policy interest, as they have historically experienced lower rates of tertiary participation and attainment and are a central focus of equity-oriented education policy.

To examine this, we extend the event study specification to include interactions between the cohort indicators and subgroup membership (following the approach used in, e.g., Bartik et al., 2021; Gándara and Li, 2020):

$$Y_i = \alpha + \sum_{c \neq 2016} \beta_c \cdot \mathbf{1}(c) + \delta \cdot G_i + \sum_{c \neq 2016} \phi_c \cdot \mathbf{1}(c) \times G_i + \mathbf{X}_i'\gamma + \varepsilon_i \qquad (7)$$

where G_i is a binary indicator for the subgroup of interest (e.g., low SES, female, Māori, or Pacific). The main effect δ captures the baseline gap between the subgroup and the reference category in the 2016 cohort. The interaction coefficients ϕ_c are the parameters of primary interest: they indicate whether the cohort-specific deviations differ between the subgroup and the reference population. A positive and significant ϕ_c for a post-policy cohort would indicate that the subgroup experienced a relative improvement under the policy.

Four dimensions of heterogeneity are examined: (i) socioeconomic status, using the low-decile (1–4) school indicator; (ii) gender; (iii) Māori; and (iv) Pacific peoples. Each interaction model is estimated separately (i.e., one subgroup indicator at a time) to maintain interpretability and avoid collinearity between overlapping group indicators. Subgroup-specific sample sizes are reported in the notes to each heterogeneity table.

Table 4 summarises the estimation specifications.

Table 4: Summary of Estimation Specifications

Specification	Purpose	Key feature
Event study	Characterise cohort dynamics	2016 = reference; β_{2015} tests pre-trend
Placebo test	Formal test of pre-trend	2015–2016 only; 2016 = pseudo-treatment
Trend-adjusted	Isolate policy from secular trend	Linear cohort trend + Fees-Free indicator
Heterogeneity	Differential effects by subgroup	Cohort $\times$ Group interactions

Note: All specifications estimated as linear probability models with robust (HC1) standard errors.

6 Results

This section presents the empirical findings in five parts. We begin with descriptive trends in tertiary participation across the five school-leaver cohorts (6.1). We then report the event study estimates for participation (6.2) and the associated placebo and trend-adjusted robustness tests (6.3). The remaining three outcomes—programme composition, retention, and completion—are reported together in Section 6.4. The section concludes with the heterogeneity analysis by socioeconomic status, gender, and ethnicity across all four outcomes (6.5). Throughout, the results are interpreted in relation to the conceptual framework outlined in Section 2.

6.1 Descriptive Trends

Figure 2 plots the tertiary participation rate for each school-leaver cohort from 2015 to 2019. Participation declined monotonically from 57.3 per cent for the 2015 cohort to 52.2 per cent for the 2019 cohort, a cumulative fall of 5.1 percentage points over five years. The largest single-year decline occurred between 2015 and 2016 (-3.1 percentage points), *before* the Fees-Free policy was introduced. Subsequent declines were more modest: -1.0 percentage points between 2016 and 2017, -1.1 percentage points between 2017 and 2018, and essentially zero between 2018 and 2019 (+0.1 percentage points). Table 5 reports the cohort-level counts underlying these rates. Cohort sizes were relatively stable across the period (approximately 49,000–52,000 per year), ruling out compositional shifts in the denominator as a driver of the participation decline.

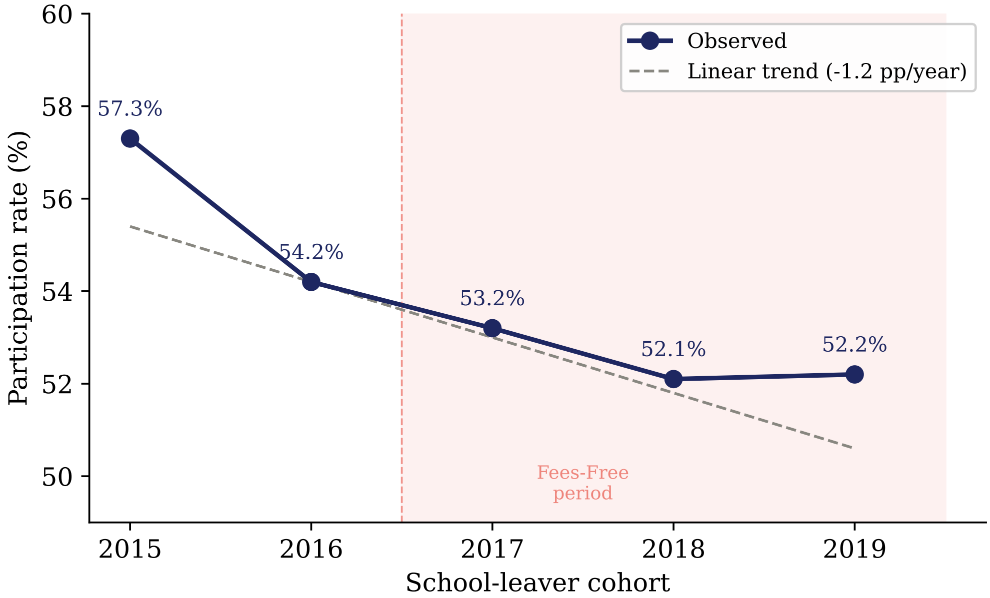
Table 5: Tertiary Participation by School-Leaver Cohort

Cohort	Enrolled	Total	Participation Rate (%)	Change (pp)
2015	28,564	49,833	57.3	—
2016	27,165	50,117	54.2	−3.1
2017	27,615	51,869	53.2	−1.0
2018	26,277	50,471	52.1	−1.1
2019	25,591	49,039	52.2	+0.1

Note: pp = percentage points; — = reference year. Sample restricted to school leavers exiting via End of Schooling (code 20678) with NCEA Level 2 or equivalent. All counts subject to Stats NZ confidentiality rules. Source: Stats NZ IDI (IDI_Clean_202510).

The pattern is notable: a steep decline between the two pre-policy cohorts (2015–2016) followed by a more gradual decline through the post-policy period. The fitted linear trend line (dashed in Figure 2) indicates an annual decline of approximately 1.2 percentage points. The presence of this pre-existing decline is central to the interpretation of the policy effects and is examined formally in Section 6.3.

Figure 2: Tertiary Participation Among School Leavers (2015–2019)



Note: Solid line = observed participation rates; dashed grey line = fitted linear trend (−1.2 pp/year). Vertical dashed red line marks Fees-Free introduction (2017 cohort); shaded area = post-policy period. $N = 251{,}329$. In this and subsequent figures: green markers = pre-policy, navy = reference year (2016), dark blue = post-policy, vertical bars = 95% CIs (HC1 robust SEs). Source: Stats NZ IDI.

6.2 Event Study: Participation

The event study reveals a strong pre-existing decline in participation that predates the Fees-Free policy. The 2015 coefficient is +3.2 percentage points ($p < 0.001$) relative to the 2016 base, indicating that participation was already falling before the policy was introduced (Table 6). This pre-trend violates the parallel trends assumption required for a naive before-and-after comparison.

Table 6: Event Study Estimates: Tertiary Participation (Base = 2016)

Cohort	Coefficient (pp)	SE (pp)	95% CI (pp)	p-value
2015	+3.2	0.3	[+2.6, +3.8]	< 0.001
2016	(base)	—	—	—
2017	−1.3	0.3	[−1.9, −0.7]	< 0.001
2018	−2.7	0.3	[−3.3, −2.1]	< 0.001
2019	−2.8	0.3	[−3.4, −2.2]	< 0.001

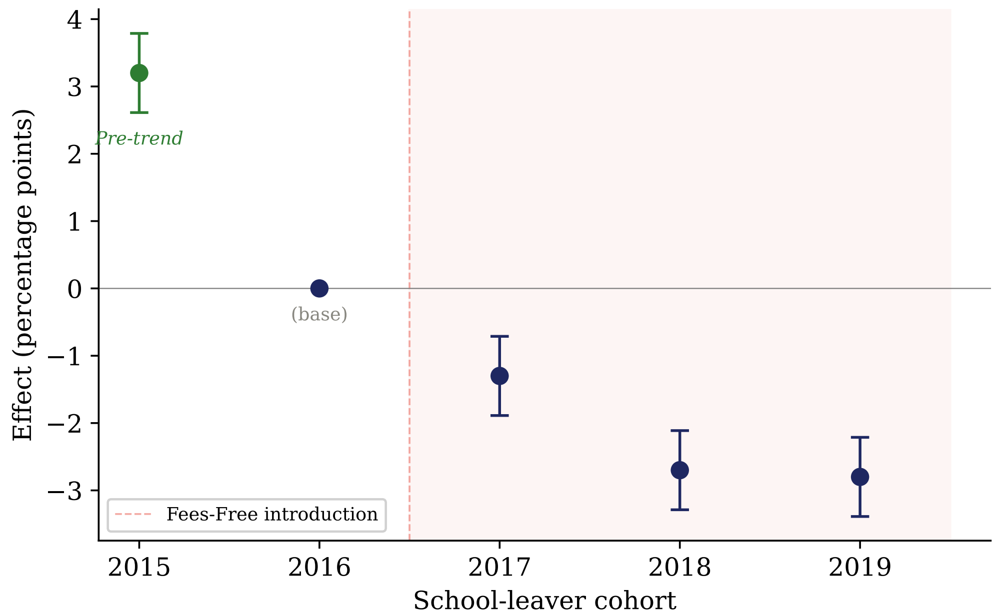
Note: pp = percentage points; — = not applicable (reference year). Linear probability model with HC1 robust standard errors. Controls: female, centred age, ethnicity indicators (Māori, Pacific, Asian), NCEA attainment fixed effects. $N = 251{,}329$. Base rate: 54.2% (2016 cohort).

The post-policy coefficients show continued decline: −1.3 percentage points for the 2017 cohort ($p < 0.001$), −2.7 for the 2018 cohort ($p < 0.001$), and −2.8 for the 2019 cohort ($p < 0.001$). However, the near-equal spacing of these coefficients—roughly 1.3 percentage points per year—is consistent with a continuation of the pre-existing linear trend rather than a discrete policy shock (Figure 3).

The later treated cohorts do not show evidence of a stronger participation response. This matters because the 2017 school-leaver cohort faced the shortest information and planning window. If the compressed implementation timeline had materially muted the first-cohort response, the 2018 and 2019 cohorts should show clearer evidence of increased participation. Instead, their estimates remain consistent with the continuation of the pre-existing downward trend.

This pattern is consistent with the conceptual framework outlined in Section 2. In a setting where upfront tuition costs are already negligible and borrowing costs are low, reductions in tuition primarily affect future repayment obligations rather than constraints at the point of enrolment. Under these conditions, large behavioural responses are not expected, and observed changes in participation are more plausibly driven by underlying trends than by the policy itself.

Figure 3: Event Study: Tertiary Participation (Base = 2016)



Note: Cohort-specific coefficients from the event study specification (LPM with full controls; 2016 = reference). See Figure 2 note for marker colour conventions. $N = 251{,}329$.

6.3 Placebo Tests and Trend-Adjusted Results

6.3.1 Participation

Table 7 reports the placebo and trend-adjusted results for participation. The placebo test—which assigns treatment status to the 2016 cohort and restricts the sample to the two pre-policy cohorts (2015 and 2016)—yields a coefficient of -3.2 percentage points ($p < 0.001$). The magnitude of this pre-policy decline is comparable to the post-policy cohort coefficients reported in Table 6.

Table 7: Placebo and Trend-Adjusted Estimates: Participation

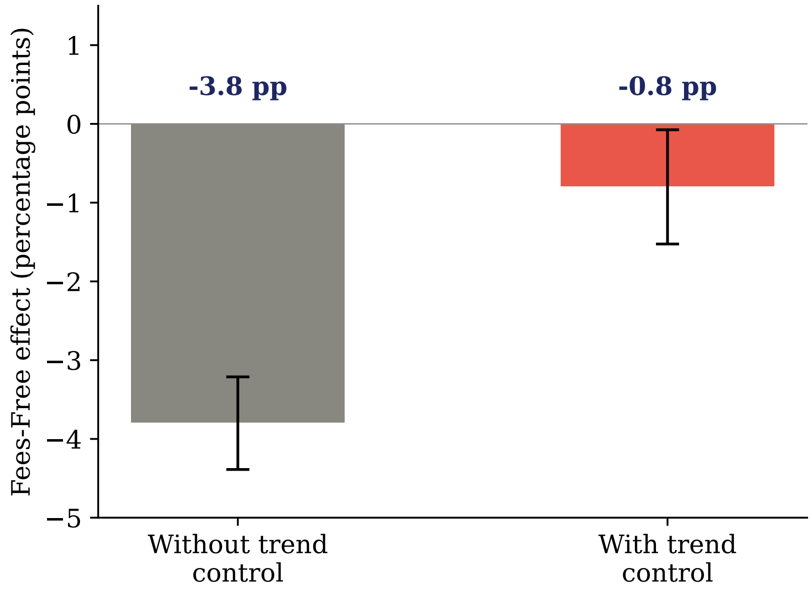
Model	FF Effect (pp)	Trend (pp/yr)	p-value (FF)	N
Without trend control	-3.8	—	< 0.001	251,329
With trend control	-0.8	-1.2	0.030	251,329
Placebo (2015–2016)	-3.2	—	< 0.001	99,950

Note: pp = percentage points; — = not applicable. All models estimated as linear probability models with HC1 robust standard errors and full controls. Placebo test restricts sample to 2015 and 2016 cohorts and assigns treatment to 2016.

The trend-adjusted model introduces a linear cohort trend alongside the Fees-Free indicator. Without the trend control, the estimated policy effect is -3.8 percentage points ($p < 0.001$). With the trend control, this estimate falls by approximately 80 per cent to -0.8 percentage points ($p = 0.030$), and the estimated annual trend is -1.2 percentage points per year (Figure 4). As declared in Section 5, the trend-adjusted specification is the preferred model for this outcome.

The attenuation of the estimated effect after controlling for the cohort trend suggests that much of the observed decline reflects underlying structural changes rather than a causal impact of the policy. This is consistent with the interpretation that the Fees-Free reform was largely inframarginal: it reduced the cost of education for individuals who would have enrolled in the absence of the policy, rather than relaxing binding constraints for marginal entrants.

Figure 4: Effect of Fees-Free on Participation: With and Without Trend Control



Note: Left bar = Fees-Free effect without trend control (-3.8 pp); right bar = with linear cohort trend (-0.8 pp, $p = 0.030$). Error bars = 95% CIs (HC1 robust SEs). LPM with full controls, $N = 251{,}329$.

6.3.2 Composition, Retention, and Completion

Table 3 (Section 5) reports the full placebo and trend-adjusted results across all four outcomes. The key findings for the remaining three outcomes are as follows.

Composition. The placebo coefficient (-0.3 pp, $p \approx 0.32$) confirms that parallel trends hold. The event study estimates in Table 8 are therefore the preferred specification. The trend-adjusted model yields a Fees-Free coefficient of -1.4 pp with a positive annual trend of $+0.4$ pp/year, suggesting a modest secular shift towards degree-level study. The trend-adjusted specification does not alter the substantive conclusion.

Retention. The placebo coefficient (-0.5 pp, $p \approx 0.10$) is not significant, supporting the parallel trends assumption. The event study in Table 9 remains the preferred specification. The trend-adjusted model yields a Fees-Free coefficient of -1.8 pp with a positive trend of $+0.7$ pp/year, consistent with the event study conclusion of no systematic policy effect apart from the 2019 improvement.

Completion. The placebo test yields a coefficient of -1.1 percentage points ($p < 0.001$), consistent with the small pre-trend identified in the event study. The trend-adjusted model produces a strong negative annual trend of -2.4 percentage points per year and a Fees-Free coefficient that reverses sign to $+3.0$ percentage points ($p < 0.001$).

This sign reversal coincides with the right-censoring of the 2019 cohort, which has had only five years to complete qualifications compared with six for earlier cohorts.

The shorter follow-up mechanically depresses the 2019 completion rate, steepening the estimated trend and inflating the policy coefficient. Given this limitation, the event study estimates in Table 10—which show small and largely non-significant post-policy coefficients for 2017 and 2018—provide a more informative basis for assessing completion. Section 7 discusses these results further.

Taken together, the results for composition, retention, and completion provide little evidence of aggregate policy effects beyond the participation margin. This is consistent with the conceptual framework: if the policy does not substantially alter participation, downstream outcomes are likewise expected to exhibit limited response, except insofar as the composition of marginal entrants changes.

6.3.3 Sensitivity to 2019 Cohort Exclusion

The 2019 cohort is subject to right-censoring (due to completion) and COVID-19 contamination (retention). Re-estimating all specifications after excluding 2019 confirms that coefficients for the 2015–2018 cohorts are virtually unchanged for both completion and retention, and the trend-adjusted Fees-Free effects become small and non-significant. Full results are reported in Appendix Table 14.

6.4 Secondary Outcomes: Composition, Retention, and Completion

The remaining three outcomes show no consistent aggregate policy effects. We report each briefly; the corresponding event study figures are provided in the Appendix (Figures 8, 9, and 10).

Programme composition. The placebo test confirms that parallel trends hold for this outcome (-0.3 pp, $p = 0.32$). Post-policy coefficients are mixed and small: a decline in 2017 (-0.9 pp, $p = 0.002$) is not sustained in 2018 or 2019 (-0.2 and $+0.2$ pp, both non-significant). The confidence intervals exclude effects larger than ± 1 percentage point, providing reasonably precise evidence of a null aggregate effect (Table 8).

Table 8: Event Study Estimates: Bachelor's Enrolment (Base = 2016)

Cohort	Coefficient (pp)	SE (pp)	95% CI (pp)	p-value
2015	$+0.3$	0.3	$[-0.3, +0.9]$	0.314
2016	(base)	—	—	—
2017	-0.9	0.3	$[-1.5, -0.3]$	0.002
2018	-0.2	0.3	$[-0.8, +0.4]$	0.608
2019	$+0.2$	0.3	$[-0.4, +0.8]$	0.579

Note: pp = percentage points. Sample restricted to enrolled students at NZQF Levels 3–7 ($N = 120{,}406$). LPM with HC1 robust SEs and full controls. Base rate: 64.0% (2016).

Retention. The pre-trend is not significant (+0.5 pp, $p = 0.128$), supporting parallel trends. The 2017 coefficient is marginally negative (−0.6 pp, $p = 0.067$), 2018 is essentially zero, and 2019 shows a significant improvement (+1.5 pp, $p < 0.001$). The positive 2019 coefficient should be interpreted with caution: it likely reflects pandemic-related factors, including labour market contraction, relaxed academic progress requirements, and institutional responses to COVID-19 (Table 9).

Table 9: Event Study Estimates: Year 2 Retention (Base = 2016)

Cohort	Coefficient (pp)	SE (pp)	95% CI (pp)	p-value
2015	+0.5	0.3	[−0.1, +1.1]	0.128
2016	(base)	—	—	—
2017	−0.6	0.3	[−1.2, +0.0]	0.067
2018	0.0	0.3	[−0.6, +0.6]	0.906
2019	+1.5	0.3	[+0.9, +2.1]	< 0.001

Note: pp = percentage points. Sample restricted to enrolled students ($N = 135{,}212$). LPM with HC1 robust SEs and full controls. Base rate: 81.0% (2016).

Completion. Post-policy coefficients for 2017 and 2018 are small (−0.7 and −0.5 pp). The 2019 coefficient (−6.4 pp) reflects mechanical right-censoring—this cohort has had only five years of follow-up—and should not be interpreted as a policy effect. The confidence intervals for 2017 and 2018 are consistent with effects no larger than ±1.3 percentage points (Table 10).

Table 10: Event Study Estimates: Completion within 6 Years (Base = 2016)

Cohort	Coefficient (pp)	SE (pp)	95% CI (pp)	p-value
2015	+1.1	0.3	[+0.5, +1.7]	0.001
2016	(base)	—	—	—
2017	−0.7	0.3	[−1.3, −0.1]	0.037
2018	−0.5	0.3	[−1.1, +0.1]	0.112
2019†	−6.4	0.4	[−7.2, −5.6]	< 0.001

Note: pp = percentage points. Sample restricted to enrolled students ($N = 135{,}212$). LPM with HC1 robust SEs and full controls. Base rate: ≈76% (2016). †Right-censored: 5 years of follow-up instead of 6.

In sum, the three secondary outcomes provide no evidence of aggregate policy effects on programme choice, persistence, or qualification attainment. The 2019 retention improvement is more plausibly attributed to COVID-19 than to Fees-Free, and the 2019 completion estimate is unreliable due to right-censoring.

These findings align with evidence from other settings where tuition costs are not the primary constraint on enrolment, and where financial aid primarily reduces student debt rather than increasing participation (Declercq and Verboven, 2015; Ponce and Loayza, 2012).

6.5 Heterogeneous Effects

This subsection examines whether the Fees-Free policy had differential effects across four population subgroups: students from low-SES schools (decile 1–4), women, Māori, and Pacific peoples. Each interaction model (Equation 7) is estimated separately. Approximate subgroup sizes: low-SES $\approx$ 59,000 (23%), women $\approx$ 125,000 (50%), Māori $\approx$ 55,000 (22%), Pacific $\approx$ 31,000 (12%). Full heterogeneity results for retention and completion—which show few significant effects—are reported in Appendix Tables 15 and 16.

The most notable finding concerns the widening of the *socioeconomic participation gap* during the Fees-Free period (Table 11; Figure 5). Low-SES students had a participation rate 4.2 percentage points lower than that of their higher-SES peers in 2016. The interaction coefficients are negative and significant in all three post-policy years: -1.8 pp in 2017, -1.5 in 2018, and -2.5 in 2019. The pre-trend interaction ($+2.1$ pp) is positive, suggesting the gap had been *narrowing* before the policy, making the post-policy reversal particularly striking.

Women showed a modest relative improvement by 2019 ($+2.3$ pp). Neither Māori nor Pacific students showed statistically significant differential effects in any post-policy year.

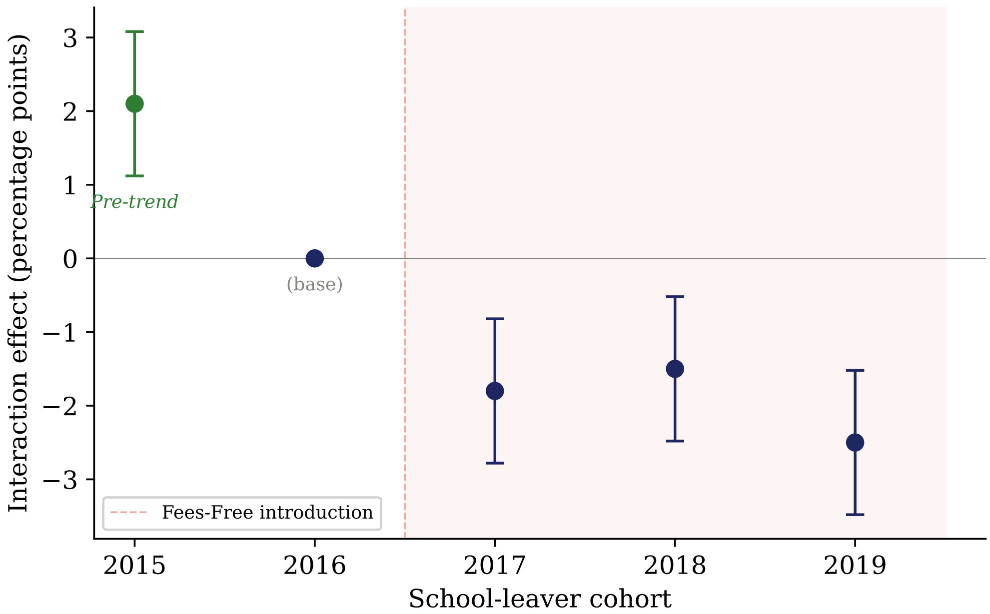
These results imply that the policy did not improve access for disadvantaged students and, in relative terms, widened existing participation gaps. This pattern is consistent with the conceptual framework: when upfront financial constraints are already weak, reducing tuition fees does not differentially benefit students who are credit-constrained. Instead, the incidence of the subsidy reflects existing participation patterns, directing resources toward students who are already more likely to enrol. In this sense, the policy primarily operates as a transfer to inframarginal students rather than as a tool for reducing socioeconomic disparities in access.

Table 11: Heterogeneous Effects: Participation

Group	Base Gap (pp)	2015 (pp)	2017 (pp)	2018 (pp)	2019 (pp)	Pattern
Māori	−8.0	+1.7	+0.4	−0.7	−0.9	No effect
Pacific	+1.5	+1.8	+0.9	−0.9	−0.7	No effect
Women	+4.3	−1.3	+0.6	+0.4	+2.3*	+2019
Low SES	−4.2	+2.1	−1.8*	−1.5*	−2.5*	**Widened**

Note: pp = percentage points. * denotes $p < 0.05$. SEs range from 0.6 to 0.9 pp. LPM with HC1 robust SEs and full controls. $N = 251{,}329$.

Figure 5: Event Study: Low SES Interaction (Participation)



Note: Interaction coefficients from the cohort-by-SES specification with 95% CIs (HC1 robust SEs). Dashed line = Fees-Free introduction; shaded area = post-policy period. $N = 251{,}329$.

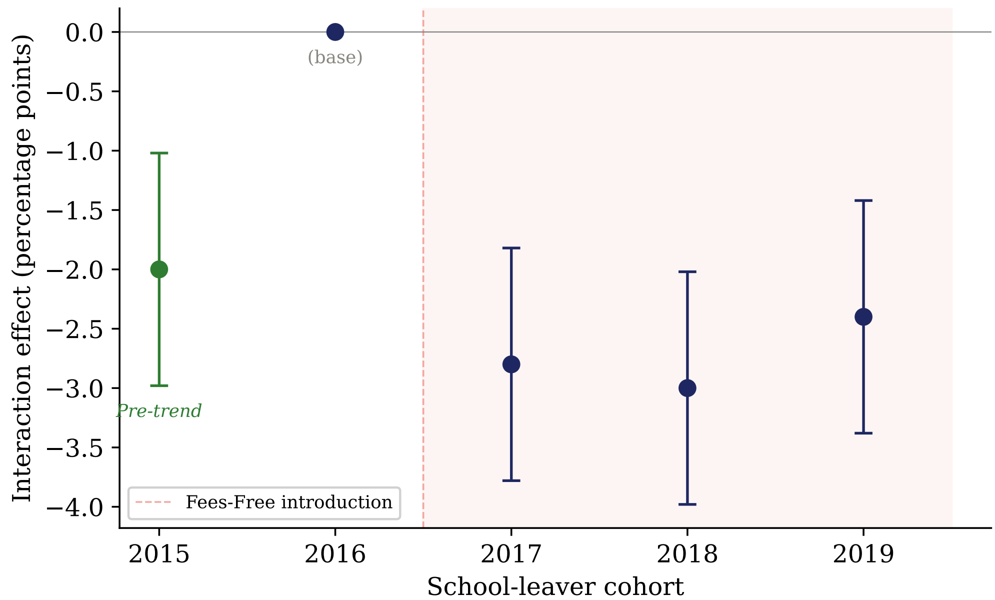
The composition gap also widened (Table 12; Figure 6). Low-SES students had a baseline gap of −8.4 pp in Bachelor's enrolment. Interaction coefficients are negative and significant across all post-policy years (−2.8, −3.0, −2.4 pp), with a significant pre-trend (−2.0 pp) suggesting the gap was already widening—though the post-policy interactions are larger. Not only were low-SES students less likely to participate, but those who did enrol were also *less likely to choose a degree-level programme*. Māori students showed a significant negative interaction in 2018 only (−2.4 pp). Pacific students and women showed no differential effects.

Table 12: Heterogeneous Effects: Bachelor's Enrolment

Group	Base Gap (pp)	2015 (pp)	2017 (pp)	2018 (pp)	2019 (pp)	Pattern
Māori	−5.1	−1.0	−0.3	−2.4*	−1.2	−2018
Pacific	−9.3	0.0	0.0	0.0	0.0	No effect
Women	+1.2	0.0	0.0	0.0	0.0	No effect
Low SES	−8.4	−2.0*	−2.8*	−3.0*	−2.4*	**Widened**

Note: pp = percentage points. * denotes $p < 0.05$. Sample restricted to NZQF Levels 3–7 ($N = 120{,}406$). LPM with HC1 robust SEs and full controls.

Figure 6: Event Study: Low SES Interaction (Bachelor's Enrolment)



Note: Interaction coefficients from the cohort-by-SES specification with 95% CIs (HC1 robust SEs). Sample restricted to NZQF Levels 3–7 ($N = 120{,}406$).

Heterogeneity results for retention and completion are less striking (Appendix Tables 15 and 16). Low-SES students experienced a significant decline in retention in 2018 (−2.3 pp), plausibly linked to their compositional shift towards sub-degree programmes. Pacific students also showed a negative retention interaction in 2018 (−2.1 pp). Women improved in retention by 2019 (+2.2 pp), consistent with the participation finding. For completion, no subgroup showed statistically significant differential effects in any year, suggesting the equity problem is concentrated at the access and early-stage margins rather than in long-run persistence.

6.6 Summary of Findings

Table 13 synthesises the main findings across all four outcomes and population subgroups.

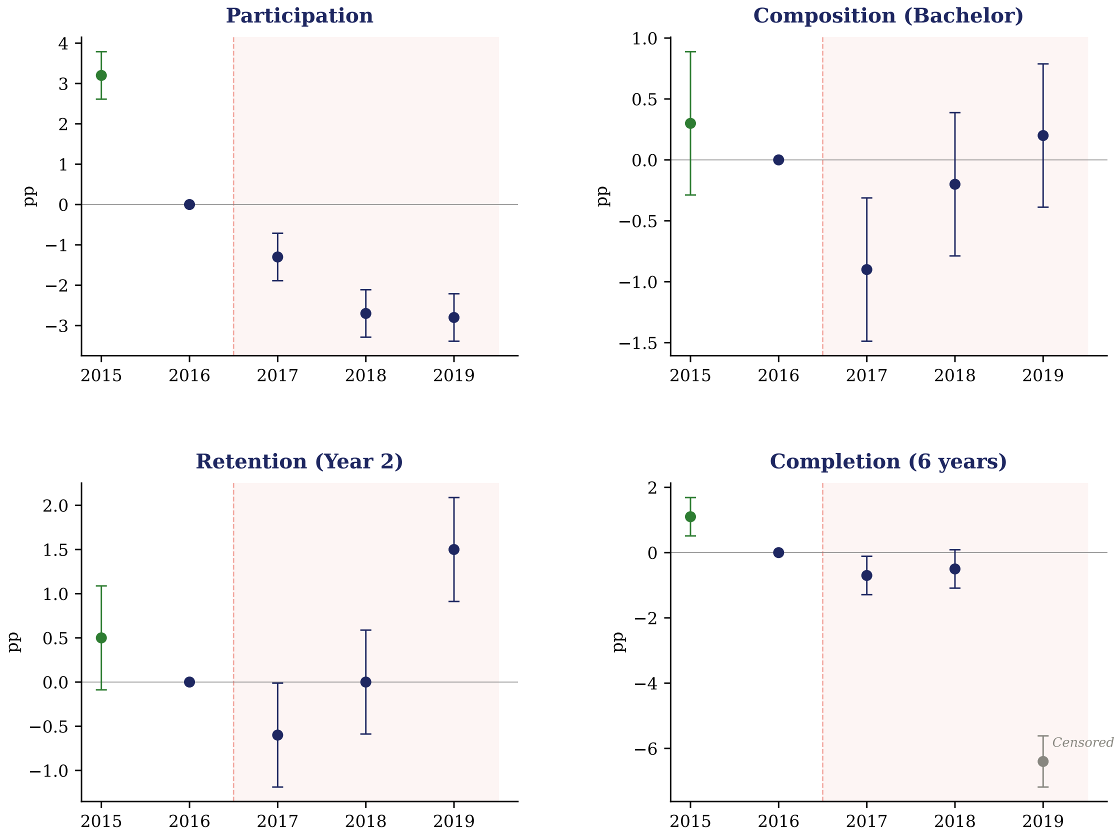
Table 13: Summary of Key Findings

Outcome	Aggregate	Māori (pp)	Pacific (pp)	Women (pp)	Low SES (pp)
Participation	−1.2 pp/yr trend	—	—	+2.3* (2019)	−2.5* (2019)
Composition	No effect	−2.4* (2018)	—	—	−3.0* (2018)
Retention	+1.5 pp in 2019	—	−2.1* (2018)	+2.2* (2019)	−2.3* (2018)
Completion	No effect	—	—	—	—

Note: pp = percentage points; — = no statistically significant effect. * denotes $p < 0.05$. Subgroup columns report the largest significant interaction coefficient for each outcome×subgroup combination, with the cohort year in parentheses; see Tables 11–16 for full results. Linear probability models with HC1 robust standard errors and full controls.

Figure 7 provides a visual overview of the event study coefficients across all four outcomes.

Figure 7: Event Study Estimates Across Four Outcomes (Base = 2016)



Note: Each panel shows cohort-specific coefficients with 95% CIs (HC1 robust SEs). 2016 = reference year. Grey marker in Completion panel = right-censored 2019. Y-axis scales differ across panels. *N*: Participation = 251,329; Composition = 120,406; Retention and Completion = 135,212.

Four overarching findings emerge from the analysis. First, tertiary participation shows a strong pre-existing downward trend (−3.2 pp in the placebo test; −1.2 pp per year in the trend model), whereas the other three outcomes show substantially smaller or non-significant pre-trends, suggesting that the parallel trends assumption is more plausible for composition, retention, and completion than for participation. Second, after controlling for this trend, the estimated Fees-Free effect on participation falls by 80 per cent and becomes only marginally significant; composition, retention, and completion show no consistent aggregate policy effect. Third, the socioeconomic participation gap *widened* during the policy period: low-SES students experienced relative declines in participation (−2.5 pp by 2019), Bachelor's enrolment (−3.0 pp), and retention (−2.3 pp in 2018), though completion shows no differential SES effect. Fourth, women showed modest relative improvements in participation (+2.3 pp) and retention (+2.2 pp) by 2019, while neither Māori nor Pacific students exhibited significant differential effects on any outcome.

Taken together, these results provide evidence for a boundary condition in the effectiveness of tuition subsidies: when upfront costs are already low and borrowing constraints are limited, reducing tuition has little effect on participation and primarily operates as an income transfer to inframarginal students. The following section develops these interpretations and discusses their implications for policy and the broader literature.

7 Discussion

This section interprets the findings through the lens of the institutional setting in which Fees-Free was introduced, and considers their implications for policy design. The results suggest that the limited aggregate impact of the policy reflects the nature of the constraints facing marginal students, rather than weak implementation. In particular, they highlight the importance of distinguishing between policies that reduce upfront costs and those that primarily affect deferred repayment obligations. We organise the discussion around four themes: the pre-existing trend, equity effects, behavioural mechanisms, and external validity.

7.1 The Pre-Existing Trend and the Limited Aggregate Effect

The central result is that the decline in tertiary participation during the Fees-Free period is largely explained by a pre-existing downward trend, rather than a discrete policy effect. The placebo test confirms that participation was already falling at approximately 3.2 percentage points between the 2015 and 2016 cohorts—before the policy was introduced. After controlling for a linear cohort trend, the estimated Fees-Free effect on participation falls by 80 per cent, from -3.8 to -0.8 percentage points, and becomes only marginally significant ($p = 0.030$).

In effect, the policy removed a price that was already weakly binding for most students. This interpretation is consistent with the international meta-evidence. Havránek et al. (2024) find that the average tuition–enrolment elasticity is close to zero after correcting for publication bias, with meaningful effects concentrated where tuition reductions are large relative to pre-existing barriers. In New Zealand, the Student Loan Scheme already eliminated upfront tuition costs through interest-free loans. Fees-Free therefore converted a deferred cost into no cost at all, but did not change the immediate cash-flow position of most students. That is, the policy reduced the long-run cost of education without materially altering the constraints faced at the point of enrolment.

This interpretation aligns with the Ministry of Education's supplementary analysis (Ministry of Education, 2024a), which concluded that the first-year Fees-Free policy did not have a measurable impact on overall participation or completion once underlying trends were accounted for. It is also consistent with Nikula and Morris Matthews's (2018) ex ante assessment, which predicted limited effects in a system where liquidity constraints were already largely addressed.

The finding contrasts with the larger effects documented for U.S. promise programmes, where tuition subsidies of similar magnitude generated enrolment increases of 5–10 percentage points (Bartik et al., 2021; Anderson et al., 2024). The key distinction is institutional rather than behavioural: in settings where students face upfront costs without income-contingent lending, fee removal represents a large change in effective price; in New Zealand, it does not. The results therefore support the view that tuition is not the primary constraint for marginal students in this context.

From a cost-effectiveness perspective, the policy appears unlikely to have been an efficient way to expand participation. The policy subsidised approximately 40,000–65,000 students per year at a cost of NZD 350 million, the vast majority of whom would have enrolled regardless. As Dynarski and Scott-Clayton (2013) note, untargeted subsidies tend to be less efficient than need-based aid when a large share of recipients are infra-marginal. More generally, when a policy reduces costs that are not binding at the point of decision, its fiscal cost may be large relative to its behavioural impact.

A back-of-the-envelope calculation illustrates the scale of this issue. The preferred trend-adjusted estimate is small, negative, and only marginally statistically significant: -0.8 percentage points. It therefore does not provide evidence of additional enrolments. However, even if one interprets the absolute size of this estimate as an upper-bound behavioural response, it would imply at most around 400 affected enrolments per cohort ($0.008 \times 50{,}000$).[3] At an annual programme cost of approximately NZD 350 million, the implied cost per affected enrolment decision would exceed NZD 800,000. This calculation is illustrative, but it reinforces the central point: when most recipients would have enrolled anyway, a universal tuition subsidy can involve substantial fiscal cost with limited behavioural change.

An instructive contrast is provided by the Targeted Training and Apprenticeship Fund (TTAF), introduced in July 2020 as part of New Zealand's COVID-19 recovery package (Tertiary Education Commission, 2020). The TTAF covered full tuition fees for vocational programmes and coincided with a visible increase in sub-degree enrolments. However, it was introduced during a sharp labour market downturn, when the opportunity cost of study fell. Taken together, the comparison suggests that the effectiveness of fee subsidies depends on both their magnitude and the broader economic context: first-year fee removal in a tight labour market may represent the least favourable combination for generating participation effects.

[3] This calculation is illustrative. Because the preferred point estimate is negative and only marginally statistically significant, it should not be interpreted as a cost per additional enrolment. Rather, it indicates the scale of fiscal expenditure relative to even a generous upper-bound behavioural response.

7.2 Equity: Why Did the SES Gap Widen?

The heterogeneity analysis shows that students from low-SES schools experienced a relative deterioration across participation, composition, and retention. These are relative effects—the gap between low- and higher-SES students widened during the policy period.

Three mechanisms could explain this pattern: non-financial barriers, opportunity cost differences, and compositional selection. The joint pattern of results is consistent with compositional selection, although the mechanism cannot be directly identified in this design.

The first explanation is that non-financial barriers dominate. New Zealand evidence suggests that academic preparation, school achievement, and information constraints are the primary determinants of participation (Meehan et al., 2017; Earle, 2018), consistent with Carneiro and Heckman (2002). If marginal non-participants were constrained by these factors, removing fees would not materially change their behaviour.

A second explanation is that opportunity cost effects disproportionately affected low-SES students. The strong labour market during 2015–2019 may have increased the relative attractiveness of employment, particularly for students with weaker academic pathways (Stats NZ, 2024).

The third explanation is compositional selection. If Fees-Free induced some marginal low-SES students to enrol, and these students faced weaker academic preparation or other barriers to persistence, they may have been more likely to enter sub-degree programmes and less likely to remain enrolled. This mechanism is consistent with the simultaneous widening of participation, composition, and retention gaps, as well as survey evidence showing weaker academic adjustment among policy-influenced students (Sotardi et al., 2019, 2020).

This interpretation is also consistent with the broader mechanism emphasised in this paper. When upfront financial constraints are weak, reducing tuition fees does not differentially benefit disadvantaged students, whose barriers are more likely to be non-financial. Instead, the incidence of the subsidy reflects existing participation patterns, directing resources toward students who are already more likely to enrol.

This interpretation shifts the emphasis from access alone to preparedness and early transition: the policy may have altered the composition of entrants without changing the underlying constraints that shape success within tertiary education. While we cannot directly identify the mechanism, the observed pattern—widening gaps in both entry and programme choice—is difficult to reconcile with pure information or labour-market explanations alone.

It is notable that completion shows no differential SES effect. This suggests that conditional on progressing through the early stages of study, disadvantaged students complete at similar rates. The equity problem is therefore concentrated at the access and early-transition margins rather than in long-run persistence.

These findings echo international evidence. In Chile, Estonia, and Massachusetts, universal fee abolition has generally failed to close socioeconomic gaps in access to selective programmes (Bucarey, 2018; Masso and Sille, 2023; Cohodes and Goodman, 2014). Across contexts, universal subsidies appear to be a blunt instrument: they reduce average costs but do not target the barriers most relevant for disadvantaged students.

7.3 Programme Composition and the Substitution Margin

The composition results suggest no aggregate shift in programme choice, but a widening SES gap in degree enrolment.

This pattern is consistent with the policy operating along a substitution margin within the tertiary system rather than at the entry margin. That is, Fees-Free did not increase overall participation but may have influenced the distribution of students across programme types.

International evidence shows similar patterns, with tuition subsidies shifting students across sectors without increasing total enrolment (Gurantz, 2019; Cohodes and Goodman, 2014). In New Zealand, the relevant margin is between degree and sub-degree programmes.

An institutional feature reinforces this interpretation. The policy covered all NZQF Level 3+ qualifications, including short certificates with relatively low entry requirements. For academically marginal students, these programmes represent the lowest-cost pathway into eligibility. If the policy primarily affected the decision to enrol in any programme rather than the type of programme, it would generate distributional changes without aggregate shifts—precisely the pattern observed.

7.4 The Gender Effect

Women show modest relative improvements in participation and retention by the 2019 cohort. However, these effects emerge only in the final cohort and are small in magnitude, making it difficult to attribute them to the policy with confidence.

One possibility is that the policy reinforced existing gender differences in participation. Another is that increased awareness over time affected groups differently. A third is that unrelated institutional or labour market factors drove the change.

Given the timing and size of the effects, they should be interpreted as suggestive rather than causal.

7.5 Limitations

Several limitations should be noted when interpreting these results.

Pre-trend identification. The strong pre-existing trend in participation complicates causal inference. Although the trend-adjusted model is our preferred specification, the linear trend assumption is itself a modelling choice: if the true underlying process were nonlinear, the residual policy effect could be larger or smaller than our estimate. With only two pre-treatment cohorts (2015 and 2016), we cannot test for nonlinear trends.

Policy timing, anticipation, and compressed implementation. The policy was announced in October 2017 and implemented in January 2018. A standard concern in policy evaluations is that people may adjust their behaviour in anticipation of a reform, contaminating the pre-policy comparison group. This concern is limited for the primary participation outcome because the 2016 reference cohort is measured by enrolment in 2017, before Fees-Free was announced. However, anticipation could still matter for less standard pathways if some 2016 school leavers delayed tertiary entry until 2018 to become eligible. Such students are not counted as participating in year $t + 1$, so this would mainly affect interpretation of delayed-entry responses rather than the main participation estimate. For the 2017 school-leaver cohort, the issue is different: the policy was announced before NCEA results and before final tertiary enrolment decisions, but late in the post-school planning cycle. Estimates for this cohort should therefore be interpreted as the effect of eligibility under a compressed implementation timeline. The later treated cohorts help address this concern: the absence of larger participation effects for the 2018 and 2019 cohorts suggests that the limited aggregate response is unlikely to be explained solely by late announcement or short-run implementation constraints.

Cohort window. Five cohorts provide limited statistical power for separating trend effects from policy effects. Additional pre-treatment cohorts (e.g. 2012–2014) would strengthen the trend estimation, but the IDI data structure and sample restrictions limit the feasible window.

COVID-19 contamination. The 2019 cohort enrolled in tertiary study from 2020 onwards, coinciding with the COVID-19 pandemic. While the participation decision was largely made before the pandemic, the retention and completion outcomes for this cohort may be contaminated by pandemic-related disruptions. The completion estimate for the 2019 cohort is also mechanically right-censored (five years instead of six).

School decile as an SES proxy. School decile is an area-level measure of socioeconomic disadvantage, not an individual-level indicator. It captures the average characteristics of the school's catchment area rather than students' household incomes, introducing non-classical measurement error. In a standard attenuation-bias framework, this biases the interaction coefficient towards zero, implying that our estimated SES gaps are likely *lower bounds* of the true differential effects.

The IDI includes individual-level proxies—including Working for Families tax credits, benefit receipt, and parental income from IRD records—that could be used in future work to construct a more precise SES measure. School decile is advantageous because it is predetermined, avoiding endogeneity concerns from post-school-leaving financial variables.

Single-margin analysis. This paper evaluates the effects of Fees-Free on school leavers who completed schooling normally with NCEA Level 2 or above. It does not capture effects on adult learners, early school leavers, or those returning to study after an extended break—populations that may also have been affected by the policy.

Multiple testing. The heterogeneity analysis yields approximately 48 subgroup$\times$outcome$\times$cohort tests without formal multiple-comparison corrections. A Bonferroni correction would raise the threshold to approximately $p < 0.001$, under which most low-SES results would remain significant. We emphasise that the heterogeneity findings should be interpreted as documenting *patterns* across outcomes rather than relying on any single coefficient.

Absence of geographic variation. The Fees-Free policy was implemented nationally and simultaneously for all eligible students, precluding difference-in-differences identification with region-by-cohort fixed effects or placebo geographies. International comparisons are infeasible given differences in data systems and concurrent policy changes. The cohort-based event study with placebo and trend-adjusted specifications represents the strongest feasible identification strategy in this context.

8 Conclusion

This paper evaluates the impact of New Zealand's 2018 Fees-Free tertiary education policy in a setting where tuition costs were already largely deferred through interest-free student loans and substantial public subsidies. Using population-level administrative data and a cohort-based empirical design, the analysis examines participation, programme composition, retention, and completion across successive school-leaver cohorts.

The central finding is that Fees-Free had little effect on tertiary participation once pre-existing trends are taken into account, and no consistent effect on programme choice, retention, or completion. The decline in participation observed over the policy period is largely explained by a pre-existing downward trend rather than a discrete policy effect. Across all outcomes, the results are consistent with a setting in which tuition costs are not the primary constraint on enrolment or progression.

The distributional results reinforce this interpretation. Socioeconomic gaps in participation and degree enrolment widened during the policy period, indicating that the policy did not improve access for disadvantaged students. Conditional on persistence, however, completion rates are similar across groups, suggesting that the primary equity challenge lies at the point of entry and early transition into tertiary study.

Taken together, these findings highlight a central mechanism: when upfront financial constraints are already weak, reducing tuition fees primarily lowers future repayment obligations rather than altering behaviour at the point of enrolment. In such settings, tuition subsidies tend to operate as transfers to infra-marginal students rather than as instruments for expanding participation.

These results have three implications for tertiary education policy. First, universal fee subsidies are unlikely to increase participation where financial barriers are already substantially mitigated. More broadly, the findings identify a boundary condition for the effectiveness of tuition subsidies: their impact depends on whether they reduce costs that are binding at the point of decision. Second, the widening of socioeconomic gaps suggests that tuition fees are not the primary barrier for disadvantaged students. Constraints related to academic preparation, information, and living costs are likely to play a more important role, and policies that do not target these factors are unlikely to improve equity outcomes. Third, the results imply that universal tuition subsidies may be a relatively costly way of expanding access. When such policies primarily benefit students who would have enrolled in the absence of the subsidy, the fiscal cost per additional enrolment is likely to be high relative to more targeted interventions.

Overall, the findings suggest that the effectiveness of tuition subsidies depends critically on institutional context. In systems where upfront costs are already low and borrowing constraints are limited, reducing tuition fees has limited effects on participation and may not improve, and may even worsen, equity. Designing effective policy therefore requires aligning instruments with the constraints they are intended to address.

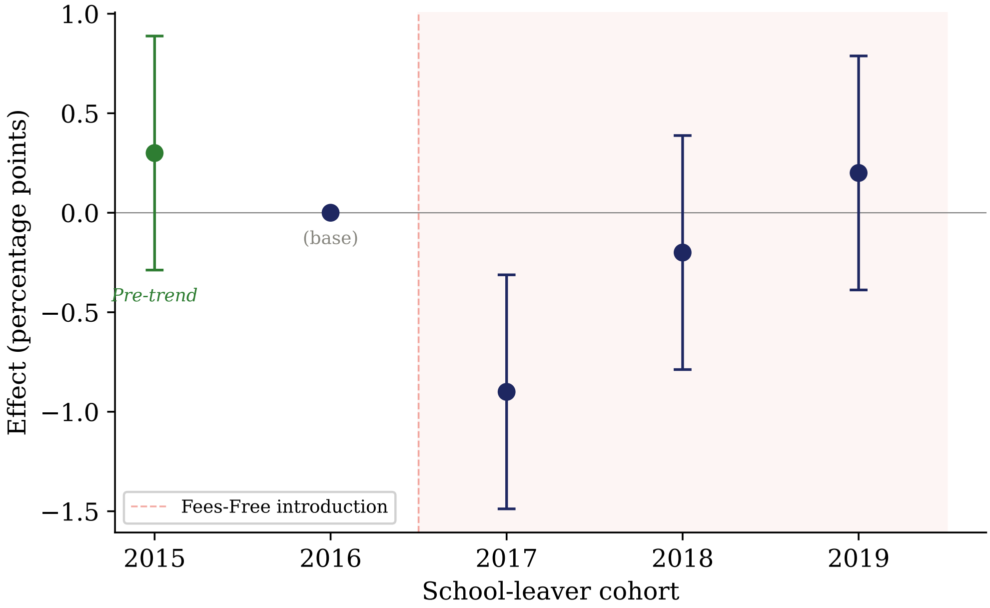
A Supplementary Tables and Figures

Table 14: Sensitivity Analysis: Estimates Excluding 2019 Cohort

Outcome	Model	Estimate (pp)	SE (pp)	p-value	N
Panel A: Completion					
	Event study: 2015	$+1.1$	0.3	0.001	109,621
	Event study: 2017	-0.7	0.3	0.032	109,621
	Event study: 2018	-0.6	0.3	0.094	109,621
	Without trend	-1.2	0.2	< 0.001	109,621
	With trend (FF)	-0.3	0.5	0.619	109,621
	With trend (trend/yr)	-0.5	0.2	0.050	109,621
	Placebo (2015–2016)	-1.7	0.3	< 0.001	55,729
Panel B: Retention					
	Event study: 2015	$+0.5$	0.3	0.110	109,621
	Event study: 2017	-0.6	0.3	0.057	109,621
	Event study: 2018	0.0	0.3	0.894	109,621
	Without trend	-0.6	0.2	0.017	109,621
	With trend (FF)	-0.6	0.5	0.229	109,621
	With trend (trend/yr)	0.0	0.2	0.946	109,621
	Placebo (2015–2016)	-0.5	0.3	0.134	55,729

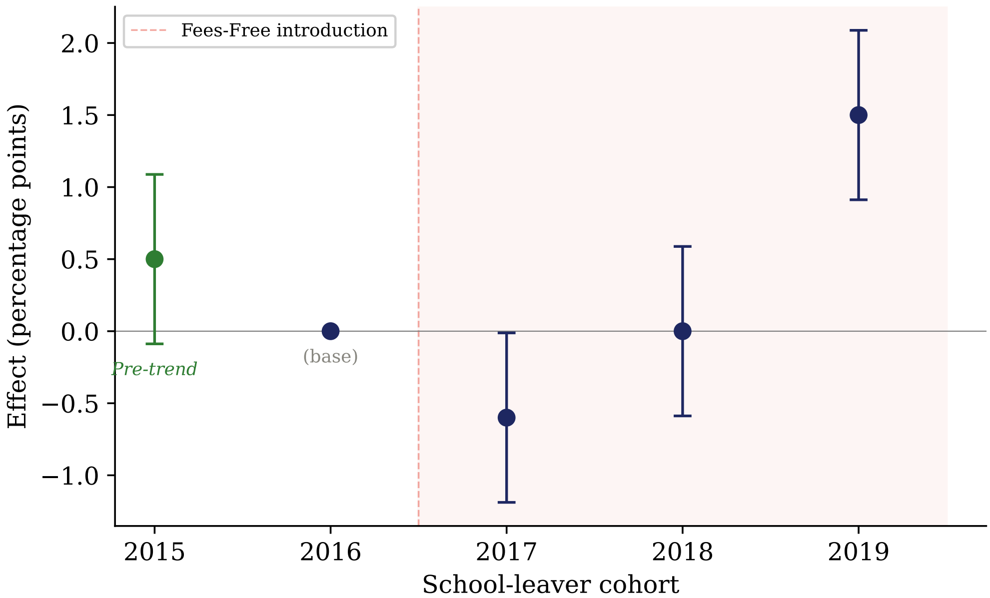
Note: pp = percentage points. All models estimated as linear probability models with HC1 robust standard errors and full controls. The 2019 cohort is excluded. Base = 2016 cohort.

Figure 8: Event Study: Bachelor's Enrolment (Base = 2016)



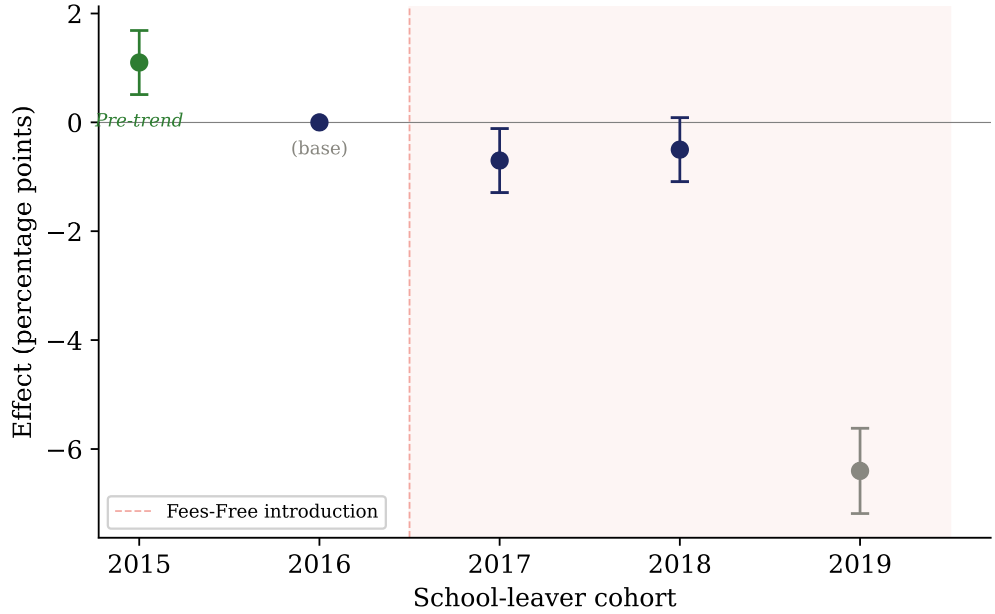
Note: Cohort-specific coefficients from a linear probability model with HC1 robust standard errors and full controls. Vertical bars show 95% confidence intervals. Sample restricted to enrolled students at NZQF Levels 3–7 ($N = 120{,}406$). 2016 = reference year.

Figure 9: Event Study: Year 2 Retention (Base = 2016)



Note: Cohort-specific coefficients from a linear probability model with HC1 robust standard errors and full controls. Vertical bars show 95% confidence intervals. Sample restricted to enrolled students ($N = 135{,}212$). 2016 = reference year.

Figure 10: Event Study: Completion within 6 Years (Base = 2016)



Note: Cohort-specific coefficients from a linear probability model with HC1 robust standard errors and full controls. Vertical bars show 95% confidence intervals. Grey marker = right-censored 2019 cohort. Sample restricted to enrolled students ($N = 135{,}212$). 2016 = reference year.

Table 15: Heterogeneous Effects: Year 2 Retention

Group	Base Gap (pp)	2015 (pp)	2017 (pp)	2018 (pp)	2019 (pp)	Pattern
Māori	−5.2	+0.4	+0.4	+0.2	+0.4	No effect
Pacific	−5.1	−1.0	+0.4	−2.1*	−0.4	−2018
Women	+2.5	−1.8	+0.9	+1.2	+2.2*	+2019
Low SES	−4.9	+0.2	−0.2	−2.3*	−1.6	−**2018**

Note: pp = percentage points. * denotes $p < 0.05$. SEs range from 0.6 to 1.2 pp. Sample restricted to enrolled students ($N = 135{,}212$). LPM with HC1 robust SEs and full controls.

Table 16: Heterogeneous Effects: Completion within 6 Years

Group	Base Gap (pp)	2015 (pp)	2017 (pp)	2018 (pp)	2019 (pp)	Pattern
Māori	+1.0	+1.0	−0.4	0.0	0.0	No effect
Pacific	+1.7	+1.7	+0.3	−0.6	+0.8	No effect
Women	+0.2	+0.2	+0.5	−0.4	−0.8	No effect
Low SES	+1.5	+1.5	−0.8	−1.4	−0.6	No effect

Note: pp = percentage points. * denotes $p < 0.05$. SEs range from 0.7 to 1.3 pp. No interactions reached statistical significance. Sample restricted to enrolled students ($N = 135{,}212$). LPM with HC1 robust SEs and full controls. 2019 coefficients subject to right-censoring.

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