MINISTRY OF SOCIAL DEVELOPMENT
TE MANATŪ WHAKAHIATO ORA

Estimating the impact of the introduction of the 2018 Families Package early-years changes

- Technical report

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Disclaimer

These results are not official statistics. They have been created for research purposes from the Integrated Data Infrastructure (IDI) which is carefully managed by Stats NZ. For more information about the IDI please visit https://www.stats.govt.nz/integrated-data/.

The results are based in part on tax data supplied by Inland Revenue to Stats NZ under the Tax Administration Act 1994 for statistical purposes. Any discussion of data limitations or weaknesses is in the context of using the IDI for statistical purposes, and is not related to the data's ability to support Inland Revenue's core operational requirements.

The views, opinions, findings, and recommendations expressed in this report are those of the authors. They do not necessarily reflect the views of MSD, Oranga Tamariki or other organisations involved in the study, or people involved in the peer review process. Any errors or omissions are our own.

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Executive summary

The 2018 Families Package increased social assistance payments as part of a strategy to reduce child poverty and improve child and youth wellbeing in Aotearoa New Zealand. Among the changes were a four-week extension to the maximum length of paid parental leave, and a new 'Best Start' tax credit of $60 per week for families with a child born on or after 1 July 2018.[1] Best Start is available regardless of family income in the first year of the child's life in weeks when paid parental leave is not received, and then available to low- and middle-income families on an income-tested basis until the child turns three.

As a result of these changes, cohorts with births in close proximity qualified for very different levels of financial support. This unique 'natural experiment' offers an opportunity to generate new evidence on the causal impacts of increasing financial assistance for children and families in the Aotearoa New Zealand context. Existing international studies suggest the changes could have a range of positive effects, but the size of these effects is uncertain.

The aim of this study is to document the changes to early-years payments and demonstrate a 'difference-in-differences' approach that can be used to estimate the causal effects of being in the first cohort eligible. We use linked administrative data, and estimate impacts on two outcomes:

- the incomes of parents with infants in the first six months post-birth
- months with no wages and salaries post-birth, as a proxy for time parents spent at home with their infants, in the first six and 12 months.

Findings

Even without the early-years changes, there were increases in income for families with children born in 2018.

- For mothers and first parents in the cohort with births just before the 1 July 2018 introduction of the early-years changes, average gross income was around $74 per week higher in the six months following the birth when compared with the equivalent cohort with births in 2017, a 17 percent increase.[2]
- A key driver of the increase was more income from Working for Families tax credits and benefit payments, consistent with the Families Package increases to Family Tax Credit and Accommodation Supplement, and the new Winter Energy Payment it introduced. There was also higher income from paid parental leave post-birth, reflecting higher levels of employment before the child's birth among those having children in 2018 compared to those having children in 2017. Income from Families Package and other benefit payments (inclusive of paid parental leave) increased by $49 a week on average. Employment income also increased.
- For fathers and second parents, the average income increase was like that of mothers/first parents in absolute terms, but much smaller in relative terms (a seven percent increase) and was driven almost entirely by increases in employment income, including self-employment.

[1] Families could also qualify for Best Start if their child was due on or after 1 July but born before that date.
[2] This is nominal gross income.

The main focus for this initial study is the additional income gains from the early-years changes, as these changes offer the best basis for estimating the causal impacts of increased financial assistance.

- Using difference-in-differences, we estimate that being in the first cohort eligible for the post-1 July 2018 early-years changes resulted in *additional* income gains for mothers and first parents that averaged $55 per week in the first six months post-birth. This is equivalent to a further 10 percent increase in mothers and first parents' income over the period, on average.
- These estimated additional income gains were broadly similar across Māori, Pacific, and non-Māori, non-Pacific mothers and first parents. While most additional income for non-Māori, non-Pacific mothers and first parents came from increases in paid parental leave income, Māori and Pacific mothers and first parents benefitted in equal measure from paid parental leave income and Best Start.
- For fathers and second parents in the first cohort eligible for the early-years changes, the difference made to total income was not statistically significant. This is because Best Start and paid parental leave tend to be received by the mother or first parent in the family.

Mothers and first parents eligible for paid parental leave are estimated to have spent more time off work in their child's first year as a result of being in the first cohort eligible for the early-years changes, as intended by the policy.

- The difference-in-differences estimation of impact is an 0.21 of a month increase in months with no wages or salaries, close to a week.
- Across ethnic groups, the effect on months with no wages and salaries was only statistically significant for non-Māori, non-Pacific mothers/first parents. This is consistent with their higher representation among those eligible the extended paid parental leave.
- Because non-Māori, non-Pacific mothers/first parents had less time with no wages and salaries post-birth on average before the early-years changes, the increase in months with no wages and salaries appears to have resulted in a slight reduction in ethnic differences in time at home with an infant after the birth.

Results remain robust when we apply a range of sensitivity and robustness tests, including tests for sensitivity to possible small birth shifting effects around the implementation date, and tests to check that there was a common pattern to differences for cohorts born either side of 1 July in the previous years we use as a basis for difference-in-differences estimation.

Areas for future research

A longer follow-up is needed before drawing conclusions about the success of the policy in achieving its aims. Low- and middle-income families are yet to receive the full amount of the additional income provided by the early-years changes.

Our next study will develop and extend the approach set out in this report, and estimate the causal effects of the policy on children's wellbeing in their first and second years. Future research can extend the follow-up, and examine impacts on children, and their parents and siblings, as they move through childhood and adolescence.

1. Introduction

A number of changes to social assistance payments in Aotearoa New Zealand were introduced in 2018. This 'Families Package' of reforms was part of the incoming Government's 100-day plan, reflecting its focus on reducing child poverty and ensuring children get the best start in life (New Zealand Government, 2017). The package also formed part of the Government's first programme of action to improve child and youth wellbeing (Department of Prime Minister and Cabinet, 2019).

Among the changes were increases to Family Tax Credits payable to low- and middle-income families with dependent children, increases to the maximum amounts of Accommodation Supplement payable, and introduction of a new Winter Energy Payment for those in receipt of either a main benefit, New Zealand Superannuation or a Veteran's Pension.

Changes to entitlements for families with children in their early-years included an extension to the length of paid parental leave to allow parents and caregivers to spend longer with their new-born child before returning to work, and the introduction of a new 'Best Start' tax credit for up to the first three years of a child's life, effective from 1 July 2018 (Arnesen & Wilson, 2019).[3]

Existing empirical studies suggest the changes could have a range of positive effects in addition to reducing child poverty. A growing international evidence base indicates that increasing families' financial resources improves cognitive, educational and other outcomes for children (Cooper & Stewart, 2020; Duncan, Morris, & Rodrigues, 2011; Ministry of Social Development, 2018). Recent studies suggest increased income from tax and benefit reforms is also protective against child neglect and entry into out-of-home care, and can reduce child welfare reports of concern (Oranga Tamariki Evidence Centre, 2019).

The two main theories used to explain the relationship between income and children's development and wellbeing are the 'investment' and 'family stress' models (Cooper & Stewart, 2013; 2020).

- In the investment model, higher income enables parents to purchase goods – better quality housing, food, health care, enriching activities – which contribute to improved developmental outcomes for children.
- In the family stress model, higher income reduces the likelihood of poor child outcomes through reduced parental stress. Less stress may mean reductions in parental conflict, or mental health issues, and allow the emotional resources needed for supportive and nurturing parenting.

There is consistent evidence that the impact of a given increase in income tends to be greater when family incomes are lower (Cooper & Stewart, 2020).

Paid parental leave has been associated with improvements in infant health, and with improvements in women's economic outcomes and attachment to the workforce over the

[3] Some elements of the package had been part of a 'Best Start Package' of proposals developed by the Labour Party in the lead up to the 2014 election (Boston & Chapple, 2014). The changes to Accommodation Supplement had already been scheduled for introduction as part of the previous government's Family Incomes Package announced in the 2017 Budget (Joyce, 2017).

longer term (so long as the length of parental leave does not exceed certain limits) (Heymann et al., 2017; Nandi et al., 2018).

While the Families Package changes were intended to boost the incomes of families, increase leave taken after a child's birth, and improve wellbeing, their actual impact is uncertain. A range of factors could influence the size of their effect. These include interactions with other financial assistance payments that might reduce the overall income gains, awareness and ease of take-up of entitlements, potential labour responses that could reduce earned income, the adequacy of income before and after the reform, and constraints (such as debt repayments) and behaviours shaping how additional financial assistance is used.

Building the evidence base is important. The success of policies in providing economic security and resources to support quality standards of living are recognised as central to the wellbeing of whānau, hapū and iwi (MSD, 2020), to supporting Pacific peoples, families and communities (MSD, 2019), and to reducing inequalities.

To date there have been few opportunities for robust empirical studies that increase our understanding of the scale of the positive causal effects of increased financial assistance in the Aotearoa New Zealand context. International studies remain limited, and more studies focussed on experiments or other sources of exogenous changes in income to identify effect sizes are needed (Cooper & Stewart, 2020).

The Families Package changes to early-years entitlements offer a unique natural experiment due to the way in which the changes were implemented. Comparing outcomes for those with births before and after the implementation date for Best Start and the extension of paid parental leave, after taking into account the usual pattern of differences between outcomes for families with children born each side of this date in the year, can provide the basis for an estimate of the difference increased financial support makes.

This report aims to provide a resource that can inform and encourage future studies focussed on this opportunity. We begin by describing in detail how families with infants and young children were affected by the Families Package changes (section 2). We next assess the degree to which cohorts with children born just before and just after the 1 July 2018 implementation date can be viewed as reliable comparison groups for the purposes of quasi-experimental impact estimation of impacts. To do this we examine whether the introduction of the early-years Families Package changes was accompanied by any shift in the timing or recording of births that might have altered the composition and comparability of the cohorts relative to previous years, and conclude that any effect of this nature was very small (section 3).

Section 4 then sets out one method that can be used to estimate the causal effect of increased financial assistance as children and families in the first cohort to receive the Families Package additional early-years financial support move through the childhood years, and robustness tests. The final sections present results (section 5) and discuss the findings (section 6), outline strengths and limitations of the analysis (section 7), and conclude with future research opportunities (section 8).

2. How the Families Package changed early-years entitlements

The new Best Start tax credit[4]

The Families Package introduced a new Working for Families tax credit, Best Start. This is a payment of up to $60 per week (up to $3,120 per year) per child to help families with costs in a child's early-years. Best Start was made available to families with a child born, or due to be born, on or after 1 July 2018.

Best Start is now available to all families in the first year of the baby's life during weeks the family is not in receipt of paid parental leave, if they meet residency requirements.[5] In the child's second and third years, support continues for low- and middle-income families. In these years, Best Start is abated at 21 percent for family income above $79,000, with no entitlement when family income is above $93,858 (Table 11).

Table 1: Best Start tax credit for a child's second and third years.

Family income (gross)	Best Start tax credit
Less than $79,000	$3,120 ($60 per week)
$80,000	$2,910 ($56 per week)
$85,000	$1,860 ($35.80 per week)
$90,000	$810 ($15.60 per week)
$93,858 and higher	$0

Like other Working for Families tax credits, Best Start can be received as a weekly or fortnightly regular payment, or as an annual lump sum after the end of the tax year, is non-taxable, and is paid to the primary caregivers of children. If children are in a shared-care arrangement, both the main Working for Families tax credit – Family Tax Credit – and Best Start can be paid proportionately to both parents (with a minimum amount of care required of one-third of the child's time).

The 'assessment period' for Working for Families payments is annual i.e. people's incomes are assessed on an annual basis to determine their eligibility for, and the payment rates of, Best Start and other Working for Families payments. People receiving main benefits such as Jobseeker Support or Sole Parent Support can choose to receive Best Start through the Ministry of Social Development (along with their benefits and any other payments) or from Inland Revenue. Working for Families tax credits paid while a person is on benefit are ringfenced, such that income earned outside the period in receipt of benefit

[4] Readers not familiar with the Aotearoa New Zealand income support system may find it useful to read this section in conjunction with an overview provided by Welfare Expert Advisory Group (2019).
[5] Consistent with other Working for Families payments, the Best Start tax credit is available to families who meet a residency requirement either through a parent or the child. The requirement can be met by the child if the child is ordinarily resident in New Zealand and is present in New Zealand for the period of entitlement. The requirement can be met by the parent if the parent is ordinarily resident in New Zealand and has been in New Zealand for 12 months continuously at any time. When the Families Package was introduced, parents and children on a temporary visa (such as a visitor, work, or student visa) were specifically excluded from being eligible for Working for Families, regardless of the time they had spent in New Zealand. As part of the response to COVID-19, this was changed from 1 April 2020 so that parents on a temporary visa and getting an Emergency Benefit with dependent children included could receive Working for Families payments, including Best Start, with their benefit.

cannot affect a person's entitlement while on benefit. This protects people against ending up in debt after moving into employment. After a child turns one, if people wish to receive their Best Start payments fortnightly through Inland Revenue, they need to provide estimates of their annual incomes as the basis for these payments. These estimates are re-assessed at the end of the tax year. Applications can be made through the Government's SmartStart online service for expecting and new parents.

Extension of paid parental leave

As part of the Families Package, the maximum number of weeks of paid parental leave increased from 18 to 22, effective from 1 July 2018, and from 22 weeks to 26, effective from 1 July 2020. Paid parental leave is available to parents and caregivers who take time off work to care for their baby or for a child who has come into their care, and is usually received by mothers.[6] It is available to parents and caregivers of children under six years old who are the primary caregivers of those children.

To qualify for paid parental leave, the parent or primary caregiver making the claim needs to have worked an average of 10 hours a week in at least half of the year before the child was born or came into their care. Entitlement is independent of the other parent's employment and earnings. The other parent may qualify for one or two weeks of unpaid 'partner's leave' following the birth of a child, but not paid parental leave.[7]

While on paid parental leave, a caregiver may use paid 'keeping in touch days' to stay connected with their employer, so long as these total 64 hours or less during the parental leave payment period and the work is not within the first 28 days after the child was born. Once either of these conditions is not met, the caregiver is considered back at work, and ineligible for further paid parental leave payments. Payments received after a person is considered back at work are treated as an overpayment. These provisions are adjusted for those with a pre-term baby.[8]

The maximum level of payment is indexed in line with wage growth on 1 July each year. On 1 July 2018, indexation increased the maximum weekly rate for eligible employees and self-employed parents from $538.55 to $563.83 gross per week. Employees are entitled to either their gross weekly rate of pay or the maximum weekly payment, whichever is lower. Paid parental leave is paid by Inland Revenue.

Abolition of Parental Tax Credit

With the introduction of Best Start, a former Parental Tax Credit was abolished. Parental Tax Credit had provided eligible families with an income tested entitlement of up to $220 per week per child for the first 10 weeks of a child's life. This payment had not been available to recipients of a main benefit or the student allowance, and could not be received by a family where a parent received paid parental leave. Recipients were typically families where one partner worked full-time and the other had no earnings, or had hours of work and duration of employment that were not sufficient for them to qualify for paid

[6] Transfers of paid parental leave to a partner/spouse occurred in less than one percent of cases pre-reform. See https://www.mbie.govt.nz/assets/22751a59e2/extending-paid-parental-leave-26-weeks-cabinet-paper.pdf.
[7] See https://www.employment.govt.nz/leave-and-holidays/parental-leave/types-of-parental-leave/.
[8] See https://www.employment.govt.nz/#gref.

parental leave. Prior to its abolition, for the year to March 2018, there were just over 12,000 parental tax credit recipients.[9]

Overall change in early-years entitlements for model families

The net gain from the changes to early-years entitlements, and how these fell across time, varied considerably. This is because Best Start replaced the more narrowly focussed Parental Tax Credit, was only able to be paid after the newly extended period of paid parental leave ended, and was income-tested in the child's second and third years.

Table 2 and Figure 1 demonstrate how the changes affected model families in different situations.

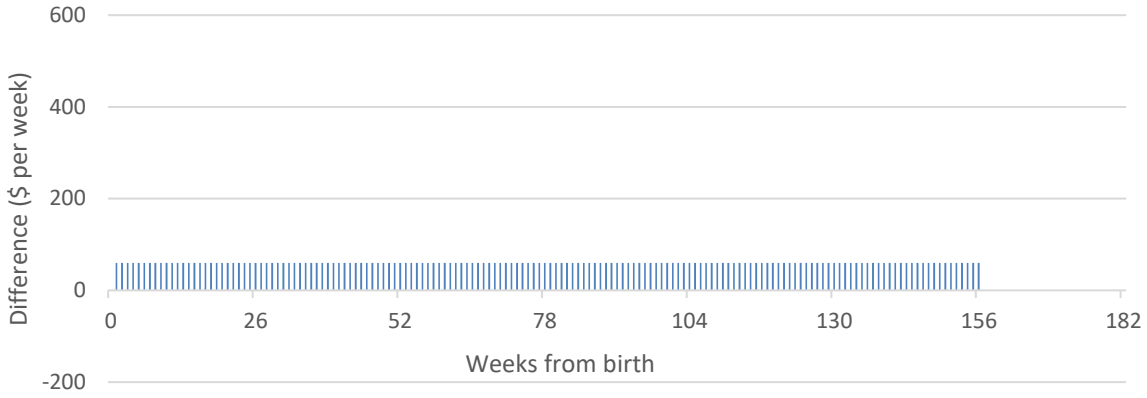
Table 2: Early-years entitlements for model families before and after the Families Package

Family 1: Parent/s supported by benefit prior to the birth; no entitlement to paid parental leave; family income below the abatement zone for Best Start from when the child turns one; no Temporary Additional Support offset							
	Before			After			Difference in total
Time from birth:	Best Start	paid parental leave	Parental Tax Credit	Best Start	paid parental leave	Parental Tax Credit	
First 6 months				1,560			1,560
First year				3,120			3,120
First three years				9,360			9,360
Family 2: Parent/s in employment prior to the birth; not receiving benefit; entitled to $500 per week paid parental leave; family income above the abatement zone for Best Start from when the child turns one							
	Before			After			Difference in total
Time from birth:	Best Start	paid parental leave	Parental Tax Credit	Best Start	paid parental leave	Parental Tax Credit	
First 6 months		9,000		240	11,000		2,240
First year		9,000		1,800	11,000		3,800
First three years		9,000		1,800	11,000		3,800
Family 3: One parent in employment, one parent not in paid work prior to the birth; not receiving benefit; no entitlement to paid parental leave; family income at the mid-point of the abatement zone for Best Start from when the child turns one							
	Before			After			Difference in total
Time from birth:	Best Start	paid parental leave	Parental Tax Credit	Best Start	paid parental leave	Parental Tax Credit	
First 6 months			2,200	1,560			- 640
First year			2,200	3,120			920
First three years			2,200	6,240			4,040

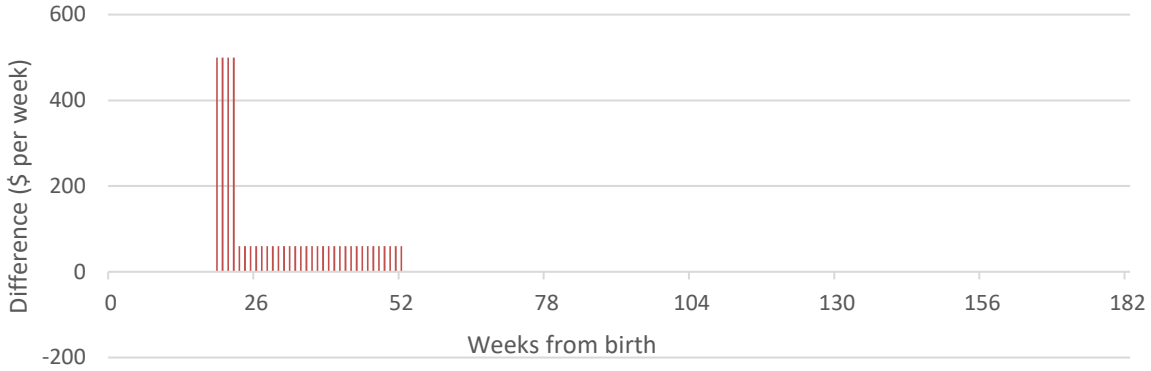
[9] For trends in receipt, see https://www.msd.govt.nz/documents/about-msd-and-our-work/publications-resources/evaluation/family-packages/families-package-monitoring-report-2019-main-report-nov.pdf. For abatement details, see Welfare Expert Advisory Group (2019).

Figure 1: Difference in total weekly early-years entitlements for model families

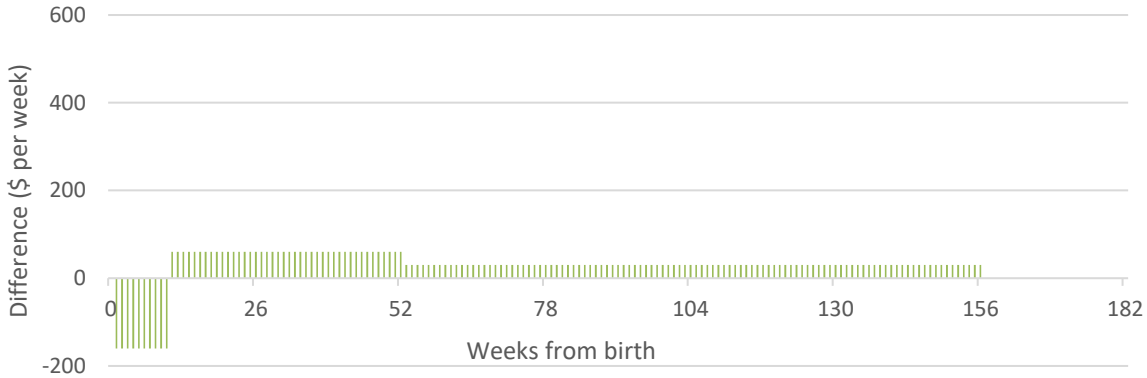
Family 1: Parent/s supported by benefit prior to the birth; no entitlement to paid parental leave; family income below the abatement zone for Best Start from when the child turns one; no Temporary Additional Support offset



Family 2: Parent/s in employment prior to the birth; not receiving benefit; entitled to $500 per week paid parental leave; family income above the abatement zone for Best Start from when the child turns one



Family 3: One parent in employment, one parent not in paid work prior to the birth; not receiving benefit; no entitlement to paid parental leave; family income at the mid-point of the abatement zone for Best Start from when the child turns one



Gains overall were greatest for those families supported by benefit or on low incomes who qualified for the unabated amount of Best Start for a full three years, and for whom there was not an offsetting reduction in Temporary Additional Support (see below) (e.g. Family 1). Gains in the first six months and first year were highest for families receiving at or close to the maximum paid parental leave amount (e.g. Family 2). Gains were attenuated for families who would formerly have qualified for Parental Tax Credit. In the first six months of the child's life, these families received $640 less in early-years financial assistance after the Families Package, and they gained just $920 in the first year of the child's life (e.g. Family 3).

Other Families Package changes

In addition to the changes in early-years entitlements, families were expected to experience different levels of income gain from other parts of the Families Package depending on their circumstances (Box 1). For example, from 1 July 2018 a couple with two children aged under thirteen, with a family income of $55,000, would gain $49 per week in Family Tax Credit payments. If they lived in Central Auckland, from 1 April 2018 they could also get up to $31 a week more Accommodation Supplement, depending on how much rent they paid. It was estimated that by 2020/21, 384,000 (62 percent) of the country's 620,000 families would gain from the Package overall, and the average additional assistance per week received by families who gained (inclusive of the increased early-years entitlements) would amount to $75 per week (New Zealand Government, 2017).

Box 1: Other Families Package changes*

Effective from 1 April 2018, two changes were made to the Accommodation Supplement. More areas were zoned as qualifying the highest possible maximum rate, and rates for Accommodation Supplement were raised, the value of the increase depending on the family's circumstances. Accommodation Benefit for students was also increased.

From 1 July 2018, Family Tax Credit payment rates were increased and simplified. For example, for a first or only new-born, the weekly rate increased by $20.31. For a new-born who was a second or subsequent child, the weekly rate increased by $26.81. The abatement threshold at which Working for Families payments started to reduce (abate) was increased from $36,350 to $42,700, while the abatement rate increased from 22.5 percent to 25 percent.

Rates for Orphan's Benefit and Unsupported Child's Benefit and Foster Care Allowance were increased on 1 July 2018. The increase of $20.31 per week from 1 July 2018 was equivalent to the increase to Family Tax Credit for a first child aged under 16. On top of these rate increases but outside the Families Package, a new Clothing Allowance introduced from 1 July 2018 further increased the support available to carers.

A new Winter Energy Payment was introduced for those receiving a main benefit, New Zealand Superannuation or Veteran's Pension. This is a non-income tested cash payment (i.e. not a voucher) made to help with household heating costs during the winter months. In 2018, payments were made from 1 July until 29 September. In 2019 and subsequent years, payments were made from 1 May until 1 October. People with dependent children received $31.82 a week.

* For more details of the changes see New Zealand Government (2017), and Arnesen & Wilson (2019). For a full description of the Aotearoa New Zealand income support system after the Families Package see Welfare Expert Advisory Group (2019).

Exposure in the post-natal period to these other Families Package gains for the cohorts that are the focus of this study depended on the timing of their child's birth (Table 3, panel A). For example, families with children born on 1 July 2018 received all the Families Package increases they qualified for from the date of their child's birth. Families with children born on 1 April 2018 received the Accommodation Supplement increases from the date of their child's birth but would not receive the Family Tax Credit increases until the child was aged three months.

The period in which the Winter Energy Payment could be paid was limited to 1 July to 29 September in 2018 and 1 May until 1 October in 2019 and following years. This means that, for the three-month birth cohorts born either side of the 1 July 2018 implementation that are of interest in this study, income from this particular source in a six month follow-up from birth was higher, on average, for the pre-1 July cohort than for the post-1 July cohort.

Table 3: Families Package components and eligibility for cohorts with births three months pre- and post-1 July 2018

Families Package component	Panel A: Eligibility in the six months post-birth by timing of birth	
	Pre-1 July 2018	Post-1 July 2018
1 April 2018 increase to Accommodation Supplement	Yes	Yes
1 July 2018 increase in Family Tax Credit and payments for carers	Yes, but post-natal exposure limited if born closer to 1 April	Yes
1 July to 29 September 2018 new Winter Energy Payment	Yes, but post-natal exposure limited if born closer to 1 April	Yes, but post-natal exposure limited if born closer to 29 September end date
1 July 2018 introduction of Best Start	No (unless due after 1 July)	Yes
1 July 2018 extension of paid parental leave	No	Yes
Parental tax credit (abolished 1 July 2018)	Yes	No
Families Package component	**Panel B: Eligibility in the three months prior to birth by timing of birth**	
	Pre-1 July 2018	**Post-1 July 2018**
1 April 2018 increase to Accommodation Supplement	Yes, but ante-natal exposure limited if born closer to 1 April	Yes
1 July 2018 increase in Family Tax Credit and payments for carers	No	Yes if older children in family, but ante-natal exposure limited if born closer to 1 July
1 July to 29 September 2018 new Winter Energy Payment	No	Yes, but ante-natal exposure limited if born closer to 1 July

Exposure in the ante-natal period to other Families Package income gains also varied, depending on the timing of the child's birth and the presence of older children in the family (Table 3, Panel B). These differences and their effects are not examined in the present study, but will be an area of focus for future research.

Temporary Additional Support

The income gain from the Families Package was also influenced by whether the family received, or would have received, Temporary Additional Support. This is a payment of last resort to help people with regular essential living costs that cannot be met from their income or assets, and is available for people receiving main benefits and for non-beneficiaries on very low incomes. Entitlement is calculated as the difference between people's essential costs and their incomes (a 'deficiency'), up to an 'upper limit' of 30% of the net rate of their main benefits (or of Jobseeker Support if they are non-beneficiaries). Any increase in income is automatically off-set by a decrease in Temporary Additional Support entitlement.

As a result, the Families Package gains in income from Best Start, increased Family Tax Credit payments and increased Accommodation Supplement could be offset by reduced Temporary Additional Support entitlements. Winter Energy Payment, however, was paid on top of Temporary Additional Support resulting in no offset. Introduction of the Families Package reduced the number of Temporary Additional Support recipients, and reduced the proportion receiving the upper limit (Arnesen & Wilson, 2019).

In addition, in June 2018, shortly before the Families Package early-years changes were introduced, a monthly mail-out to benefit clients who were estimated to be eligible for Temporary Additional Support but not receiving it was stopped. This was because changes in the structure of benefit payments meant new computer code was needed to identify eligible individuals. It is possible that fewer clients took up Temporary Additional Support post-1 July 2018 due to this change. A new process was trialled in November 2018 and has been subsequently implemented, but the new process is unlikely to have benefited the families in this study.

3. Testing for birth shifting effects

In other countries where payments like Best Start have been introduced or disestablished, birth shifting has been found to occur around the implementation date for the change (Momsen, 2021b). The Australian Baby Bonus introduced in 2004 was estimated to result in six percent of births shifting from one month to the next in order to qualify for the payment (Gans & Leigh, 2009). Understanding whether such effects were associated with the Families Package, and if so whether they were on a scale that would alter the comparability of cohorts born before and after the changes, is an important initial step when seeking to use the natural experiment created by the implementation to estimate the impacts of the changes.

As noted, all babies due to be born on or after 1 July 2018 were eligible for the Best Start payment, regardless of when the birth occurred. However, babies due before 1 July would only become eligible for the payment if the birth occurred on or after 1 July. This meant that prospective parents with a due date in late June 2018 would have had a financial gain if the birth were delayed. In addition, parents with children born on or after 1 July 2018 could qualify for the extended period of paid parental leave, which also created a financial gain from a delayed birth.

If large-scale birth shifting, shifting of due dates, or shifts in timing of birth registrations occurred, and if the shifted cases had different characteristics from non-shifted cases, then (depending on the data source used) this could impact on the robustness of any impact evaluation findings based on a comparison of the outcomes of families with children born shortly before and after the programme was implemented.

In Appendix 1 we provide a detailed description of the different methods we used to explore possible shifting effects as part of the present study. We find evidence of a very small shift in births occurring, but no evidence of large-scale birth shifting of a magnitude that could affect the comparability of cohorts born immediately before and after the implementation of the changes. Box 2 below summarises the methods and main results.

In addition, the Families Package was signalled on 8 November 2017 and full details were announced on 14 December 2017.[10] The births of children conceived after these dates could be the result of parents making conception decisions cognisant of, and possibly influenced by, anticipation of the change in early-years entitlements. We did not test whether there were shifts in births in the months after the implementation date consistent with a response to the announcement of the package.[11] Instead, our approach was to limit our analysis to the cohort born in July, August or September 2018. For the parents of this cohort, there was little or no time to respond to the announcement of the Families Package.[12] We also tested the sensitivity of results to considering only births in July and

[10] A first general announcement that a "family benefit in the form of the Best Start package" would take effect on July 1, 2018 was made as part of the Speech from the Throne in Parliament on 8 November 2017. A more detailed announcement about the Families Package was made on 14 December 2017, alongside the Budget Policy Statement and the Half Year Economic and Fiscal Update. This provided exact details of eligibility, duration and the level of the payment.
[11] Unlike the 1 July shifting effect, there was no clear date around which to test conception effects. Any effect would be unlikely to occur instantly and would be likely to be spread over a wide period. Without a localised effect to test for, it would be impossible to separate conception effects from broader trends.
[12] Babies conceived on or after 8 November would have a due date of approximately on or after 1 August 2018, while those conceived on or after 14 December would have a due date of approximately on or after 6 September 2018.

August. Conceptions for these children would have in most cases occurred before the 14 December 2017 announcement of full details of the Families Package.

Box 2: Testing for shifting effects around the implementation date

In theory, several possible shifting effects could occur around the implementation date:

- a birth shifting response could potentially occur through delaying the use of interventions such as inductions or elective caesarean sections, or through mis-recording of births as happening on dates later than was actually the case
- pre-implementation due dates could be altered and recorded as post-implementation in order to provide access to Best Start
- birth shifting could appear to have occurred in birth registration data as a result of a reduction in the (small) likelihood of a birth remaining unregistered for a long period of time – Best Start increased the incentive to register a birth soon after it occurred because registration was required in order to receive the payment.

We applied tests seeking to assess the extent of these possible effects.

Time series analysis of aggregate data on birth dates from birth registrations

Our first method of estimating whether birth shifting effects occurred used aggregated time series data provided by Statistics NZ on births registered in Aotearoa New Zealand by day of birth. The approach was to fit a statistical model to the time series and then test whether there is a significant divergence from this model on or around 1 July 2018.

Table B1 presents results. We found no evidence of significantly fewer births being observed than we would otherwise have expected in the weeks prior to 1 July 2018. In the first three days of July 2018, however, there were estimated to be around eighty more births than would have otherwise been expected, an increase of almost a fifth. The estimate had a confidence interval from 38 to 125, suggesting that some births were likely to have shifted, and that the number shifted is unlikely to be greater than 125, approximately 2.5 percent of all June births.

Table B1: Estimated birth shifting effects in different windows pre- and post-implementation of the Families Package using a two-stage TBATs/RegArima model* based on daily registered birth data from 2013 to 2018

	Days covered by window			
Window	1-3 days	4-7 days	8-14 days	15-28 days
Pre-implementation	2	-13	-3	-18
	(-42,45)	(-64,38)	(-74,67)	(-124,88)
Post-implementation	81	-7	22	-14
	(38,125)	(-58,44)	(-48,93)	(-120,92)

Note: Numbers in brackets give the lower and upper confidence intervals on the birth shifting estimate, respectively.
* See Appendix 1 for details.

The lack of a significant negative effect on births recorded as occurring in June 2018 (the pre-implementation period) was unexpected, in that birth shifting is expected to involve a reduction in the number of births in one period, accompanied by an equivalent increase in another period. A similar analysis undertaken following the introduction of the 'Baby Bonus' in Australia showed not only much larger effects, but also largely symmetrical effects, whereby a reduction of almost 1,200 births in the weeks prior to the introduction of the payment was followed by an increase of similar magnitude in the subsequent weeks (Gans & Leigh, 2009).

One possibility for this unusual result is that births shifted into the first few days of July would have otherwise occurred over a broader period in June, dampening our ability to detect the effect. This could have been coupled with an increase in

registration of births not only in July, but also at the end of June, for births which had due dates in early July. The data we have does not allow us to draw definitive conclusions about the reason for this unusual finding, however, it does suggest any large-scale birth shifting in New Zealand is unlikely.

Regression analysis of births relative to due dates from maternity data

The second approach to estimating the magnitude of birth shifting used individual-level National Maternity Collection data in the IDI. Given that the financial incentive to shift births was only relevant for parents with due dates in June, we were able to estimate whether the introduction of the Families Package was associated with a change in the probability that a child due in one month would actually be born after that month, and to test whether June 2018 differed from the pattern in previous years. Table B2 shows our birth shifting estimates from this analysis using progressively widening windows of June due dates.

Table B2: Estimated birth shifting effects from regression models of maternity data*, 2013 to 2018 in windows pre- and post-implementation of the Families Package

Window (maximum number of days before 1 July that births were due):	3 days	7 days	14 days	28 days
Total births with due dates in June 2018	444	1008	2046	4122
Total 2018 June-due births born after June	150	282	378	408
Expected number of June-due births born after June without the Families Package	127	226	296	331
Estimated births shifted	23	56	82	77
Confidence interval	(4,49)	(31,94)	(46,122)	(38,118)

Note: Numbers in brackets give the lower and upper confidence intervals on the birth shifting estimate, respectively.
* See Appendix 1 for details.

As with the time series analysis of registered births, the results of this second analysis suggested that a small number of births were shifted. In this case, around 82 births that were due in the last two weeks of June were estimated to have been shifted into July or later (with a confidence interval ranging from 46 to 122 births). Around a quarter of those shifted were due in the last 3 days of the month (an estimated 23 births), and over two thirds were due in the last week of June (an estimated 56 births). This provides further evidence suggesting there was not large-scale birth shifting.

This second analysis uses a data source that is unaffected by any possible effects on the timing of parents registering births, as data is sourced from hospitals and lead maternity carers. This suggests that the small birth shifting effect estimated using our first method was largely the result of changes in the actual dates of birth, rather than changes in the timing of birth registrations.

4. Difference-in-differences estimation

Data sources

The analysis presented in the remainder of this report is based entirely on data held in the IDI. This is a collection of de-identified linked administrative and survey data made available for approved research (Statistics NZ, 2017; Milne et al., 2019). Administrative data in the collection have national coverage of those who engage with services. Data sets used included Department of Internal Affairs data on birth registrations, Ministry of Health maternity collection data, MSD data on benefit payments and Working for Families tax credits paid via MSD, and Inland Revenue data on wages and salaries, paid parental leave, Working for Families tax credits and income from self-employment.

Study population

The study population is comprised of families where the birth of a child occurred three months either side of 1 July over the period 2015-2018. Our main analysis focuses on families identified in birth registration data (child, mother/first parent, father/second parent). Given that parents recorded on birth registrations may not always be the people who care for a child, we also test the sensitivity of results to examining mothers and children as recorded in maternity data, and mothers/female caregivers and children as recorded in benefit data (for the sub-population supported by benefit after the birth).[13]

Outcome variable definitions

We estimate impacts on two broad administratively recorded outcomes.

- Total income in the six months following the birth is estimated using data from Inland Revenue and MSD, on a gross and net basis. Gross income is made up of taxable earnings recorded in IR3, IR4 and IR20 tax returns, employer monthly schedules, and personal tax summaries, as well as non-taxable payments, including Working for Families tax credits made by MSD. Working for Families tax credit income which is not paid by MSD is observed in annual family returns data from Inland Revenue. While these entitlements are only readily available on a tax-year basis, we can disaggregate entitlements across the year. In many cases families only receive their entitlement after the end of the tax year, however, we treat tax credits as income in the period in which the entitlement falls. Appendix 2 outlines our approach to Working for Families tax credit income in more detail.
- Time spent at home with a new-born in the six and 12 months following the birth is approximated using data on months with no wages and salaries in the IDI. These data are available from the Inland Revenue Employer Monthly Schedule (EMS) tables, which include all PAYE tax-withheld earnings payments on a calendar monthly basis. EMS data do not allow investigation of which days in the month a person was employed, or hours of work. Given these limitations, our outcome measure was a count of the number of months in the six and 12 months following the birth month in which the parent had no receipt of wages and salaries recorded in EMS.

[13] While family relationship data is also collected by Inland Revenue in administering the tax credit system, policy changes over time mean that it is not possible to identify families on a consistent basis over time – relationships are more likely to be able to be identified after the Families Package reform because more families would have registered for Working for Families in order to receive Best Start. For this reason, we did not use Inland Revenue family relationship data.

There are lags in the inclusion of income data in the IDI, so 12-month outcomes for income, extending beyond the tax year ending March 2019, are not yet available. Data on Working for Families tax credits has a particularly long lag-time in recording,[14] and 2020 tax year data is unlikely to be substantively complete until the latter half of 2021.

As well as total income, we also report income in six broad categories: employment income (including wage and salary income and self-employment), Best Start tax credit income, Parental Tax Credit income, other Working for Families tax credit income (including, among other payments, the increased Family Tax Credit), paid parental leave, the new Winter Energy Payment, other benefit income (including supplementary payments such as Temporary Additional Support and the Accommodation Supplement), and other income.

With the introduction of the Families Package, we would expect incomes for families with infants to increase due to the increased financial assistance, and for this increase to be potentially partially offset by a decrease in earned income due to parents making the decision to spend more time with their babies, or potentially modified by a change in take up of entitlements. As a result, observed changes in income will largely reflect the quasi-experimental 'treatment' we are interested in (increased income that occurred as a result of changes in financial assistance), but also reflect impacts on behaviour (to the extent that the changes in entitlement affected employment decisions or take up for example).

Other variable definitions

Sociodemographic variables and measures of employment and benefit history are derived from a range of sources.

Age of the parent at birth and gender comes from the data collection used to identify the parent-child relationship. For example, for the main analysis where we examine parent-child relationship in birth registration data, this data source is used to obtain age and gender. The ethnic groups of the new-born and parents are sourced from Stats NZ estimates, which are derived from multiple collections in the IDI using a set of specific rules. Ethnicity variables in this set of estimates are an 'ever-indicator' that shows all ethnic groups a person has ever been recorded as identifying with across data collections over time.[15] 'Total response' ethnic groups are derived from these data, where a person was counted in all the ethnic groups they were recorded as identifying with (Statistics NZ, 2004).

Benefit receipt is derived from the BDD combining information on spells of benefit receipt as the primary recipient, and as a partner. Measures of the percentage of days supported by benefit in the six months before the birth, the year prior to that, and the three years prior to that, are derived from the BDD, based on a count of days combining information on spells of benefit receipt as the primary benefit recipients, and as a partner.[16]

[14] For those who receive these payments at the year-end, there is also a lag in receipt relative to the time in which they were entitled to the payments.
[15] See http://archive.stats.govt.nz/browse_for_stats/snapshots-of-nz/integrated-data-infrastructure/idi-data.aspx. In practice, most data for the ethnicity of newborns in our study was sourced from DIA or health data, as most children had no other interaction with agencies at the time of our study.
[16] Parents who were aged under 18 at the time of the birth were identified separately, as most would not have been eligible for benefit before that age. Adjustments are made such that percentages of time on benefit in specified periods relate only to the time after the person turned 18.

Employment status prior to the birth is inferred using Inland Revenue data on wage and salary earnings and self-employment income. Self-employment income is available on an annual basis from the IDI and is derived from people's end-of-year IR3 tax returns or from EMS schedular payments data. Those with self-employment income identified through people's IR3 returns are treated as if they were working for the whole tax year for the purpose of this measure.

The count of months in wage and salary employment in the six months before the birth, the year before that, and the three years before that, are derived from the same sources, although self-employment income is not included in our measure of months worked. If a person had any wage or salary earnings in a month, that month is counted as a month in employment.

Income prior to the birth is derived using the approach described above, and considering periods covering the six months leading up to the birth, in the year prior to that, and in the three years prior to that (i.e. 0-6 months prior, 6 months–1.5 years prior, and 1.5-4.5 years prior to the birth). Income in the six months prior to the birth is restricted to earned income only, as the implementation of other Families Package changes in April and July 2018 (the Accommodation Supplement, Family Tax Credit and Winter Energy Payment changes) will have affected total pre-birth income for the parents in the 2018 cohorts. Income from all sources is included in the other measures.

Location of residence is derived from Stats NZ data in turn derived from multiple sources,[17] or from DIA birth registrations, if not available in the Stats NZ derived data. Location data is then used to define region of residence and New Zealand Deprivation Index (NZDep2013) quintiles (Atkinson, Salmond, & Crampton, 2014).

Number of siblings is derived by identifying any earlier children born to the mother identified in the maternity data, or to either parent identified in the birth certificate.

Sub-group definitions

Sub-group analysis is conducted in order to investigate impacts for different ethnic groups, and for sub-groups potentially affected in different ways by the change in early-years entitlements (illustrated by Table 2 and Figure 1 above).

High-level ethnic groups are defined on a total response basis, as described above, and categorised as being Māori, Pacific, or non-Māori, non-Pacific. The first two groups overlap, as people can be identified as being of both Māori and Pacific ethnicity, but the third group only includes those who are not identified as being either Māori or Pacific.

Three sub-groups of families potentially affected in different ways are approximated as follows:

- mothers/first parents supported by benefit in the month before the child was born
- mothers/first parents eligible for paid parental leave (estimated based on pre-birth earnings) and not supported by benefit in the month before the birth

[17] Address information is sourced from the Ministry of Health's PHO and NHI registers, the Ministry of Social Development, the Ministry of Education, ACC, and Inland Revenue, with higher quality sources given priority. Once an individual has an address recorded from a high-quality source, the address is only replaced by an update from the same source or another high-quality source (see https://vhin.co.nz/guides/geographic-information-in-idi/). We use the most recent address identified after the birth.

- mothers/first parents not eligible for paid parental leave (estimated based on pre-birth earnings) and not supported by benefit in the month before the birth.

Whether the mother/first parent was supported by benefit in the month before the birth is based on BDD data on receipt of main benefits. Assessing whether the mother/first parent was eligible for paid parental leave required us to approximate the eligibility rules. To qualify for paid parental leave, a parent or primary caregiver needs to have worked an average of 10 hours a week in at least half of the year before the child was born. We assess whether the count of months in which the mother/first parent appeared to have had weekly wage and salary earnings above ten times the minimum hourly wage was six or greater in the 12 months prior to the month of the child's birth. We consider self-employed parents to be eligible for paid parental leave if they had earnings in the most recently completed tax year greater than 10 hours per week at the minimum wage worked over a six-month period.[18]

We hypothesised that the scale of estimated effects on non-income outcomes will vary across the three analysis sub-groups influenced by the level of additional income gain each received, and that the scale of estimated effects will vary across ethnic groups in line with their distribution across the three sub-groups.

Difference-in-differences estimation

As noted, Best Start was made available to all families with children born or due to be born after 1 July 2018 in weeks when they did not receive paid parental leave. Payments are available for all families who meet residence requirements up until a child's first birthday and on an income-tested basis in the following two years. The extension of paid parental leave applied from 1 July 2018, and abolition of the Parental Tax Credit also occurred from this time.

There was no random assignment to the package of changes, and it is available nation-wide. As such, there is no obvious contemporaneous comparison group against which outcomes could be robustly compared to estimate the impacts of the package. Similarly, there is no eligibility threshold (apart from – potentially – due date and date of birth) around which regression discontinuity estimates could be constructed.

We can compare outcomes for families where the child was born before July 2018 with outcomes for those where the child was born after July 2018 (a pre-post comparison). The problem with this approach is that outcomes where children are born post-1 July could be systematically different from outcomes for children born earlier in the year. This is particularly likely to be true as children age, since there are established differences in outcomes for children born in different times of the year in adult health, behaviour and education (Boland et al., 2018; Ali & Menclova, 2018). One key reason for educational differences in New Zealand is that children born earlier in the year (typically before July) are generally placed in a higher school year than those born later in the year (Ali, 2019). For other outcomes earlier in life, and for post-birth parental employment and earnings, there are potentially season of birth effects too. In a typical pre-post comparison, some of

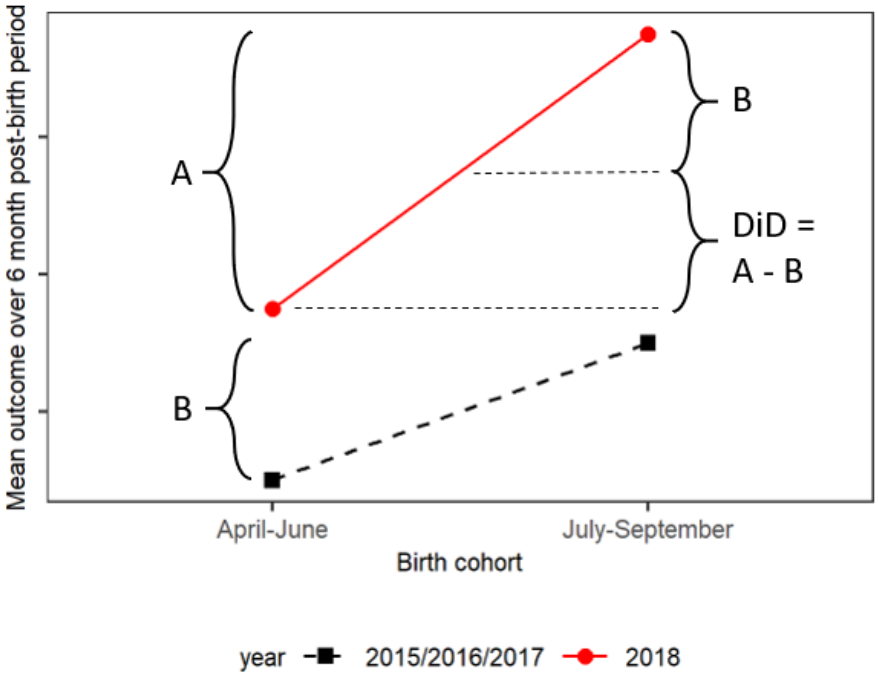
[18] These only approximate paid parental leave eligibility, as eligibility is determined on the basis of both weeks worked, and hours worked during those weeks. Data on weeks and hours worked is not available in the IDI, so paid parental leave eligibility is unable to be determined exactly. Eligibility for paid parental leave is based on a person having been employed as an employee (not necessarily for the same employer) for at least an average of 10 hours per week over any 26 of the 52 weeks just before the baby's due date. See: https://www.employment.govt.nz/leave-and-holidays/parental-leave/parental-leave-payment/who-can-get-parental-leave-payments/.

the apparent impact ascribed to the program could in fact be due to these systematic differences.

Our approach to dealing with this issue is to compare pre-post outcomes in the year of implementation with similar pre-post outcomes in 2015-2017, following a difference-in-difference analysis conducted by Deutscher & Breunig (2018) examining the introduction of an Australian 'Baby Bonus' payment.

Figure 2 provides a stylised illustration of the approach: 'A' represents the difference between the average outcome over the six months post-birth for the cohort of families with children born July – September 2018 and the average outcome for the cohort of families with children born April – June 2018; 'B' represents the same difference calculated over the equivalent comparison in 2015, 2016 and 2017; the difference-in-difference – 'DiD' – is calculated by subtracting B from A.

Figure 2: The difference-in-difference estimation



Formally, the difference-in-differences estimator of the impact of the introduction of the early-years Families Package changes on outcome y is given in equation (1).

$$I_y = \left(\bar{y}_{\geq Jul2018} - \bar{y}_{\leq Jun2018}\right) - \left(\bar{y}_{\geq Jul2015-2017} - \bar{y}_{\leq Jun2015-2017}\right) \quad (1)$$

Where I_y is the estimated impact on outcome y at a specified age, and $\bar{y}_{\geq Jul2018}$ represents the average outcome for those with children born in a specified window after July 2018.

In order to provide a valid estimate of the impact, our analysis is dependent on an assumption that the differences in pre-July cohort and post-1 July cohort outcomes would have been consistent across the 2015-2018 period without the introduction of the early-years Families Package changes. This is known as the common trends assumption.

This can be re-specified such that the impact I_y on outcome y is expressed as regression parameter β_3 in equation (2).

$$y_i = \beta_0 + \beta_1 z_{2018,i} + \beta_2 z_{>July,i} + \beta_3 z_{2018,i} \cdot z_{>July,i} + \varepsilon_i \quad (2)$$

Where y_i represents outcome y for individual i in our analysis sample composed of families with children born in specified windows before and after 1 July each year, $z_{2018,i}$ is an indicator variable which is set to 1 if a child was born in 2018 and 0 otherwise, and $z_{>July,i}$ is an indicator variable which is set to 1 if a child was born after July.

By estimating the impact in this way, we can add in control variables that account for compositional differences between the characteristics of families with children born in 2018 and those born in earlier years (X_{ij}).

$$y_i = \beta_0 + \beta_1 z_{2018,i} + \beta_2 z_{>July,i} + \beta_3 z_{2018,i} \cdot z_{>July,i} + \gamma_j X_{ij} + \varepsilon_i \quad (3)$$

Control variables include ethnic composition, regional council area, neighbourhood deprivation (NZDEP quintiles), pre-birth income,[19] an indicator of whether at least one parent appears to meet the eligibility criteria for Working for Families tax credits, maternal age, employment history (months worked and income over different periods pre-birth), an indicator of whether two parents are recorded on the birth certificate, benefit history over different periods pre-birth, an indicator of estimated eligibility for paid parental leave, and the number of older siblings born to either parent.

As we have multiple comparison years, we can add further terms to our model to establish whether there is any evidence that the common trends assumption does not hold.

$$y_i = \beta_0 + \beta_1 z_{2018,i} + \beta_2 z_{>July,i} + \beta_3 z_{2018,i} \cdot z_{>July,i} + \beta'_3 z_{2017,i} \cdot z_{>July,i} + \beta''_3 z_{2016,i} \cdot z_{>July,i} + \gamma_j X_{ij} + \varepsilon_i \quad (4)$$

In this specification, β'_3 and β''_3 provide estimates of any divergence from the 2015 pre-July and post-1 July trend, in 2017 or 2016, respectively. If these parameters are statistically significant it would suggest the common trends assumption may not hold.

This design allows us to account for any systematic differences that exist in the outcomes for families with children born pre- and post-1 July, where those systematic differences are consistent across our study years.

The design does not account for other changes that may have influenced relativities in outcomes for families with children born pre- and post-1 July over the years of interest where those changes are not consistent year to year, as might be the case with a sharp change in economic conditions or prices. Inspection of consumer and house price indexes and the unemployment rate and employment ratio (the proportion of the population employed) for men and women in quarters covering the six-month outcome periods for the pre- and post-1 July birth cohorts showed no sharp changes of this nature, with the exception of the consumer price index in 2016 (Figure 3). We consider that the controls included for income, benefit and employment status of parents prior to the birth provides adequate controls for the slight variation in the economic circumstances of families in the cohorts studied.

Similarly, the design also does not account for other changes that may have influenced relativities in outcomes over the years of interest where those changes are not consistent year to year, such as other community, policy or service delivery changes. This necessitates a good understanding of other changes affecting families with young children over the period of study.

All models are estimated as linear models with heteroscedastic-robust standard errors clustered at the family (i.e. children with the same mother/first caregiver) level. Clustered

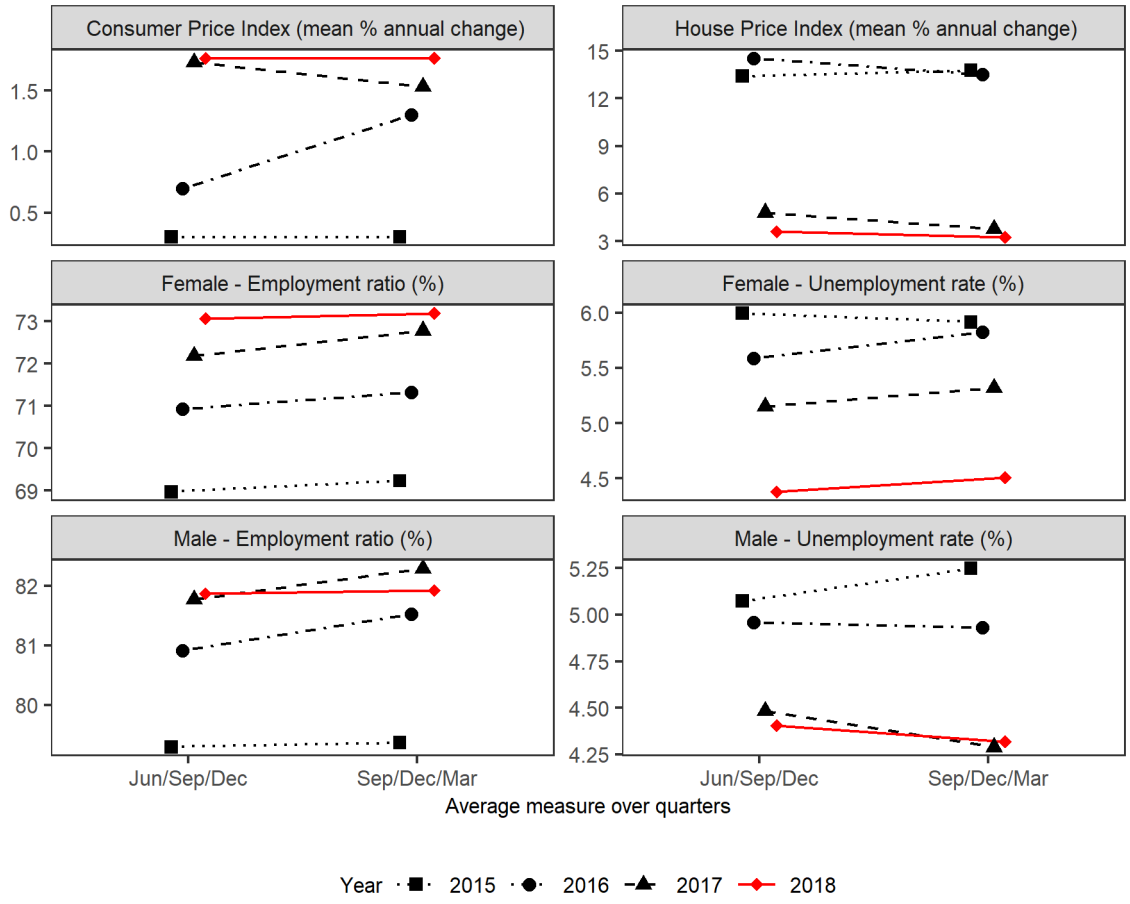
[19] Income is not adjusted for inflation; however, pre-birth income measures are interacted with a calendar year indicator to allow the relationship between income and our outcome measure to change over time.

errors help to account for auto-correlation between the outcomes for parent-child dyads with the same parent.

Although regression discontinuity methods that estimate discontinuities in outcomes depending on the birth date of the child could be explored, we favour the approach outlined above for two reasons:

- families could qualify for Best Start if their child was *due on or after 1 July 2018 but born before that date*, meaning that a sharp discontinuity according to the birth date of children born immediately before and after the implementation date would not be expected
- due to de-identification applied to data held in the Integrated Data Infrastructure, birth year and month were available for analysis, but not birthdate.

Figure 3: Mean consumer price index, house price index, employment ratio and unemployment rate for quarters covering the period six months post-birth for pre- and post-1 July birth cohorts



Source: Employment and unemployment statistics, Statistics NZ Household Labour Force Survey. Consumer Price Index, Statistics NZ. House Price Index, Reserve Bank of New Zealand/CoreLogic. Authors' calculations.

Additional robustness and sensitivity tests

In addition to the tests of common trends described earlier in our model specification, we also run a series of models that estimate treatment effects for a range of maternal characteristics. These characteristics are treated as outcome variables, instead of controls, in our model. Under the common trends assumption, we would expect differences in these characteristics within birth years to be similar, and our estimation should not uncover

significant effects. These effects could signal changes in the underlying characteristics of our population across the pre-July and post-1 July periods, suggesting a possible violation of the common trends assumption.

A series of sensitivity tests are applied that assess whether results are altered if we change the time periods examined, change the cohorts of interest or use alternative data sources to infer parent-child relationships. These tests are described in the results section.

Finally, we apply supplementary robustness checks to assess whether other selected policies or events could explain the results. This involves removing periods in which new-borns and their families could have been affected by these other policies or events. These supplementary checks are described in the discussion of results.

Ethical review

The research did not fall within the scope of Health and Disability Ethics Committee Review. The research team received ethical review from an independent Research Ethics Panel established to provide advice on MSD and other government agency projects.

5. Results

The study cohorts

Table 4 provides a descriptive profile of the 2018 pre- and post-1 July study cohorts and equivalent pooled data for the three previous years. Appendix 3 provides profiles for the three control years separately.

As would be expected given the size of the cohorts and the proximity of birth timing, the composition of the pre- and post-1 July study cohorts is very similar within each of the years, with few statistically significant differences:

- the only statistically significant difference comparing pre- and post-1 July cohorts in 2018 was in the proportion of children with mothers/first parents with earned income in the month before the birth (increasing slightly from 55 percent to 56 percent), and in the level of pre-birth income,[20] with the post-1 July cohorts having slightly higher average income prior to the birth
- earlier years saw significant differences comparing pre-and post-1 July cohorts in the ethnic composition of babies and their parents, in the proportion with a mother/first parent supported by benefit with a partner, and in the proportion with a father with earned income pre-birth.

While significant, these differences were all small, and unlikely to impact on our results.

Comparing 2018 with the pooled average for previous years:

- there were statistically significant but small increases over time in the proportions of new-borns who were of Asian ethnicity (reaching 20 percent in the 2018 cohorts), who had a mother/first parent aged 30 or over, and whose mother/first parent had earned income in the month before the birth
- there were similarly small decreases in the amount of time mothers/first parents spent on benefit pre-birth, in the number of new-borns with older siblings, and in the proportion whose family met Working for Families residence requirements
- consistent with the higher proportion of mothers/first parents who were in paid employment pre-birth, the proportion of children with a mother/first parent estimated to be eligible for paid parental leave (and not on benefit) was larger in 2018 than in earlier years.

In light of these differences, all results in the sections that follow include controls for socio-demographic characteristics.[21]

[20] In the reporting that follows, 'income' is gross income unless otherwise specified.
[21] Results for specifications without controls for socio-demographic characteristics are available from the authors on request.

Table 4: Socio-demographic characteristics of children born 3 months pre- and post-1 July and their parents as recorded on birth certificates, 2018 and pooled control years

births in 3 months either side of 1 July:	2018 Pre-	2018 Post-	Pooled 2015-2017 Pre-	Pooled 2015-2017 Post-
CHILD				
Ethnic groups				
European	64%	64%	65%	64%
Māori	28%	28%	28%	28%
Pacific	16%	15%	15%	15%
Asian	20%	20%	18%	19%
MELAA	2%	2%	2%	2%
Other	0%	0%	1%	1%
No siblings	42%	42%	41%	41%
At least one parent eligible for WFF tax credits	96%	96%	97%	97%
MOTHER/FIRST PARENT				
Age				
Under 25	19%	18%	20%	20%
25-29	28%	27%	27%	28%
30-34	32%	32%	31%	31%
35 and over	21%	22%	21%	21%
Ethnic groups				
European	61%	61%	62%	61%
Māori	23%	23%	23%	23%
Pacific	12%	11%	12%	11%
Asian	19%	19%	18%	19%
MELAA	2%	2%	2%	2%
Other	1%	1%	1%	1%
With earned income in the month prior to birth	55%	56%	52%	52%
Mean earned income prior to birth - 0-6 months	$14,834	$14,886	$13,164	$13,213
Mean total income prior to birth - 6 months to 1.5 years	$35,064	$35,818	$31,606	$31,995
Mean total income prior to birth - 1.5 to 4.5 years	$88,492	$89,172	$84,645	$84,245
Supported by benefit in the month before birth	20%	20%	22%	22%
Supported by benefit with partner in month before birth	3%	3%	3%	4%
FATHER/SECOND PARENT				
No father/second parent on birth registration	5%	5%	5%	5%
Where father/second parent recorded:				
Ethnic groups				
European	56%	56%	58%	57%
Māori	20%	20%	20%	20%
Pacific	13%	12%	12%	12%
Asian	18%	18%	16%	17%
MELAA	2%	2%	2%	2%
Other	2%	2%	2%	2%
With earned income in the month prior to birth	79%	78%	78%	77%
Mean earned income prior to birth - 0-6 months	$29,145	$28,995	$26,505	$26,715
Mean total income prior to birth - 6 months to 1.5 years	$58,822	$60,088	$53,339	$52,842
Mean total income prior to birth - 1.5 to 4.5 years	$138,452	$139,189	$128,514	$127,118
NEIGHBOURHOOD				
Deprivation quintile (NZDep2013)				
1	15%	16%	16%	16%
2	18%	18%	18%	18%
3	19%	20%	19%	19%
4	21%	21%	21%	21%
5	26%	26%	26%	26%
ANALYSIS SUB-GROUP (of mother/first parent)				
(1) in receipt of benefit in month prior to birth	20%	20%	22%	22%
(2) not in receipt of benefit and PPL eligible	56%	56%	53%	52%
(3) not in receipt of benefit and not PPL eligible	24%	24%	26%	26%
Total %	100%	100%	100%	100%
Total n	14,427	14,742	44,361	45,366

In 2018, around one in five study cohort children had a mother/first parent supported by benefit in the month prior to birth. Slightly over half had a mother/first parent estimated to be eligible for paid parental leave (and not on benefit). Around a quarter had a

mother/first parent estimated to be not eligible for paid parental leave (and not on benefit).

Table 5 shows the contrasting profiles of these three sub-groups, within the 2018 post-1 July cohort.

- Where children had a mother/first parent supported by benefit, in most cases mothers/first parents were single, and just over half resided in the most deprived quintile of neighbourhoods.
- Where children had a mother/first parent estimated to be eligible for paid parental leave (and not on benefit), parents had the highest mean levels of pre-birth earnings, and children were the most likely to have no siblings. We lack data on family structure, but expect that in most cases parents were partnered.
- Where children had a mother/first parent estimated to be not eligible for paid parental leave (and not on benefit), mothers/first parents had the lowest mean levels of pre-birth earnings. We expect that in most cases mothers/first parents were supported by a working partner. This sub-group had the lowest proportion estimated to meet the residence test for Best Start and other Working for Families tax credits (92 percent, compared with 100 and 96 percent of the other sub-groups).

Mothers/first parents of new-borns from different ethnic groups had different probabilities of being in different financial situations (Table 6). Mothers/first parents of Māori and Pacific children were more likely than mothers/first parents of non-Māori, non-Pacific children to be supported by benefit before the birth of their child, and more likely to meet residence requirements for Best Start and other Working for Families tax credits.

The distribution of children in different ethnic groups across analysis sub-groups varied: 41 percent of Māori children in the 2018 post-1 July cohort had a mother/first parent supported by benefit; 41 percent had a mother/first parent estimated to be eligible for paid parental leave (and not on benefit); and 18 percent had a mother/first parent estimated to be not eligible for paid parental leave (and not on benefit). For Pacific children, the equivalent proportions were 35, 42, and 23 percent. For non-Māori, non-Pacific new-borns, the proportions were eight, 66, and 26 percent.

Table 6 highlights the high proportion of Māori and Pacific new-borns who identified as having multiple ethnic groups. Of the Māori new-borns in the cohort, two thirds were also European and almost one in five also identified as having a Pacific ethnicity. Of the Pacific new-borns in the cohort, one third were also European and one third were also Māori.

Table 5: Socio-demographic characteristics of children born 3 months post-1 July 2018 and their parents as recorded on birth certificates, by analysis sub-group

Analysis sub-group of mother/first parent	Supported by benefit	Estimated to be eligible for paid parental leave	Estimated to be not eligible for paid parental leave
CHILD			
Ethnic groups			
European	52%	71%	58%
Māori	58%	21%	21%
Pacific	27%	11%	15%
Asian	6%	22%	28%
MELAA	3%	2%	3%
Other	0%	0%	1%
No siblings	27%	49%	38%
Siblings with previous C&P notifications	43%	7%	9%
At least one parent eligible for WFF tax credits	100%	96%	92%
MOTHER/FIRST PARENT			
Age			
Under 25	43%	10%	17%
25-29	29%	27%	26%
30-34	16%	38%	34%
35 and over	11%	25%	23%
Ethnic groups			
European	50%	68%	53%
Māori	53%	15%	16%
Pacific	21%	8%	12%
Asian	5%	21%	28%
MEELA	2%	2%	3%
Other	1%	1%	1%
With earned income in the month prior to birth	10%	90%	15%
Mean earned income prior to birth - 0-6 months	$1,450	$25,231	$1,561
Mean total income prior to birth - 6 months to 1.5 years	$23,130	$52,460	$6,878
Mean total income prior to birth - 1.5 to 4.5 years	$54,893	$121,816	$40,281
Supported by benefit in the month before birth	100%	0%	0%
Supported by ben. with partner mth. before birth	16%	0%	0%
FATHER/SECOND PARENT			
No father/second parent on birth registration	19%	1%	2%
Where father/second parent recorded:			
Ethnic groups			
European	35%	66%	52%
Māori	40%	15%	16%
Pacific	18%	10%	13%
Asian	5%	20%	26%
MELAA	3%	2%	3%
Other	1%	2%	2%
With earned income in the month prior to birth	42%	89%	81%
Mean earned income prior to birth - 0-6 months	$10,253	$34,112	$29,745
Mean total income prior to birth - 6 months to 1.5 years	$26,014	$70,476	$58,855
Mean total income prior to birth - 1.5 to 4.5 years	$63,387	$162,120	$136,872
NEIGHBOURHOOD			
Deprivation quintile (NZDep2013)			
1	4%	19%	16%
2	8%	20%	19%
3	15%	21%	20%
4	22%	20%	21%
5	51%	18%	23%
Total %	100%	100%	100%
Total n	2,931	8,313	3,495

Table 6: Socio-demographic characteristics of children born 3 months post-1 July 2018 and their parents as recorded on birth certificates, by ethnic group

High-level ethnic group of child:	Māori	Pacific	Non-Māori, non-Pacific
CHILD			
Ethnic groups			
European	66%	34%	70%
Māori	100%	34%	0%
Pacific	18%	100%	0%
Asian	3%	5%	30%
MELAA	1%	1%	4%
Other	0%	0%	1%
No siblings	31%	34%	49%
Siblings with previous C&P notifications	30%	21%	8%
At least one parent eligible for WFF tax credits	100%	97%	94%
MOTHER/FIRST PARENT			
Age			
Under 25	34%	33%	9%
25-29	29%	31%	26%
30-34	23%	21%	38%
35 and over	14%	15%	27%
Ethnic groups			
European	61%	33%	67%
Māori	74%	30%	2%
Pacific	9%	69%	1%
Asian	2%	4%	30%
MELAA	1%	1%	3%
Other	1%	1%	2%
With earned income in the month prior to birth	43%	41%	65%
Mean earned income prior to birth - 0-6 months	$8,827	$8,377	$17,743
Mean total income prior to birth - 6 months to 1.5 years	$30,307	$29,044	$39,427
Mean total income prior to birth - 1.5 to 4.5 years	$77,505	$68,070	$98,046
Supported by benefit in the month before birth	41%	35%	8%
Supported by ben. with partner mth. before birth	5%	5%	2%
FATHER/SECOND PARENT			
No father/second parent on birth registration	9%	9%	2%
Where father/second parent recorded:			
Ethnic groups			
European	50%	20%	67%
Māori	64%	19%	2%
Pacific	13%	74%	1%
Asian	2%	4%	27%
MELAA	1%	0%	3%
Other	1%	1%	3%
With earned income in the month prior to birth	67%	67%	85%
Mean earned income prior to birth - 0-6 months	$19,755	$19,266	$33,309
Mean total income prior to birth - 6 months to 1.5 years	$44,687	$39,938	$69,670
Mean total income prior to birth - 1.5 to 4.5 years	$107,969	$91,586	$159,485
NEIGHBOURHOOD			
Deprivation quintile (NZDep2013)			
1	8%	5%	21%
2	11%	8%	22%
3	18%	13%	22%
4	23%	21%	20%
5	40%	53%	14%
ANALYSIS SUB-GROUP (of mother/first parent)			
(1) in receipt of benefit in month prior to birth	41%	35%	8%
(2) not in receipt of benefit and PPL eligible	41%	42%	66%
(3) not in receipt of benefit and not PPL eligible	18%	23%	26%
Total %	100%	100%	100%
Total n	4,188	2,259	9,060

Outcomes for pre- and post-1 July birth cohorts

Descriptive data on outcomes for pre- and post-1 July study cohorts in 2018 compared with previous years are presented in Figures 4 – 6 for total income in the first six months post-birth, and months not in work in the first six and 12 months post-birth.

Income

Figure 4 shows mean total gross income for different cohorts of mothers/first parents and fathers/second parents, as well as for sub-groups of mothers/first parents in the first six months following the birth. Cohorts of both mothers/first parents and fathers/second parents experienced year-on-year increases in this measure of mean income between 2015 and 2017, consistent with earnings growth over the period.

The mean income for the pre-1 July cohort was substantially higher in 2018 than earlier years. Overall, mean income was $1,900 ($74 per week) higher in the six months following the birth for mothers/first parents in the 2018 birth cohort than for mothers/first parents in the 2017 cohort. The large increase was driven by a mix of factors:

- mothers/first parents earned higher employment income and received more paid parental leave post-birth than earlier cohorts, consistent with higher employment rates and earnings pre-birth, explaining over half of the difference
- the cohort also received more Family Tax Credit and Accommodation Supplement payments than earlier cohorts, consistent with the Families Package increases to these payments.

Income from Families Package and other benefit payments (inclusive of paid parental leave) increased by $49 a week on average.

Mothers/first parents in the pre-1 July cohort who were receiving a benefit in the month before the birth had particularly large gains in total income comparing 2017 and 2018, with incomes being almost $2,900 ($110 per week) higher in the six months following the birth for the 2018 birth cohort than for the 2017 cohort. Almost half of this increase was due to higher income from Working for Families tax credits, again consistent with the Families Package increases.

For fathers/second parents in the pre-1 July cohort, the increase in income in 2018 was similar to that of mothers/first parents in absolute terms, but much smaller in relative terms, and was driven almost entirely by increases in employment income, including self-employment.

In general, there were only small differences between pre- and post-1 July cohorts in any of the control years 2015-2017. The most marked of these increases was between the income of fathers/second parents pre- and post-1 July in 2017, a change driven entirely by rises in post-birth employment income. These consistent patterns across control years provides some encouragement that our common trends assumption may hold with respect to income.

As expected, income for mothers/first parents of new-borns increased markedly between the pre- and post-1 July 2018 cohorts with the introduction of the Families Package early-years changes. These increases were evident for all main ethnic groups, and, to a lesser or greater degree the three analysis sub-groups. Increases were largest for mothers/first parents who were estimated to be eligible for paid parental leave (and not on benefit), and smaller for other sub-groups, particularly for those mothers/first parents who were on benefit in the month before the birth.

No clear pattern of change is apparent for fathers/second parents between pre- and post-1 July periods of 2018.

Months with no wages and salaries

In 2015-2017, mothers/first parents of children born post-1 July spent a similar number of months with no wages and salaries in the first six months post-birth as mothers/first parents of children born pre-1 July (Figure 5).

In 2018, mothers/first parents of babies born pre-1 July spent less time with no wages and salaries after the birth than earlier cohorts had (by almost 0.1 of a month, around three days over 6 months). This is consistent with this group's higher likelihood of working pre-birth, and higher levels of post-birth employment income discussed above. The post-1 July 2018 cohort of mothers/first parents had similar time not working to post-1 July cohorts in earlier years, despite the higher levels of pre-birth employment discussed earlier.

When we look at analysis sub-groups, the increase in time with no wages and salaries is only evident for mothers/first parents who were estimated to be eligible for paid parental leave (and not on benefit), consistent with the income gains this group appeared to receive.

Despite income gains comparing pre- and post-1 July 2018 cohorts being similar across mothers/first parents in different ethnic groups (Figure 4), only non-Māori, non-Pacific mothers/first parents appeared to increase their time with no wages and salaries in Figure 5. The effect was to reduce ethnic differences in time with no wages and salaries post-birth. Patterns over 12 months were similar.

No clear pattern of change is apparent for fathers/second parents.

Figure 4: Mean gross income from all sources in the six months post-birth for pre- and post-1 July birth cohorts

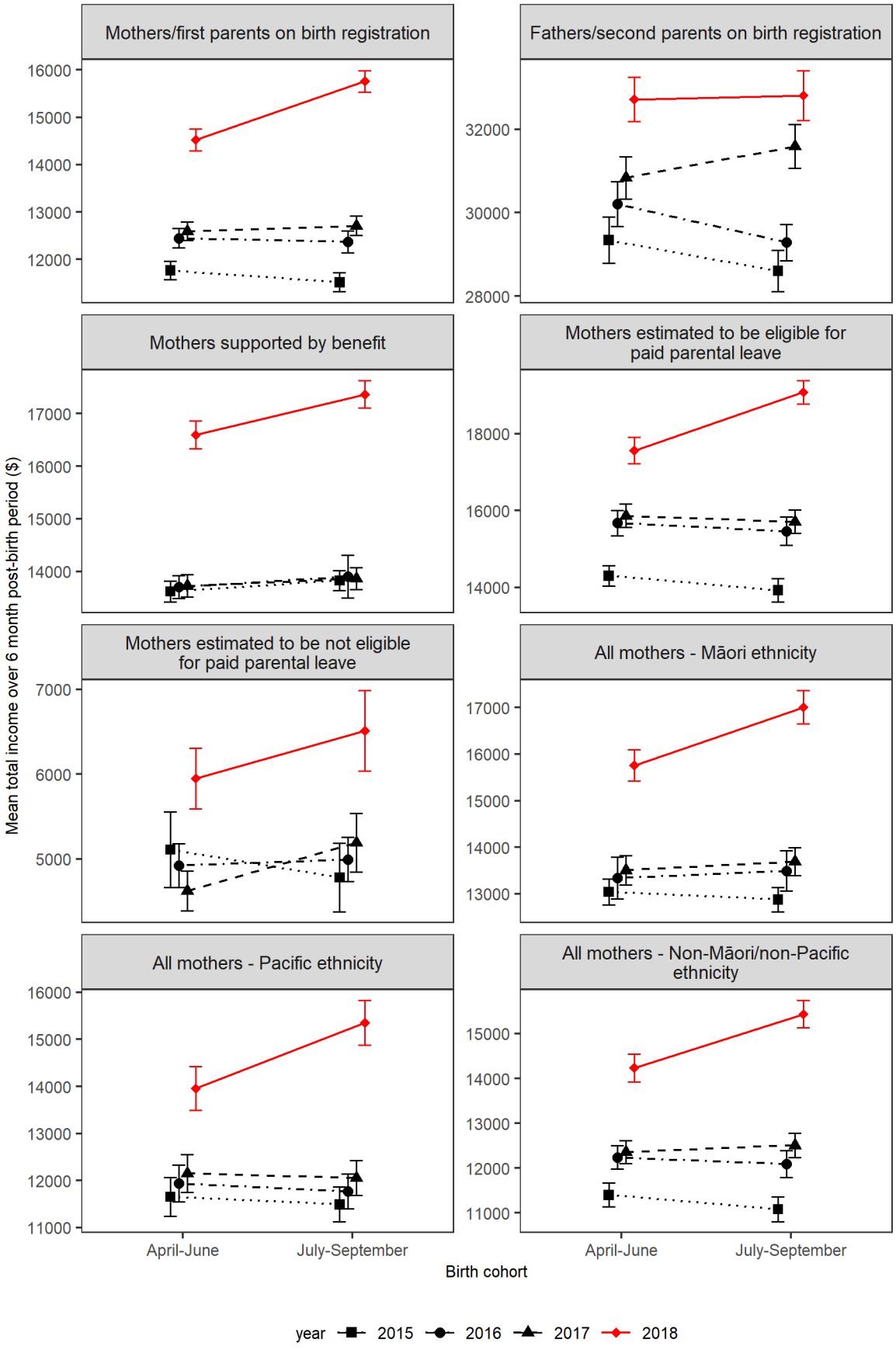


Figure 5: Mean months with no wages and salaries (proxy for time spent at home with infant) in the six months post-birth for pre- and post-1 July birth cohorts

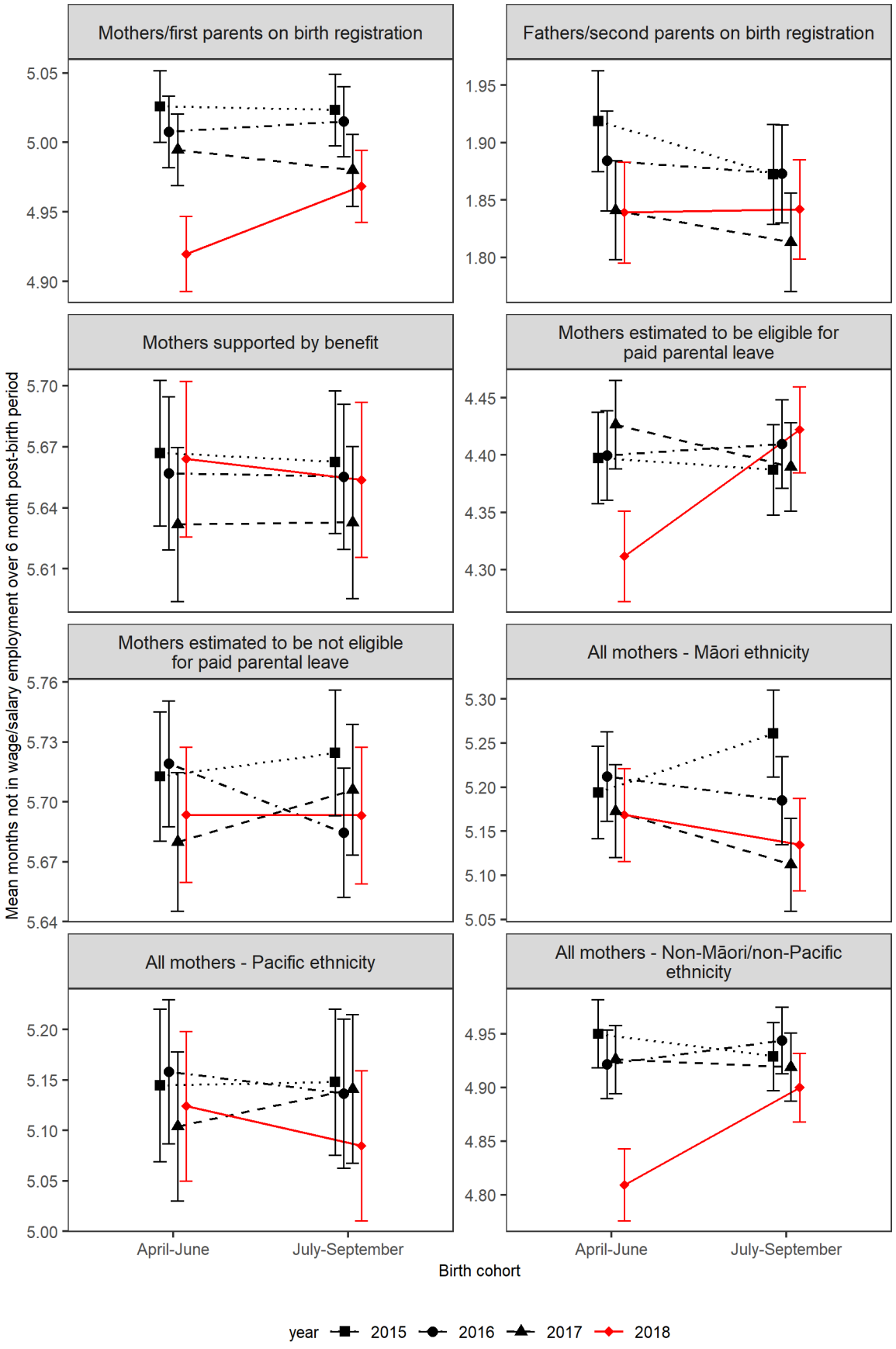
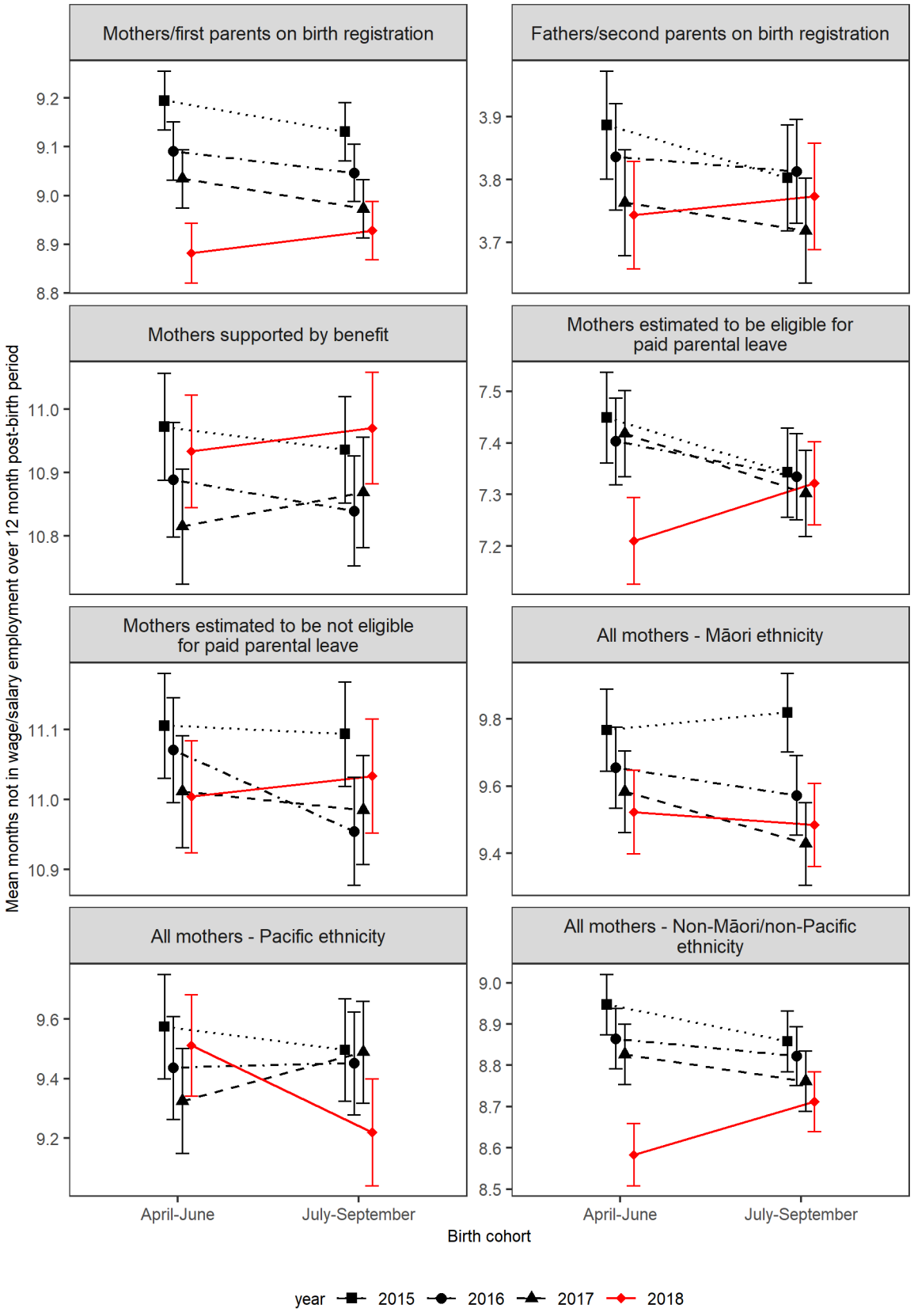


Figure 6: Mean months with no wages and salaries (proxy for time spent at home with infant) in the 12 months post-birth for pre- and post-1 July birth cohorts



Impact estimates

Income

Table 7 presents gross (before tax) and net (after tax) income difference-in-difference estimates for different groups of parents in the first six months following the birth, with all control variables included in the models.

Impacts are expressed in dollars, and as a percentage of the counterfactual income - the income we estimate the person would have received in the first six months post-birth had they not been members of the first three-month cohort eligible for the early-years Families Package changes (i.e. they had received the other Families Package changes they were eligible for but not Best Start and paid parental leave, as summarised in Table 3).

Overall, we estimate that being in the three-month birth cohort that was the first to be eligible for the early-years Families Package changes increased mothers/first parents' before-tax incomes by an average of over $1,400 in the first six months after the birth of their child (equivalent to $55 per week and representing a 10 percent increase in income). Mothers/first parents' after-tax incomes were estimated to have increased by over $1,200 (equivalent to $48 per week). Percentage gains in after-tax income were similar to before-tax gains. In what follows, we describe the before-tax income estimates.

The size of mothers/first parents' estimated income gains in the first six months varied depending on mothers/first parents' circumstances. Additional income ranged from around $820 ($31 per week) on average for mothers/first parents who were supported by benefit in the month before the birth, up to $1,900 ($72 per week) on average for mothers/first parents who were estimated to be eligible for paid parental leave. Mothers/first parents who were estimated to *not* be eligible for paid parental leave received less additional money in the first six months ($1,040, or $40 per week, on average) than those who were eligible. However, this represented a larger relative increase in their personal income of 19 percent (because many are likely to have a working partner, the percentage increase is likely to represent a smaller share of their family income).

Income gains were similar for mothers/first parents from different ethnic groups, with increases ranging from $1,310 ($50 per week) for Pacific mothers/first parents on average, to $1,460 ($56 per week) for Non-Māori, non-Pacific mothers/first parents on average. Māori mothers/first parents gained around $1,440 ($55 per week) in total income on average. These increases reflected estimated average income gains of between 9 and 10.5 percent.

Fathers'/second parents' before-tax incomes are estimated to have increased by around $450 over six months (equivalent to $17 per week) on average in the same period. This difference was not statistically significant.

Table 7: Estimated impact of Families Package early-years changes on parents' gross and net incomes over the first six months post-birth

	Gross income (before tax)		Net income (after tax)	
Panel A: All mothers/first parents				
Estimated impact	1438.96	**	1240.18	**
Standard error	[165.78]		[135.23]	
Impact as % of counterfactual	10.0%		10.2%	
N	117,183		117,183	
R-squared	0.415		0.385	
Panel B: All fathers/second parents				
Estimated impact	448.98		261.74	
Standard error	[363.03]		[278.08]	
Impact as % of counterfactual	1.4%		1.0%	
N	111,495		111,495	
R-squared	0.603		0.564	
Panel C: Mothers/first parents on benefit in the month before the birth				
Estimated impact	818.26	**	801.15	**
Standard error	[192.37]		[181.90]	
Impact as % of counterfactual	4.9%		5.2%	
N	24,828		24,828	
R-squared	0.234		0.226	
Panel D: Mothers/first parents estimated as eligible for paid parental leave				
Estimated impact	1881.37	**	1597.43	**
Standard error	[251.42]		[199.31]	
Impact as % of counterfactual	10.9%		11.5%	
N	62,640		62,640	
R-squared	0.351		0.312	
Panel E: Mothers/first parents estimated as not eligible for paid parental leave				
Estimated impact	1043.77	**	955.34	**
Standard error	[327.19]		[274.10]	
Impact as % of counterfactual	19.1%		18.8%	
N	29,718		29,718	
R-squared	0.429		0.405	
Panel F: All mothers/first parents - Māori ethnicity				
Estimated impact	1439.91	**	1254.92	**
Standard error	[244.33]		[214.31]	
Impact as % of counterfactual	9.3%		9.0%	
N	26,634		26,634	
R-squared	0.342		0.309	
Panel G: All mothers/first parents - Pacific ethnicity				
Estimated impact	1310.17	**	1089.16	**
Standard error	[318.64]		[291.61]	
Impact as % of counterfactual	9.3%		8.6%	
N	13,527		13,527	
R-squared	0.476		0.432	
Panel H: All mothers/first parents - Non-Māori, non-Pacific ethnicity				
Estimated impact	1462.45	**	1259.32	**
Standard error	[223.14]		[179.12]	
Impact as % of counterfactual	10.5%		11.0%	
N	79,395		79,395	
R-squared	0.429		0.401	

Note: * = significant at the 5 percent level, ** = significant at the 1 percent level.

Figure 7 presents difference-in-difference income estimates for all mothers/first parents, and fathers/second parents in the first six months following the birth, by income component. The impact on total income and on each income component was estimated separately.

As discussed earlier, the estimated income impacts largely reflect the size of the Families Package early-years 'treatment', providing additional income to families with young children through a combination of the new Best Start tax credit, and extensions to paid parental leave. The removal of the Parental Tax Credit offset some of this positive effect, as did small reductions in Temporary Additional Support and Accommodation Supplement income. Reductions in employment income also offset the gain. This could reflect the decision by some mothers/first parents to spend more time at home and delay their return to work, consistent with the policy intent.

The introduction of the Winter Energy Payment had a small, and seemingly counter-intuitive negative estimated impact on income. The payment was introduced in June 2018, and available until September of that year. It was therefore available for more weeks during the outcome period for the pre-1 July 2018 cohort than for the post-1 July 2018 cohort. In earlier years there was no pre- versus post-1 July effect, as the payment didn't exist. As a result, we estimate a negative impact on Winter Energy Payment income. This effect should disappear once we are able to estimate incomes over a full year period.

As noted, overall, we estimate being in the three-month cohort that was the first to be eligible for the Families Package early-years changes increased mothers/first parents' incomes by over $1,400 dollars. This was driven by an increase in paid parental leave and Best Start, which contributed $2,000 in additional income, offset by reduced employment income of almost $400.

Post-1 July 2018 cohort fathers/second parents were estimated to receive a $450 increase in income over the first six months after the birth of their child, a result that was not statistically significant. Fathers/second parents received a small, but statistically significant, increase in income from Best Start, partially offset by a small decrease in Parental Tax Credit (an increase of $100 in the former case and decrease of $60 in the latter case).

Figure 7: Estimated impact of Families Package early-years changes on parents' incomes over the first six months post-birth, by income source

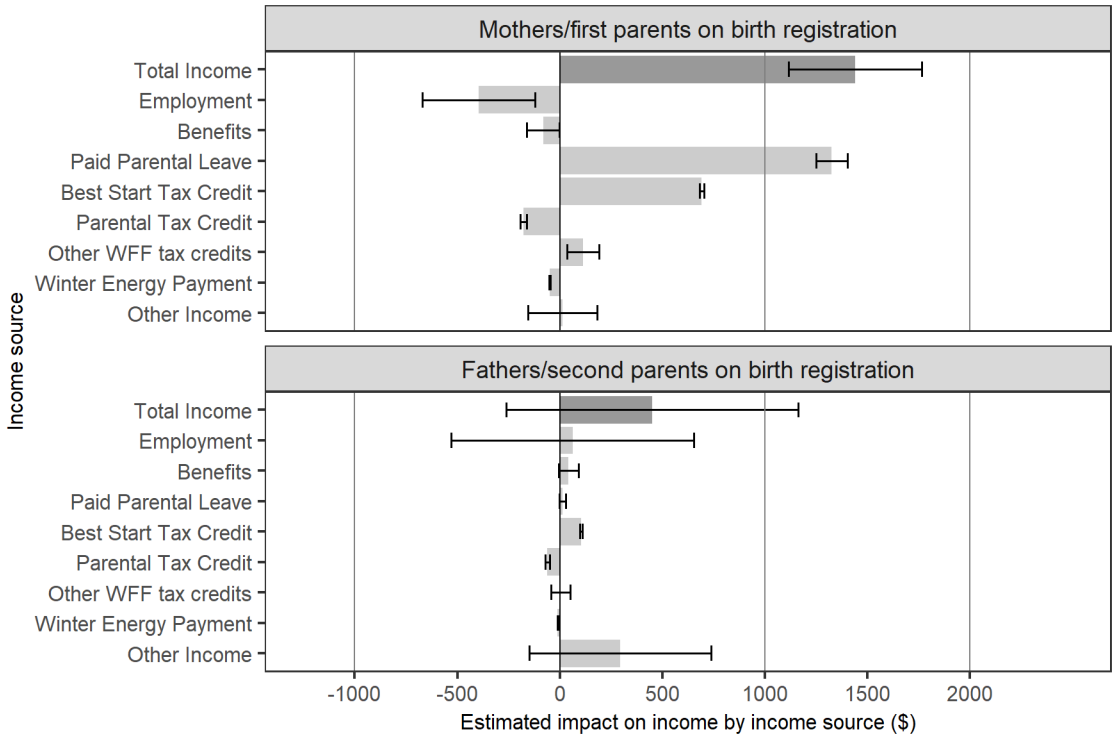


Figure 8 shows the way estimated impacts on income differed for our analysis sub-groups. As expected, the early-years Families Package changes impacted in markedly different ways.

- Mothers/first parents who were on benefit before the birth received the largest gain in income from Best Start of almost $1,200. However, this was largely offset by decreases in income from other sources, resulting in an overall estimated increase of around $820 in the first six months post-birth. These offsets were largely driven by a $150 loss of income from benefit sources (particularly Accommodation Supplement and Temporary Additional Support), and a $200 loss due to receiving less Winter Energy Payment than the pre-1 July birth cohort families.
- Mothers/first parents estimated to be eligible for paid parental leave (and not on benefit) received by far the biggest boost in post-birth income across our six-month period, estimated at almost $1,900, despite an estimated $800 drop in employment income. Gains from Best Start were relatively small for this group, at around $450. This reflects the policy design which prevented Best Start from being received at the same time as paid parental leave.
- Mothers/first parents estimated to be not eligible for paid parental leave (and not on benefit) received a large increase in income of around $830 due to the introduction of Best Start, however, this was offset by more than $500 of lost income due to the removal of the Parental Tax Credit. The group also received around $170 of additional paid parental leave income, highlighting the approximate nature of our estimation of eligibility.

Figure 8: Estimated impact of Families Package early-years changes on mothers' incomes over the first six months post-birth for analysis sub-groups, by income source

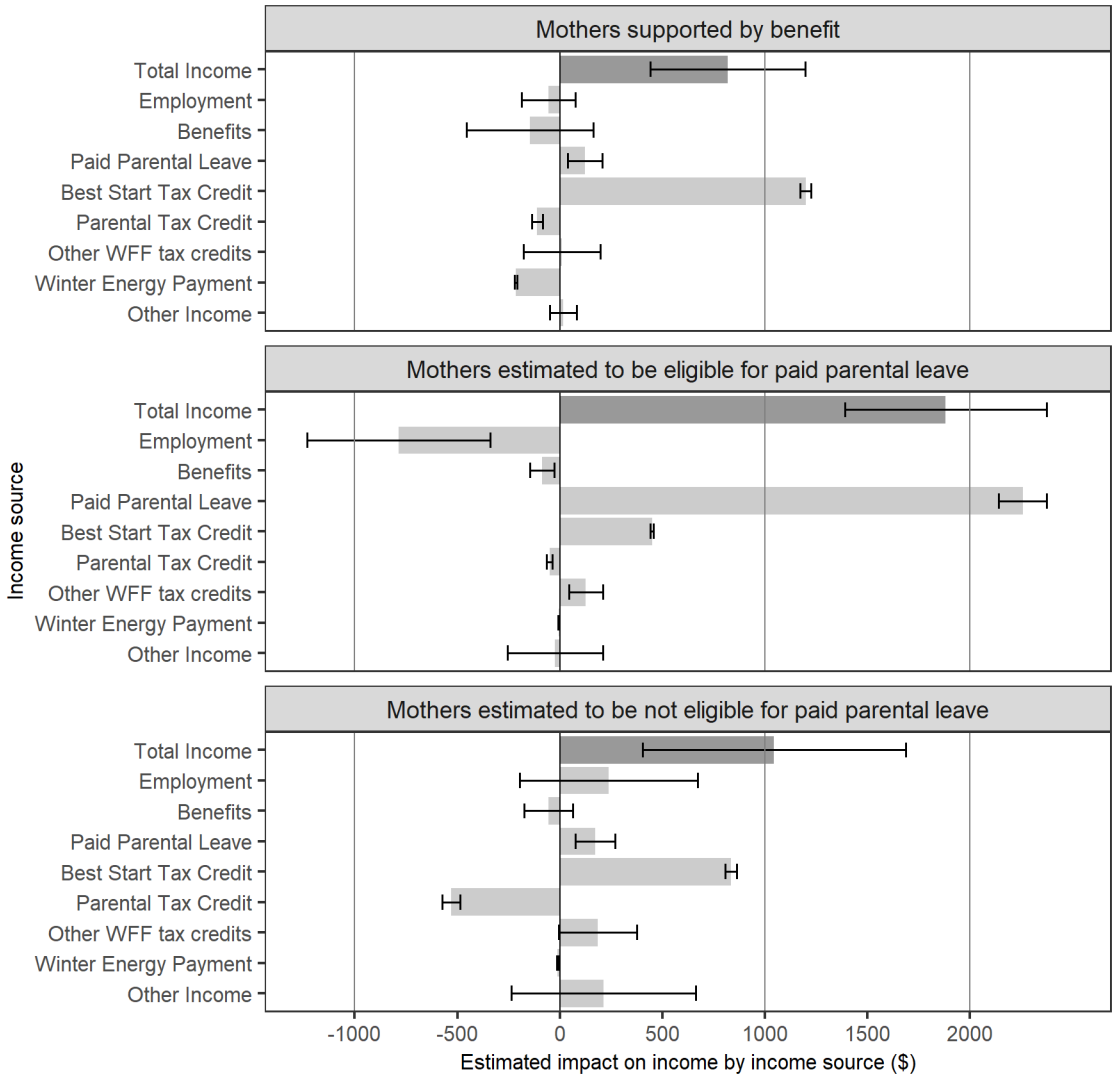


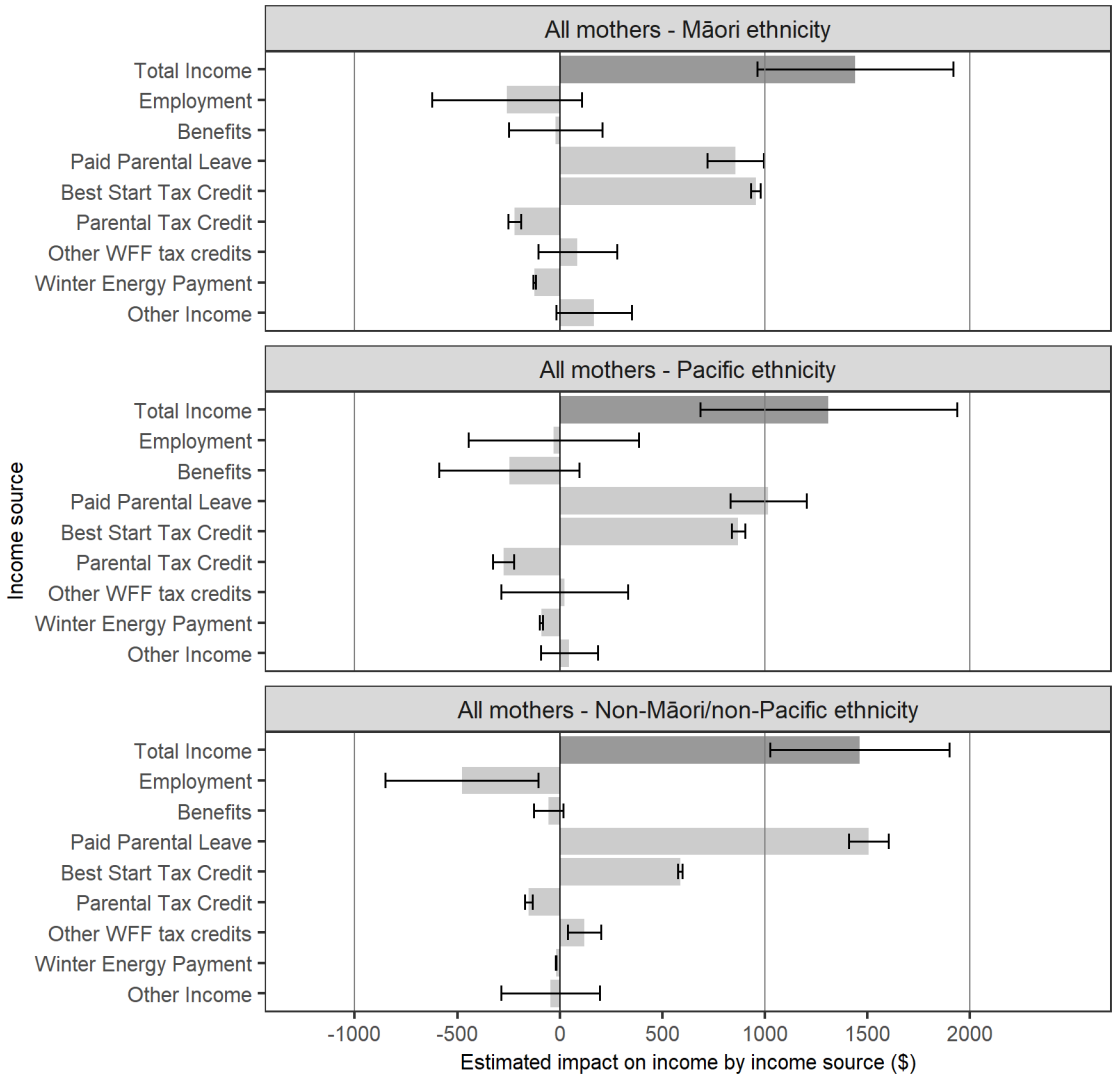
Figure 9 illustrates the way mothers/first parents of different ethnic groups benefitted from being in the post-1 July 2018 cohort eligible for the Families Package early-years changes.

Overall, while the different ethnic groups benefitted from the package similarly in total dollar terms, the distribution of these effects was somewhat different, consistent with their probability of being in the different analysis sub-groups discussed above, and differences in the likelihood of meeting residence requirements for Best Start and other Working for Families tax credits.

Pacific and Māori mothers/first parents had very similar patterns of income change, with similar, and almost equally split average gains of around $800-$1,000 in paid parental leave and Best Start tax credit income, partially offset by a loss of more than $200 on average in Parental Tax Credit income.

Non-Māori, non-Pacific mothers/first parents, on the other hand, benefitted far more substantially from paid parental leave changes (by around $1,500 on average), and received less than $600 in Best Start tax credit income on average. This was also the only group to experience a significant drop in employment income of around $480, possibly due to a decision to delay returning to work, while the loss of Parental Tax Credit income was smaller, estimated at around $150.

Figure 9: Estimated impact of Families Package early-years changes on mothers/first parents' incomes over the first six months post-birth by ethnic group and income source



Months with no wages and salaries

We developed a proxy indicator of time parents spent at home with infants based on the number of months after the birth that the parent was not receiving wage or salary income. Estimated impacts on this measure as a result of being in the first-three-month cohort eligible for the Families Package early-years changes are reported in Table 8.

Table 8: Estimated impact of Families Package early-years changes on months with no wages and salaries (proxy for time spent at home with infant) in specified periods post-birth

	Within 6 months from birth		Within 12 months from birth	
Panel A: All mothers/first parents				
Estimated impact	0.063	**	0.141	**
Standard error	[0.023]		[0.048]	
Impact as % of counterfactual	1.3%		1.6%	
N	117,183		117,183	
R-squared	0.290		0.396	
Panel B: All fathers/second parents				
Estimated impact	0.006		0.035	
Standard error	[0.023]		[0.047]	
Impact as % of counterfactual	0.3%		0.9%	
N	111,495		111,492	
R-squared	0.740		0.709	
Panel C: Mothers/first parents on benefit in the month before the birth				
Estimated impact	-0.014		0.037	
Standard error	[0.034]		[0.078]	
Impact as % of counterfactual	-0.2%		0.3%	
N	24,828		24,828	
R-squared	0.229		0.247	
Panel D: Mothers/first parents estimated as eligible for paid parental leave				
Estimated impact	0.120	**	0.214	**
Standard error	[0.037]		[0.077]	
Impact as % of counterfactual	2.8%		3.0%	
N	62,640		62,640	
R-squared	0.150		0.216	
Panel E: Mothers/first parents estimated as not eligible for paid parental leave				
Estimated impact	0.005		0.078	
Standard error	[0.031]		[0.072]	
Impact as % of counterfactual	0.1%		0.7%	
N	29,718		29,718	
R-squared	0.185		0.199	
Panel F: All mothers/first parents - Māori ethnicity				
Estimated impact	-0.037		0.072	
Standard error	[0.043]		[0.093]	
Impact as % of counterfactual	-0.7%		0.8%	
N	26,634		26,634	
R-squared	0.358		0.448	
Panel G: All mothers/first parents - Pacific ethnicity				
Estimated impact	-0.028		-0.187	
Standard error	[0.060]		[0.130]	
Impact as % of counterfactual	-0.6%		-2.0%	
N	13,527		13,527	
R-squared	0.366		0.472	
Panel H: All mothers/first parents - Non-Māori, non-Pacific ethnicity				
Estimated impact	0.108	**	0.209	**
Standard error	[0.029]		[0.060]	
Impact as % of counterfactual	2.3%		2.5%	
N	79,395		79,395	
R-squared	0.262		0.369	

Note: * = significant at the 5 percent level, ** = significant at the 1 percent level.

Overall, we observe small positive increases in months not working for mothers/first parents. Over the first six months post-birth, mothers/first parents are estimated to spend 0.06 of a month more not working on average, equating to a 1.3 percent increase. Equivalent figures for the twelve-month post-birth period are estimated at 0.14 of a month, or a 1.6 percent increase.

Looking at our sub-groups of interest, only mothers/first parents not on benefit who were estimated to be eligible for paid parental leave reduced the time they spent working post-birth by a statistically significant amount, increasing the time out of work by 0.12 months (or 2.8 percent) over six months (around four days) on average, and 0.21 months (or 3.0 percent) over 12 months (around six days) on average.

For the broad ethnic groupings, only the non-Māori, non-Pacific group of mothers/first parents had a statistically significant estimated increase in months not working, consistent with this group being much more likely than Māori or Pacific mothers/first parents to be eligible for paid parental leave.

Robustness and sensitivity tests

Robustness tests – testing for common trends using a placebo policy change

In order to interpret statistically significant results as causal impacts, we need to verify the assumption that prior to implementation of the Families Package early-years changes, there was no significant deviation in the relativity of outcomes for births pre- and post-1 July occurring between years. This is sometimes referred to as the 'common trends' assumption.

As discussed earlier, to test this assumption we added interaction terms between our indicator of post-1 July births and the 2016 and 2017 years to our model. Essentially this provides a placebo policy change test in each of the earlier years, relative to the 2015 year.

For income and employment, we ran a total of 88 models with control variables included in them. In 52 (59 percent) of these models we estimated statistically significant impacts on our main policy change outcome of interest – outcomes for post-1 July 2018 births, and 50 (57 percent) were statistically significant at the 1 percent level of significance.

Given we had two common trends (placebo) test parameters in each model, for a total of 176 tests, we might expect a few (five percent at the 5 percent level) to be statistically significant due to random chance, even if our common trends assumption was valid for all models.

Looking at these placebo test parameters we did find some significant results. Of our 176 placebo test parameters, 14 (or 8 percent) were statistically significant at the 5 percent level, not far outside our expected result of 5 percent of significant results. Seven of these 14 significant results were significant at the 1 percent level, with many having p-values very close to zero.

Most of these highly significant results (six of the seven) relate to estimated impacts on paid parental leave income of the placebo tests in 2016 and 2017. Although these are statistically significant, they are much smaller than those estimated for 2018. For all mothers/first parents, estimated increases were around $210 in 2016 and $180 in 2017, compared to a much larger increase of around $1,320 in 2018. Estimates were also significant, and of slightly larger magnitude, for mothers/first parents not on benefit and

estimated to be eligible for paid parental leave, and for mothers/first parents of non-Māori and non-Pacific ethnicity.

These placebo estimates are unable to be accounted for by previous extensions to paid parental leave in the study period, which came into effect in April 2015 and 2016, increasing from 14 weeks to 16 weeks on the first date, and then to 18 weeks on the second. These increases impacted on both the pre-1 July and post-1 July cohorts equally.

However, in addition to these extensions, maximum payment values are adjusted on 1 July each year, in line with wage growth. These annual increases, which affect incomes for the post-1 July cohort to a greater degree than the pre-1 July cohort, plausibly explain the apparent positive placebo test estimates we observe for paid parental leave income. The annual adjustment of paid parental leave maximum payment rates may also contribute to some of our estimated impact of the 2018 Families Package early-years changes. The annual adjustment that occurred at the same time as the Families Package changes was much larger than in previous years.[22] Therefore, these tests plausibly pick up real rather than placebo policy changes, and strengthen the case for viewing the 2018 paid parental leave estimates as causal.

The final highly significant result was related to a small increase of $60 in fathers'/second parents' average incomes from benefits in 2016. Overall, we conclude that the common trends assumption holds and statistically significant estimated impacts can therefore be interpreted as causal.

Further robustness tests - estimating impacts on maternal characteristics

We also ran a series of models estimating post-1 July 2018 treatment effects using a range of maternal characteristics. Under the common trends assumption, we would expect differences in these characteristics within birth years to be similar, and our estimation should not uncover significant effects. These effects could signal changes in the underlying characteristics of our population across the pre-1 July and post-1 July periods, suggesting a possible violation of the common trends assumption. We ran models estimating treatment effects on indicator variables with different ages of mothers/first parents, ethnic groups, and whether the child was a first baby.

Most results were very small and non-significant. The one exception was a small almost statistically significant estimated effect of less than a percentage point on the proportion of mothers/first parents of Māori ethnicity (p-value of a little over 0.05). This is unlikely to be of major concern in our modelling but suggests additional caution should be taken with results that are close to the 5 percent level of significance.

Sensitivity tests

A range of other sensitivity tests were conducted. In all cases results were not substantively altered.

- We examined mother-child relationship as recorded in maternity data rather than as recorded on birth certificate. No substantive difference in results was found. This tells us results are not likely to be influenced by some very late birth registrations being absent from the birth registration data available to us.

[22] The increase on 1 July 2018 was $25.83 compared with $12.75, $10.87 and $10.83 on 1 July 2015, 2016 and 2017 respectively, reflecting the varying pace of earnings growth over the period. See McKenzie (2020).

- We examined mother-child dyads supported by benefit as recorded in benefit data rather than as recorded on birth certificate (benefit data have the advantage of providing information on who cares for the child once born, which may be different to the parent/s listed on the birth registration). No substantive difference in results was found.
- In future years, we hope to include post-implementation comparison years in our analysis as well as pre-implementation years. This would provide additional assurance that our results are not being affected by changing trends over the time period of our study. At this stage, data was only available to do this for the number of months spent with no wages and salaries in the six months post-birth. We re-ran this model using 2019 as an additional control year. This resulted in an almost identical estimate of 0.063 additional months worked by mothers/first parents over the six-month period for the cohort born post-1 July 2018.
- Given the small size of estimated birth shifting effects we do not expect that the inclusion of June and July births will compromise the robustness of the study. Nevertheless, we tested the sensitivity of our results to the exclusion of these months' births and the results were almost identical to our main results. This also provided confidence that results would not be substantively different if we modified the analysis to take account of the fact that families could qualify for Best Start if their baby was born on or after 1 July, or due on or after 1 July 2018 but born earlier.
- We also tested whether results were sensitive to the inclusion of September births, which could be affected by announcement of the full details of the Families Package influencing conceptions and again found no significant differences between the two sets of results.[23] If conception effects began immediately after the first government announcement of what was to become the Families Package, the effects could also be seen in August births. However, this seems highly unlikely given the lack of media attention around the Families Package, and the general nature of the early announcement. Further research could explore possible effects on conceptions, and examine the distribution, mechanisms and possible health consequences of the small birth shifting effects estimated here. Existing international evidence on these effects is sparse (Momsen, 2021b).

[23] At the time of writing, we were unable to examine possible conception effects. While birth shifting effects are expected to be localised in time around the Families Package implementation date (see Deutscher and Breunig, 2018, for an example), conception effects may be more subtle, and spread over time. This is considerably more challenging to identify, as gradual changes are difficult to separate from broader secular trends that may occur at around the same time. Our situation is further complicated by the fact that births are not always registered immediately in New Zealand, with some births registered a year or longer after the birth.

6. Discussion

This report documents the changes the 2018 Families Package made to early-years social assistance entitlements, and demonstrates use of difference-in-differences techniques to estimate the additional increase in income parents gained as a result of the early-years changes and causal impacts on administratively recorded outcomes.

Our discussion focusses on the following high-level findings:

- even without the early-years changes, increases in income occurred for families with children born in 2018 overall, consistent with the broader Families Package changes and increased employment income
- for mothers/first parents, being in the first cohort eligible for the early-years changes is estimated to have resulted in additional income gains (over and above the broader Families Package income gains) that averaged $1,400 in the first six months
- for fathers/second parents in the first cohort eligible for the early-years changes, resulting additional income gains averaged $450 in the first six months, but the difference was not statistically significant
- estimated additional income gains as a result of being in the first cohort eligible for the early-years changes were:
    - largest for mothers/first parents eligible for paid parental leave
    - broadly similar across Māori, Pacific, and non-Māori, non-Pacific mothers/first parents
    - partially offset by the loss of some other financial assistance payments, particularly for lower income families
- mothers/first parents eligible for paid parental leave are estimated to have spent more time off work in their child's first year as a result of being in the first cohort eligible for the early-years changes
- results remain robust after taking into account the possible influence of small birth shifting effects.

Even without the early-years changes, increases in income occurred for families with children born in 2018 overall, consistent with the broader Families Package changes and increased employment income

While the primary focus of this study is on the additional gains for families who were the first eligible for the Families Package early-years changes, it is important to recognise that they also benefited from the broader income gains resulting from other parts of the Families Package.

The scale of these gains is highlighted by descriptive data for the three-month cohorts born pre-1 July each year. These show particularly large increases in the incomes of parents with new-borns between 2017 and 2018. Overall, average income was $1,900 ($74 per week) higher in the six months following the birth for mothers/first parents in the 2018 birth cohort than for mothers/first parents in the 2017 cohort.

A key driver was more Family Tax Credit and Accommodation Supplement payments than earlier cohorts, consistent with the Families Package increases to these payments. Another

contributor was higher employment income and higher income from paid parental leave post-birth than earlier cohorts.

Mothers/first parents in the pre-1 July cohort who were receiving a benefit in the month before the birth had particularly large gains in income comparing 2017 and 2018, with incomes being almost $2,900 ($110 per week) higher in the six months following the birth for the 2018 birth cohort than for the 2017 cohort.

Around half of this increase was due to higher income from Working for Families tax credits, consistent with increases made by the Families Package. Higher Accommodation Supplement and core benefits also contributed, as did the introduction of the Winter Energy Payment.

For mothers/first parents, being in the first cohort eligible for the early-years changes is estimated to have resulted in additional income gains (over and above the broader Families Package income gains) that averaged $1,400 in the first six months

Turning to the difference-in-difference estimates of the causal additional difference made to incomes by the early-years changes in the Families Package, overall we estimate that being in the three-month birth cohort that was the first to be eligible for Best Start and the extension to paid parental leave increased mothers/first parents' incomes by an average of over $1,400 in the first six months after the birth of their child ($55 per week), equivalent to a 10 percent increase in income over the period.

The increase was mainly accounted for by an increase in paid parental leave and Best Start, with some offsets as a result of reductions in other payments and reduced employment income.

Beyond the time period able to be covered by this study, the dollar value of the positive impact on income will have continued to grow as mothers/first parents who received paid parental leave in the first six months become eligible for Best Start for the remainder of their child's first year of life, and as low-and middle-income families have continued to receive Best Start until their child turns three. This will provide an opportunity for future research on the causal impacts of increased financial assistance in the Aotearoa New Zealand context.

Future research will complement results expected to flow from a large randomised controlled trial currently underway in the United States.[24] In the 'Baby's First Years' study, 1,000 mothers of infants with incomes below the federal poverty line receive monthly cash gift payments by debit card for the first 40 months of the child's life. Parents in the experimental group receive $US333 per month ($US4,000 per year), whereas parents in the comparison group receive a nominal monthly payment of $US20. These income gains are larger than those provided by the Families Package early-years changes, and this major study will make an important contribution to the evidence base by using direct measurement, observation, and interviews with parents to explore impacts on children's cognition, health and development, and by examining causal pathways.

Research focussed on the Families Package, and using existing data sources, will not have the same richness, but will be able to provide a long-term follow-up on outcomes such as educational participation and attainment and health service usage. However, future

[24] More information is available from https://www.babysfirstyears.com/ and https://clinicaltrials.gov/ct2/show/NCT03593356.

research could include some of the features of the 'Baby's First Years' study through additional data collection. Surveying families in the cohorts could be a vehicle for, for example, building understanding of causal pathways and impacts on culturally informed measures of wellbeing.

For fathers/second parents in the first cohort eligible for the early-years changes, resulting additional income gains averaged $450 in the first six months, but the difference was not statistically significant

The increase in income for fathers/second parents is not estimated to be statistically significant. The small but statistically significant increase in Best Start tax credit income for fathers/second parents highlights that for the most part these payments are received by the mother, and as such, our estimated absolute dollar gains in income for mothers/first parents are likely to largely represent the extent of the gains in family incomes also.

Estimated additional income gains as a result of being in the first cohort eligible for the early-years changes were largest for mothers/first parents eligible for paid parental leave

In the first six months post-birth, the mothers/first parents with the potential to gain most from the Families Package early-years changes were those who qualified for the extension of paid parental leave (Table 2). This was reflected in our results.

Mothers/first parents eligible for paid parental leave received an estimated average of $1,900 in additional income in the six months after the birth ($72 per week) as a result of the early-years changes, representing an 11 percent increase over and above their expected income without the early-years changes.

While mothers/first parents supported by benefit in the month before the birth received the largest gains from other parts of the Families Package, they had the smallest estimated additional average increase in income as a result of the early-years changes in the first six months post-birth, an additional $820 on average ($31 per week), representing a five percent increase over and above their expected income without the early-years changes.

Mothers/first parents not eligible for paid parental leave received an estimated $1,040 in additional income in the six months after the birth on average ($40 per week) as a result of the changes. This group tended to comprise mothers/first parents with little recent employment prior to having the child, who did not qualify for a main benefit due to their partner having earnings. The group had a comparatively high representation of families who did not meet residence requirements for Best Start and other Working for Families tax credits (making up eight percent of the group). The $1,040 average income gain represented a large relative increase in their personal income of 19 percent (but because many are likely to have a working partner, the percentage increase is likely to represent a smaller share of their family income).

Estimated additional income gains as a result of being in the first cohort eligible for the early-years changes were broadly similar across Māori, Pacific, and non-Māori, non-Pacific mothers/first parents

Mothers/first parents from different ethnic groups had different probabilities of being in different financial situations, with Māori and Pacific mothers/first parents being more likely than other mothers/first parents to be supported by benefit before the birth of their child,

and less likely to be eligible for paid parental leave. This impacted on the way and extent to which these different groups benefited from the Families Package.

Estimated income gains ranged from around $1,310 ($50 per week) for Pacific mothers/first parents on average, to more than $1,460 ($56 per week) for Non-Māori, non-Pacific mothers/first parents on average. Māori mothers/first parents gained an average of around $1,440 ($55 per week) in total income. These increases reflected estimated income gains of 9.3, 10.5 and 9.3 percent respectively. Partnered parents are the dominant family form for infants across all ethnic groups. But Māori and to a lesser extent Pacific mothers/first parents with infants are more likely than Non-Māori, non-Pacific mothers/first parents to be in a sole parent family (MSD, 2018a, p. 36). As a result, we expect these income gains were larger as a proportion of total family income for Māori and Pacific women.

While the majority of additional income for non-Māori, non-Pacific mothers/first parents came from increases in paid parental leave, Māori and Pacific mothers/first parents benefitted equally from both paid parental leave and Best Start.

Estimated additional income gains as a result of being in the first cohort eligible for the early-years changes were partially offset by the loss of some other financial assistance payments, particularly for lower income families

Offsets occurred due to the loss of income from some other payments:

- low- and middle-income families not on benefit and not receiving paid parental leave lost access to a Parental Tax Credit which had provided $200 per week in the first 10 weeks of the child's life – this payment was discontinued when Best Start was introduced (around 14 percent of earlier study cohorts had received Parental Tax Credit in the six months post-birth)
- some families on benefit or low income lost Temporary Additional Support – this is a payment of last resort that is withdrawn dollar-for-dollar as income from other sources, including Best Start and other Working for Families tax credits, increases
- some families appear to have also lost Accommodation Supplement income – reasons for this are less clear, but gains in income from other sources may have reduced the likelihood that families went through the claims process for this payment, and gains in income from paid parental leave and increased employment income of fathers/second parents (while not statistically significant) may have reduced Accommodation Supplement entitlement.

The result was more modest additional income gains from the early-years changes (over and above income gains from other parts of the Families Package) in the first six months for some of the families for whom increased financial assistance around the time of birth might have resulted in the greatest impacts on wellbeing.

A study that looked at example families and budgets after the Families Package was undertaken to inform the work of the Welfare Expert Advisory Group (Welfare Expert Advisory Group, 2019a). The study found that even with the other parts of the Families Package in place (but not taking into account the early-years changes), for families receiving benefits and in low-wage work, income was inadequate to meet estimated 'core' and 'participation' expenditure needs.

In the years leading up to and immediately following the Families Package reform, a series of studies asked families and whānau on low incomes about their experiences when accessing social assistance payments. Many reported that the level of financial assistance provided through the benefit system was inadequate. While in receipt of a benefit, families and whānau continued to experience significant poverty and hardship and reported that their income did not cover basic living expenses. Reliance on family and friends or credit lenders for financial support could strain relationships and increase problem debt. Some interviewed in the studies were unaware of entitlements, and some with high need for support had come to avoid engaging with agencies involved in the delivery of financial assistance (Auckland Action Against Poverty, 2017; Baker, Williams, & Tuuta, 2012; Cram, Adcock, O'Brien, & Lawton, 2019; Ministry for Women, 2018; Welfare Expert Advisory Group, 2018; Momsen, 2021a).

These insights raise the possibility that, for some families, until income meets at least core needs, income gains through increased financial assistance may not flow through to marked improvements in wellbeing through either the investment or the family stress pathway. We estimated a $31 average weekly income gain for mothers/first parents on benefit as a result of the early-years changes. These gains were not included in the Welfare Expert Advisory Group (2019a) calculations. Addition of $31 per week to their calculations would still leave deficits between 2018 current incomes and core and participation expenditure levels for some example families. We note that the picture will have changed for more recent cohorts with increased assistance made available in 2020 as part of a Budget 2019 package of changes and the initial response to the COVID-19 pandemic (see Arnesen, 2021).

Insights from qualitative studies also suggest that for some families, increases in debt servicing, lack of awareness of or access to entitlements, or difficulties claiming may limit the degree to which increases in social assistance entitlements flow through to improved living standards, and to improvements in the wellbeing of children. Further study to estimate the scale of impacts on child wellbeing will be an important next step.

Mothers/first parents eligible for extended paid parental leave are estimated to have spent more time off work in their child's first year as a result of being in the first cohort eligible for the early-years changes

For the sub-group of mothers/first parents estimated to be eligible for paid parental leave (and not on benefit before the birth), the Families Package early-years changes increased time off work post-birth, consistent with the policy intent. These mothers/first parents spent seven months with no wages and salaries in the first 12 months post-birth on average, and being in the first cohort to qualify for the Families Package was estimated to increase this time by 0.21 of a month, close to a week, on average.

The number of months in the first six months post-birth that mothers/first parents had no earnings from wages and salaries increased by 0.12 of a month, or around four days (2.8 percent of expected levels in the absence of reform). Wages and salaries reduced by $780. This amount equates to 4.6 percent of expected income over the six months post-birth in the absence of reform, and 35 percent of the $2,255 maximum possible income gain as a result of the four-week extension in paid parental leave.

We found no significant effects on fathers'/second parents' months with no earnings.

These results are in line with international evidence that policies that lengthen the duration of paid parental leave entitlement are accompanied by increases in mothers'

leave-taking and longer durations of leave (Nandi et al., 2018). The size of the effect is small relative to the number of additional weeks of paid parental leave provided by the policy. One possible explanation is that recent inflation in house prices and rents worked in opposition to the policy reform, and constrained the amount of leave working parents were financially able to take. Another is that mothers/first parents' leave taking is influenced by concerns that employment opportunities and career pathways may be negatively impacted, and by employer preferences and practices (Costantini, Dickert, Sartori, & Ceschi, 2020; Nowak, Naude, & Thomas, 2013). Attitudes and practices may have still been adjusting in the period we focus on. Qualitative research would be useful in this area.

Findings from the Growing Up in New Zealand (GUiNZ) longitudinal study predate the Families Package but suggest financial constraints are an important factor. At their ante-natal interview, 95 percent of the GUiNZ mothers who were working said they intended to take parental leave. The average anticipated leave period was eight to nine months (this is longer than the seven months mothers/first parents estimated to be eligible for paid parental leave spent without wages and salaries in the first year post birth on average found in this study – see Figure 6). However, when interviewed when the child was around nine months old, only 30 percent of mothers who took some form of leave remained on leave (Morton et al., 2012). Almost three-quarters of the mothers who were working when their child was nine months old reported needing the money as a reason for why they returned to work before their child reached nine months (Peterson et al., 2018).

Most mothers in the GUINZ study used a combination of different types of leave to be home with their babies in the first few months after their child's birth. Close to nine in ten of the mothers who took leave received paid parental leave, half took unpaid leave, and one-third used annual leave (Morton et al., 2012). This suggests that increased weeks in receipt of paid parental leave would not necessarily be matched by reduced weeks in paid employment. Instead, some of the effect may have been to reduce use of unpaid leave or annual leave.

Based on overseas studies, more generous paid leave policies, up to a point, have the potential to increase women's labour force participation, employment, and job retention over the longer-term, and could increase longer-term wages and income (Nandi et al., 2018). However, at least one United States reform has had the opposite effect, suggesting that the institutional context is important (Bailey, Byker, Patel, & Ramnath, 2019). Examining whether employment and earnings were higher over the longer term, and whether the 'parenthood penalty' experienced by Aotearoa New Zealand women (Sin, Dasgupta, & Pacheco, 2018) was reduced by the Families Package early-years changes could be useful areas for further research.

Existing international literature suggests that supporting increased time off work following the birth of a child is likely to have flow-on health benefits, with strong evidence of a reduction in infant mortality (Heymann et al., 2017; Nandi et al., 2018), and emerging evidence of improvements in mothers' mental health and wellbeing (Doran et al., 2020; Lee et al., 2020). Pathways by which increased maternal time off work following the birth might influence outcomes are complex (D'Inverno, Reidy, & Kearns, 2018). Further research examining the impact of the Families Package changes on these outcomes, to the extent possible with the available data and given the small scale of the estimated leave taking response, could provide useful additions to the evidence base.

Across ethnic groups, the effect on months with no wages and salaries was only statistically for non-Māori, non-Pacific mothers/first parents, consistent with the larger

average income gains from extended paid parental leave for these mothers/first parents. Because prior to the early-years reform non-Māori, non-Pacific mothers/first parents had less time with no wages and salaries post-birth on average, the result appears to be a reduction in ethnic differences in time mothers/first parents spend at home with an infant after the birth.

Results remain robust after taking into account the possible influence of small birth shifting effects

We began the present study by using several different approaches, and two independent data sources, to estimate how many births may have been shifted from June 2018, before the implementation of the Families Package, to July 2018, after its implementation. The results were remarkably consistent. There appear to have been around 80, and at most around 120, births shifted following the introduction of the Families Package in July 2018. This represents around 1.5 to 2.5 percent of the 5,000 typical monthly births in New Zealand. There is potential for further research to explore whether birth shifting occurred more frequently in particular demographic groups, but given the small size of the overall effect, this was not needed for our study.

In our central models we selected treatment and comparison groups over three-month birth windows around the introduction of the package, and we would expect birth shifting to only have affected around half a percent of these groups. Even if shifted births were an extremely biased group, this would only have the potential to have a very small effect on the impact estimation.

After the Australian Baby Bonus was introduced in 2004, it was estimated that six percent of the babies who would have been born in the month prior to the reform had births shifted to the post-reform period (Gans & Leigh, 2009). Our estimate is a third of the Australian estimate. This is consistent with our a priori expectation that the Families Package would be likely to result in more muted birth shifting effects than occurred in Australia for several reasons. While the payments were similar in magnitude on an annual basis, the Aotearoa New Zealand payment was made in instalments or at the end of the tax year, rather than in a single upfront lump sum, presenting an arguably smaller financial incentive overall. Secondly, unlike in Australia, all babies due to be born after the Family Package's introduction were eligible for the payment, regardless of when the baby was born. Thirdly, in Australia, delaying the birth occurred more frequently in higher socio-economic groups which may reflect greater private health coverage (Gans & Leigh, 2009) and a greater ability for parents to influence birth timing. The predominance of publicly-funded maternity care in Aotearoa New Zealand may have meant parents had less scope to influence birth timing.

Finally, the Australian payment was announced not long before its introduction and appears to have been subject to considerable media attention, in particular regarding perverse incentives to delay births. The announcement of the package in Aotearoa New Zealand was made much earlier, in the context of a wider suite of changes, and there was little media commentary about Best Start at the time it was introduced. As a result, parental awareness of the financial incentive to delay births may have been much lower than in Australia.

7. Strengths and limitations

IDI data are a new resource for building evidence about what works (Connelly et al., 2016; Milne et al., 2019), but there is a need for greater transparency about their existence, use, and limitations (Gulliver et al., 2018). This report demonstrates the application of quasi-experimental methods to IDI data to help begin to address an important policy question – what difference does increased financial assistance make to children and their families?

A strength of this study is that it exploits a change in entitlements which occurred in such a way as to create a natural experiment, and it uses a control group – earlier years' cohorts with births either side of 1 July – who might be expected to be otherwise similar but unaffected by the change. This offers greater confidence that we are identifying the causal effects of the Families Package itself, so long as we can be assured that there were 'common trends' in outcomes for those with births either side of 1 July before the Families Package.

In this initial study we focus on families with children born in 2015 to 2017 as the control group for the estimation of impacts on employment and incomes. Our results provide some reassurance of common trends prior to the reform, but it is important to note that the policy environment was far from static in these years (McKenzie, 2019; Department of Prime Minister and Cabinet, 2019). Updates and extensions of this study will be able to include 2019 and later births as additional controls once data becomes available. This will provide a much more robust test of whether there were common trends before 2018 that would have continued in later years. However, ongoing consideration will need to be given to whether other events, like the 2020 COVID-19 pandemic and response, might prevent the use of some years' data.

Basing the study on linked administrative data in the IDI has the strength of drawing on a longitudinal data source unaffected by non-response bias, and with national coverage and the universe of Families Package recipients. This allowed a focus on a narrowly defined population – new-borns and their parents – and allowed examination of important sub-groups within this population in a way that would not be possible using survey data. In future studies, it will also allow examination of a range of outcomes, such as parental employment and incomes, and children's potentially avoidable hospitalisations, school attendance and social service contact, and longer-term educational participation and attainment.

Against these strengths, several limitations need to be borne in mind. IDI data linking is generally probabilistic. Some errors and missed links are inevitable in this process (Milne et al., 2019). The IDI data used in this study was information collected or generated in the process of administering services, and inevitably will also embody any errors in measurement, reporting and recording that occur in those processes.

In addition, administrative data may not always provide a good proxy for the underlying outcomes of interest. Here we infer parents' time out of employment post-birth from the presence or absence of employee earnings recorded for tax purposes in each month, but don't capture employment in the informal economy, and had insufficient follow-up at the time of writing to comprehensively consider self-employment income. In addition, the simple count of months with no wages and salaries we use as a proxy may not be sufficiently sensitive to identify increased time not in paid employment, and alternative specifications could be explored in future research.

The limitations of the administrative data available to us mean that we are unable to examine *family* incomes – we are only able to examine the separate, individual, incomes of parents. While family relationship data is collected by Inland Revenue in administering the tax credit system, policy changes over time mean that it is not possible to identify families on a consistent basis over time – relationships are more likely to be able to be identified after the Families Package reform because more families would have registered for Working for Families in order to receive Best Start. For this reason, we were unable to use Inland Revenue family relationship data to explore impacts on family incomes.

Data limitations at the time of writing also mean that we are required to impute the flow of Working for Families tax credit income across the year. As a result of this approach we overstate actual within-year incomes (by assuming that end of year lumpsum payments were received in the periods during which entitlements fell).[25] We also take no account of recouping of overpayments.

A further limitation of reliance on the IDI is that that administrative data does not enable us to look at a range of outcomes of importance to Māori and Pacific communities. At the time of writing, administrative data in the IDI offered no measures of whānau, or whānau wellbeing, for example (Kukutai, Sporle, & Roskruge, 2017).

The nature of the research opportunity we examine means that we will be unable to say whether impacts changed over time as families learned more about the changes in entitlements – we examine only those with births immediately post-implementation, and arguably this group had little time to learn about and adapt to the reform. Lags in employers adjusting expectations about the time to return to work, or in awareness of the changes in paid parental leave, may mean the small impact on months with no wages or salaries post-birth found for this cohort may not be generalisable to later cohorts.

More extensive analysis could be applied. For example, future analysis could usefully compare impacts for families with first-born children with impacts for families having second or subsequent children, and take into account exposure to increased Families Package income in the ante-natal period. On the one hand, effects may be larger for families with first-born children because the early-years entitlements – extended paid parental leave and Best Start – were paid at a flat rate, meaning their equivalised value in the post-natal period was highest in one-child families. On the other hand, parents' income gains in the ante-natal period from increased Family Tax Credit entitlements for older children may mean effects on measures of wellbeing are larger for second or subsequent children.

Adjustment for consumer price index changes will be a useful enhancement in planned extensions of this study. However, it would make only a very slight difference to the figures presented in this report. Price inflation over the period was low, and the outcome periods of for the impact estimates that are the main focus of this report are within three months of each other on average. For descriptive estimates presented comparing 2017 and 2018 cohorts, outcome periods are just one year apart.

[25] A particular concern with this approach could be that we overstate within-year receipt of income. We note that most of the families we expect to be the most responsive to increased income receive Working for Families tax credits as weekly or fortnightly payments within the year together with main benefits, and are unaffected by our treatment of end of year Working for Families income as being received in the period in which the entitlement falls.

Only one main analytical approach is applied, and others could be explored. Estimation could be enhanced by adding estimation of the effect of having a birth in 2018 and qualifying for other income gains from the Families Package (while noting that it is the early-years changes that offer the most persuasive basis for causal inference). Other quasi-experimental control groups could also be examined, such as families who did not meet residency requirements for Best Start and other Working for Families tax credits, or families with high levels of past reliance on Temporary Additional Support. Both these groups would be expected to have received more limited income gains from the Families Package. Regression discontinuity methods could be applied in studies undertaken outside of the IDI if data that includes full birth date is available for analysis.

Another important limitation to note is that this study, and further analysis using the approach taken here, is unable to cast much light on causal pathways, given the limits of administrative data and given that it is unlikely to be feasible to use existing survey data when focussing on narrowly defined birth cohorts.

Finally, the nature of the quasi-experimental 'treatment' we consider is complex. Entitlements clearly changed in a way that provided a boost to incomes, but variation in entitlements across groups and the complex interaction of eligibility rules, administrative practice and take-up behaviours for different payments means that, unlike the United States Baby's First Years study, it is not possible to describe the size of the income gain in straightforward terms. We refer to the 'estimated impact of the introduction of the Families Package early-years changes' as a short-hand for a complex set of differences.

8. Conclusion and future research opportunities

The 2018 implementation of the Families Package offers a unique natural experiment that can be used to examine the impact of increasing financial assistance for families. While most components of the Families Package were available to all parents who met eligibility criteria regardless of the timing of their children's births, the additional increases in income from strengthened early-years entitlements (the new Best Start tax credit and extension of paid parental leave) were only available to families with births after an implementation date.

We follow the first three-month cohort to qualify for the early-years changes for six to 12 months. Using de-identified linked administrative data held in the IDI and difference-in-differences techniques, we estimate that the Families Package early-years changes increased mothers/first parents' incomes by $1,400 (10 percent) in the first six months post-birth.

While mothers/first parents supported by benefit had the largest increase in average income from other parts of the Families Package, additional gains from the early-years changes in the short follow-up to date were largest for mothers/first parents not supported by benefit who qualified for paid parental leave, and smaller for mothers/first parents supported by benefit. In time, gains are expected to be greatest for mothers/first parents receiving benefit and other low-income mothers/first parents.

We estimate that the early-years changes had a positive impact on time mothers/first parents eligible for paid parental leave spent off work in their child's first year of life and reduced their earnings, as intended by the policy. The effect was small relative to the number of additional weeks of paid parental leave provided by the policy. One possible explanation for the small effect is that recent inflation in house prices and rents worked in opposition to the policy reform, and constrained the amount of leave working parents were financially able to take. Another is that parents and employers were still adjusting to the changes. Alternatively, our 'months with no wages and salaries' proxy may not be sufficiently sensitive to identify increased time not in paid employment.

Further research with a longer follow-up is required before drawing conclusions about the success of the early-years changes in achieving their aims. Low- and middle-income families are yet to receive the full amount of the additional income provided by Best Start, which after the first year can be received on an income tested basis up until a child's third birthday.

Our next study will extend the analysis to examine the increase in financial assistance in ante-natal period that occurred as a result of the Families Package, and estimate the combined effects of the increased ante-natal and post-natal financial assistance on children's health and wellbeing in their early years.

There is considerable scope for other research teams to also build on this initial study.

Further analysis to explore effects on parental leave taking and employment could be undertaken. This could include, for example, estimating impacts on an outcome that estimates the fraction of each month that is worked based on estimated hourly earnings, or estimates impacts on time to return to substantive earnings, disregarding months when small levels of earnings might be associated with 'keeping in touch days' worked within a period of paid parental leave. Longer-term effects on maternal employment and earnings will also be of policy and research interest.

Further research on impacts on children and their families with a longer follow-up and examining effects on a wider set of administratively recorded outcomes will be useful. Supplementing the research through additional data collection from families in the different cohorts could be contemplated. This could, for example, help build understanding of causal pathways, and impacts on self-reported and culturally informed measures of wellbeing.

This study focusses on the difference made by the *introduction* of the Families Package changes. Another area for future research is the impact of the complete *withdrawal* of Best Start when children turn three. This aspect of the payment's design balances a range of policy considerations and assumptions about parents' employment responses (Boston & Chapple, 2014), and generating empirical evidence on what actually occurs will be important.

Finally, the differences in entitlements for cohorts born in close proximity to one another provides an important opportunity to provide policy makers with evidence on the difference additional financial assistance provided through Best Start made to families with young children through the 2020 COVID-19 crisis and beyond.

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Appendix 1 – Birth shifting estimation method and detailed results

Method

We take two, independent, approaches to estimating the birth shifting effect following the introduction of the Best Start payment in New Zealand. In the first approach, we use aggregated time series data on births registered in New Zealand, by day of birth. Data was provided as an aggregated series by Statistics NZ. We take a similar approach to Gans and Leigh (2009), however, we run our regression models in a time series framework which explicitly accounts for temporal autocorrelation in the series.

Our second approach uses individual level maternity data held by Statistics NZ in the Integrated Data Infrastructure (IDI). Maternity data in the IDI does not identify day of birth, however, estimated due dates are identifiable, alongside month of birth. Unlike with the Australian payment, the Best Start payment's introduction in NZ only provided incentives for birth shifting of babies due to be born before 1 July 2018. Using daily due date data, coupled with our expectation that such incentives are likely to disproportionately affect babies due to be born in the last few days and weeks of June 2018, we model the probability of a baby being born after the month in which it is due, and whether June 2018 is significantly different from other months in this respect. Our two approaches are discussed in detail below.

Aggregated time series regression approach

A time series of births registered in New Zealand by day of birth covering the six-year period from 2013 to 2018 was sourced from Statistics NZ. In order to identify whether a significant birth shifting event occurred, it was necessary to account for the specific characteristics of this series in the regression model i.e.

- Autocorrelation – As is typical in time series, we do not expect observations to be independent of one another, with observations closer in time more likely to be of similar magnitude than observations observed at wider intervals. Typical regression-type analysis assumes errors are un-correlated, and as such, specific time series modelling methods need to be employed to account for autocorrelation.
- Weekly seasonality – Births are not randomly distributed through the week. Given many births are scheduled (i.e. voluntary caesareans and inductions), these births are more likely to occur on weekdays than at weekends and are more likely to occur on a Saturday than a Sunday. It is important these effects are taken account of in our modelling.
- Annual seasonality – As with days of the week, births are not uniformly distributed across the calendar year, with conceptions being more likely at specific times of the year, either due to planning decisions or other factors. This also needs to be considered in modelling daily birth data.
- Holiday effects – Similar to weekends, we expect that public holidays observed on weekdays might be associated with fewer births due to fewer planned caesareans and inductions being scheduled on those days.

Taking account of day of the week effects is particularly important in our analysis, since 1 July 2018 (the day the Families Package was introduced) was a Sunday, the day of the week in which births are lowest. We also treat one additional day as a holiday in our analysis. There was a nurse's strike held on 12 July 2018, less than two weeks after the

Families Package was introduced. This resulted in many operations being cancelled, and as such we would expect fewer births on this day.

In addition to the effects discussed above, there is a further complication in our analysis. While most births are registered in a timely manner, and as such birth registration data is largely complete for 2018, some babies are not registered until a year or longer after the actual birth. For this reason, our approach needs to account for a generally declining trend in the births series during 2018. Time series approaches are well-adapted to deal with this type of pattern.

Linear regression approach

Gans and Leigh (2009) undertook an analysis to understand birth shifting in Australia following the introduction of a similar payment, the 'Baby Bonus'. Using a long historical time series of daily births, their analysis focussed only on births across selected windows before and after the introduction of the payment and contrasted these against births in the same window in other years. A linear model was run with fixed effects to account for calendar year, day of year, and day of week differences. No specific accounting for temporal autocorrelation in the series was done, and seasonal effects were essentially assumed to be fixed over time. We replicate this approach, albeit with the shorter series available to us, but also take an alternative approach which explicitly accounts for temporal autocorrelation in the data and changing seasonality over time.

One potential further issue with this approach is that the lack of complete registrations data for late 2018 might influence our estimates – for example, July 2018 may have fewer births than we otherwise would have expected relative to June 2018 simply due to a greater number of late registrations not yet appearing in the data. This has the potential to bias our birth shifting estimates downwards to a small degree and is a further reason why this is not our preferred approach.

2 step ARIMA regression model

Our preferred time series approach involved a two-stage process to estimating the impact of birth shifting around the implementation of the Families Package. The first step consisted of running an exponential smoothing state space model with box-cox transformation, ARMA errors, and trend and seasonal components, as implemented through the TBATS package in R and described in De Livera, Hyndman & Snyder (2011). This approach allowed us to remove the complex (weekly and annual) seasonality from the data. The resulting de-seasonalised series was then run through a regression model with ARMA errors and regressors to account for holiday effects and to estimate the size of any birth shifting effects.

If we represent the number of births on day t as y_t, we first decompose the series into level (l_t), seasonal ($s_t^{(1)}$ and $s_t^{(2)}$), and error (η_t) components as in equation (1) below. As discussed above our error term, ε_t is modelled as an ARMA process, accounting for serial correlation. We then remove the annual and weekly seasonality from our original series to create a de-seasonalised series $\hat{y}_t$, as in equation (2).

$$y_t = l_t + s_t^{(1)} + s_t^{(2)} + \eta_t \quad (1)$$

$$\hat{y}_t = y_t - s_t^{(1)} - s_t^{(2)} \quad (2)$$

In the second stage our de-seasonalised series is modelled using a time series regression model with ARMA errors, as illustrated in equation (3). In this model $PreBS_{j,t}$ is an indicator variable which is set to 1 where t falls during window j prior to the introduction of the Best

Start payment, and 0 otherwise, while $PostBS_{j,t}$ is an indicator capturing the same period following the payment's implementation. As such, $\beta_{1,j}$ and $\beta_{2,j}$, in combination, capture the number of births we estimate to have been shifted from the pre-Families Package period to the post-introduction period. In the case of large-scale birth shifting having occurred during period j we expect the former to be significant and negative, while the latter is significant and positive.

Weekly and annual seasonality have already been removed from the series we are modelling, and we introduce a further control for public holidays. We allow η_t to be autocorrelated and model it as an ARMA process.

$$\hat{y}_t = \alpha + \beta_{1,j} PreBS_{j,t} + \beta_{2,j} PostBS_{j,t} + \gamma Hol_t + \eta_t \quad (3)$$

Alternative 1-stage time series approach

We also tried a more direct approach which involved modelling the weekly and annual seasonality in the data directly in the regression model. This was done by fitting a dynamic harmonic regression model with an ARMA error structure. The annual seasonality in the data was modelled using a Fourier series while weekly seasonality was modelled through the introduction of day of week fixed effects. While this approach is more direct, it has the disadvantage that the seasonal pattern is fixed across our series, while in our preferred approach seasonality is able to shift.

Individual level regression approach

We also estimated birth shifting using an alternative approach with unit record administrative maternity data, linked with other data sources in the Integrated Data Infrastructure (IDI). Parents of any baby due to be born on or after 1 July 2018 were eligible for the Best Start payment, regardless of whether the baby was born after 1 July or not. For this reason, the only people with a financial incentive to shift the birth of their child were parents with a baby due before 1 July. These parents would benefit from the Best Start payment only if their baby were born (or recorded as being born) after 1 July.

To investigate this, we examine whether babies who were due to be born in June 2018 were more likely to be recorded as being born late (in this case on or after 1 July 2018), than babies due to be born in other months. From the maternity data we have information on due dates (by day), and information on birth dates (by month). We expect that births are more likely to be shifted if they are close to the end of the month than if they are earlier in the month. We run a linear probability model as given in (4) below.

$$y_i = \alpha + \gamma_j mon_{i,j} + \delta_k yr_{i,k} + \beta mon_{i,June}.yr_{i,2018} + \sigma_m days_{i,m} + \lambda_n dow_{i,n} + \eta_p x_{i,p} + \varepsilon_i \quad (4)$$

In this model y_i is an indicator of whether baby i was born after the due month or not (set to 1 if they were born after the due month, or 0 otherwise), $mon_{i,j}$ is a set of indicator variables which are set to 1 if baby i was due to be born in month j and 0 otherwise, and $yr_{i,k}$ is an indicator variable which is set to 1 if child i was due to be born in year k. We include a further interaction term which is set to 1 if baby i was due to be born in June 2018, and 0 otherwise. The associated coefficient, β, is our estimate of birth shifting associated with the introduction of the Best Start payment. Finally, we include control dummy variables for the number of days the due date falls from the end of the calendar

month ($days_{i,m}$)[26] and the day of the week the end of the month falls on ($dow_{i,n}$).[27] To test birth shifting over different time windows we run the model including births due within progressively wider intervals before the end of the calendar month.

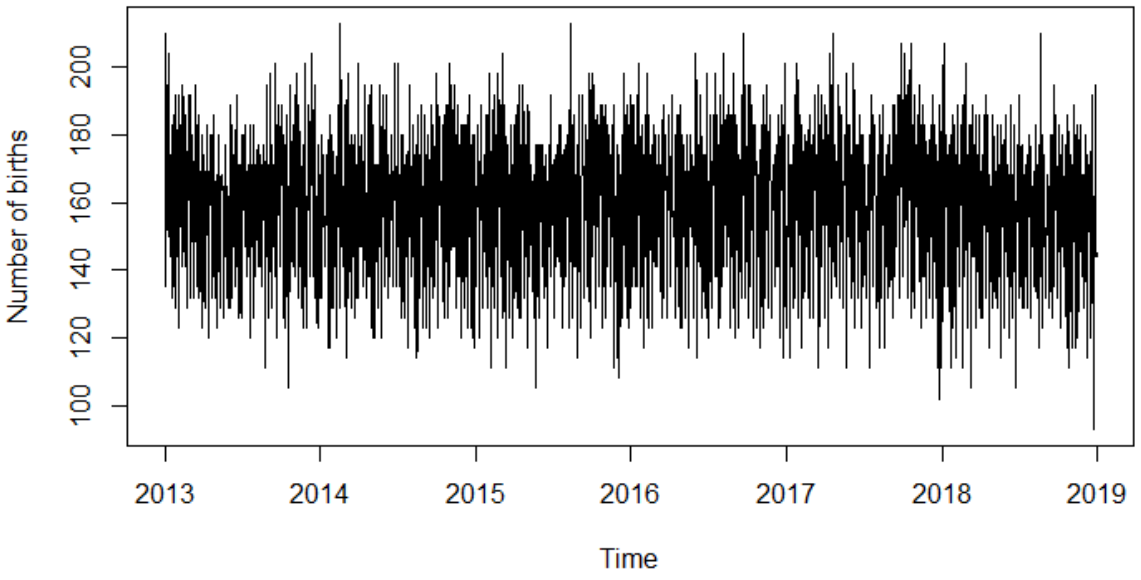
Other factors have also been shown to have an influence on the gestational length of a pregnancy, and whether a baby is born after the month in which it is due.[28] The ethnicity and age of the mother, the number of births the mother has previously had (parity), and the number of babies in the pregnancy (e.g. twins or triplets) all have been shown to have some influence on the term of a mother's pregnancy, as have health behaviours and socioeconomic factors. We run our estimation with and without a range of other controls ($x_{i,p}$) that have been shown to be associated with gestational length,[29] to test whether this has an influence on our estimated late birth parameter. Including such variables could be useful to both control for confounding trends, as well as to reduce the variance of our error term, which may reduce the standard errors of our estimated late birth effect, β.

Time series regression analysis results

Exploring the characteristics of the time series

The daily births time series from 2013 to 2018 is presented in Figure 1 below. We can see that there is considerable variation in the number of births from day to day, with most days' births falling between 120 and 190. Whilst we expect the series to contain seasonal patterns, the nature of these patterns is not obvious from the raw data series.

Figure 1 – Time series of registered births by day of birth, 2013 to 2018

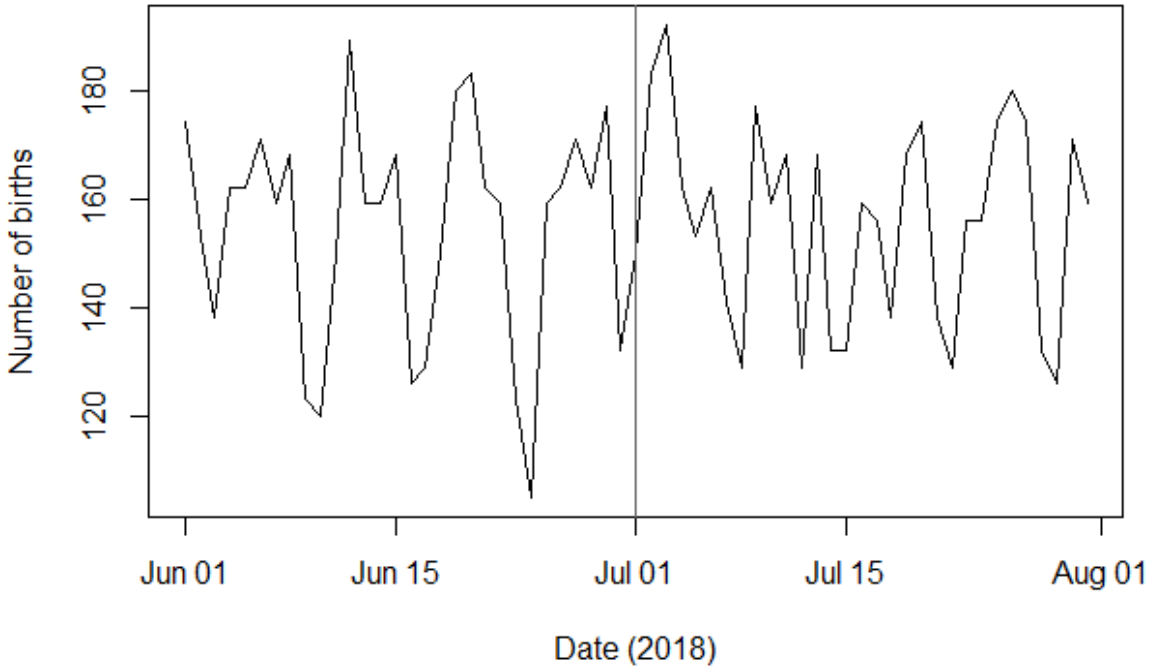


The period we are most interested in for this report is the month before and after the Families Package implementation date of 1 July 2018. In Figure 2 we show the number of

[26] For consistency across months, we only include babies with a due date in the last 28 days of each month. Given the low likelihood of births due early in the month to occur in the following month, this should not have any impact on our results. We further estimate whether birth shifting occurred for babies due to be born in only the last few days and weeks of the month, by excluding progressively earlier births.
[27] Given that fewer babies are born at weekends, we might expect the day of the week at the end of the due month to have some impact on the chance that a baby is born in the following month.
[28] See, for example, Mittendorf et al (1993).
[29] We include indicators of maternal ethnicity, parity, multiple births, BMI, smoking status and age in the model.

births over this period. At this level of detail, the weekly effect becomes much more obvious. A seven-daily repeating pattern is clearly observable, with higher birth numbers during the week and lower numbers at weekends. Unusually, Monday the 4th of June, which is the Queen's Birthday public holiday, does not have low births as we expected a priori. As expected, the 12th of July, the day of the nurse's strike, had fewer recorded births than any other weekday in June or July 2018.

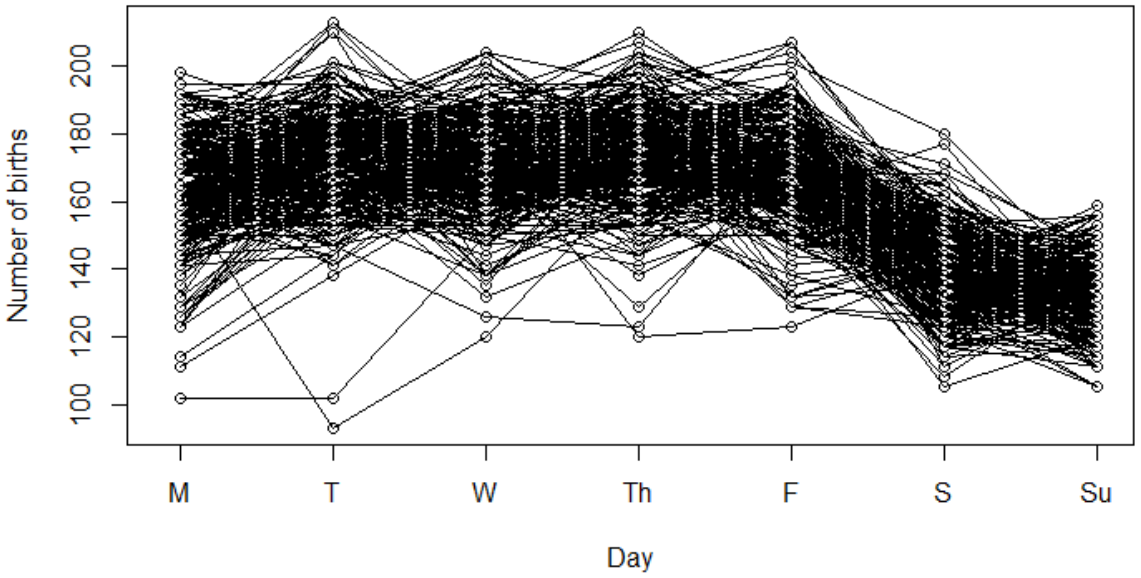
Figure 2 – Registered births by day of birth June to July 2018



Regarding potential birth shifting on or around 1 July, there are no obvious large-scale patterns observable, however, Monday 2 and Tuesday 3 July were 2 of the 4 days with the highest number of births during June and July 2018, while Sunday 24 June, the week before the package was implemented, recorded the 3rd lowest number of births of any day in 2018 (the lowest and next lowest being Christmas day and 11 March).

In Figure 3 we examine the weekly seasonal pattern in more detail. We plot all 313 weeks, from Monday to Sunday, showing a very clear pattern between weekdays and weekends. While weekdays occasionally record lower births than weekends, these are almost all public holidays. Annual seasonality is more difficult to see by eye in the raw data, however, the patterns become evident as we decompose the series in the next section.

Figure 3 – Registered births by weekday of birth, 2013 to 2018



Removing seasonality

As discussed above, the weekly and annual seasonality was removed from our data using the TBATS package in R. As illustrated in Figure 4 below, the weekly seasonal pattern did show some shifts over the course of our study. The model fitted to the data was of the form TBATS(0.995, {0,0}, -, {<7,2>, <365.25,7>}). The box-cox transformation parameter was 0.995, not far from 1, indicating little transformation was necessary; no AR, MA or damping parameters were required; and fourier series of length 7 and 2 were applied to model the annual (period of 365.25)[30] and weekly (period of 7) seasonality respectively.

While the peaks and troughs in annual seasonality are shown to be reasonably consistent year on year, there is some variation evident across time. For example, in earlier years the seasonal peak in late September and early October was less pronounced, as was the dip in early July. Differences in seasonality are also observed in March and April, possibly due to the movement of Easter during these months. These patterns are much smaller in magnitude, however, than the weekly seasonality.

Over most of the study period, births were most prevalent on Tuesdays, Wednesdays and Thursdays, however, Friday births became increasingly common in the last two to three years. Births on weekend days are clearly much less common than weekday births throughout the series, however, Saturday births became more common over time relative to Sunday births.

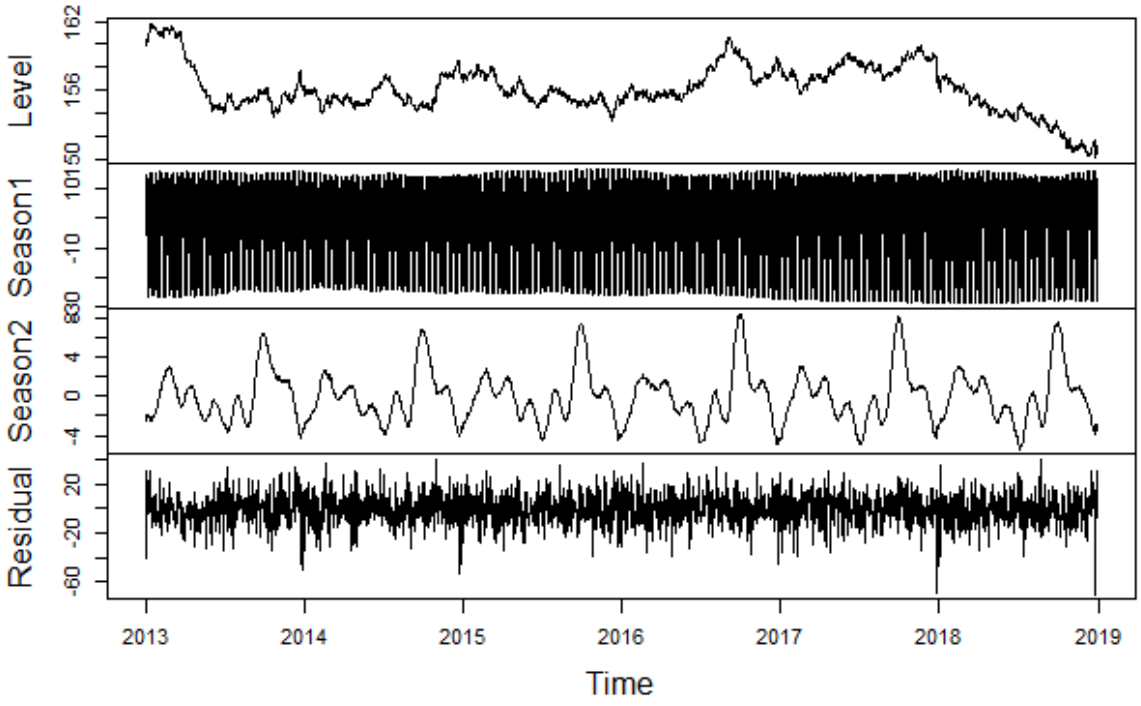
The level series picks up broader trends over time, and captures the effect of late registrations in 2018, as illustrated by the tailing off of the level series during 2018. The scale of level shifts is small however, relative to the size of the weekly seasonal effect and residual error term.

We calculated our de-seasonalised series, to be modelled in the subsequent step, by subtracting the two seasonal series from our original raw series. As such the de-seasonalised series captures both random fluctuations (including possible birth shifting

[30] Note that a period of 365.25 was used to account for leap years.

and other idiosyncratic effects), and level shifts (capturing long-term trends, and the effect of late registrations).

Figure 4 – Seasonal decomposition of daily birth registration data, 2013 to 2018



Birth shifting estimation

As discussed earlier, we tested three model specifications to estimate whether the introduction of the Families Package resulted in birth shifting. In the first approach we take the deseasonalised series constructed as described above and run it through a regression model with ARMA errors. We also introduce an additional control to account for public holidays, on which we expect to see fewer births.

Our second approach also involves running a regression with ARMA errors, but in this case, we run the model on the original series, and account for the annual seasonal pattern by fitting a fourier series. We also considered fitting weekly seasonality with a fourier series. While a 2-term series fit reasonably well, we finally adopted a series of dummy variables to account for days of the week, producing an improved fit. While this approach was simpler than our first approach, it had the disadvantage of being unable to account for changing seasonal patterns over time.

Finally, we adopted a similar approach to one of the specifications used by Gans and Leigh (2009) in their paper looking at the introduction of the Baby Bonus in Australia. This approach involved fitting a linear model with fixed effects to control for day, month, and year. As with our second approach, this model does not allow seasonal effects to change over time.[31] Additionally, the model does not account for autocorrelation in the series, while our relatively short data series could also be a concern in adopting this approach. In our case we also have increasingly incomplete registration data toward the end of our series, and this could introduce a negative bias on our results.

[31] The Gans and Leigh study used a very long series of daily data covering the thirty years from 1975 to 2004. The potential downside of such a long series is that seasonal patterns evident in earlier data may be less reliable predictors of more recent patterns.

Table 1, below, presents the results from our three models. We split the time before and after the introduction of the Families Package into progressively wider windows, beginning with the 3 days before and after its introduction, extending this to 1 week, 2 weeks, and finally 4 weeks. While we identify negative effects implying possible reductions in births both 4 to 7 days and 3 to 4 weeks before the package's implementation, none of these results are significant. The only significant effects we see to indicate that birth shifting occurred were in the first 3 days after the Families Package was introduced, with 81 estimated additional births in specification (1), and a confidence interval of 38 to 125. Our alternative specifications (2) and (3) produced slightly lower, but not inconsistent, estimates of 69 and 62 additional births respectively.

Table 1 – Regression models and birth shifting estimates of daily birth registration time series data, 2013 to 2018

Model specification		Two stage TBATS/RegARIMA (1)		RegARIMA with Fourier terms (2)		Linear model with fixed effects (3)	
Dependent variable		Deseasonalised daily births 2013-2018		Daily births 2013-2018		Daily births May-Aug 2013 to 2018	
		Model	Estimated shift	Model	Estimated shift	Model	Estimated shift
Pre-1 July	15-28 days	-1.272	-18	-2.027	-28	-1.756	-25
		[3.862]		[3.895]		[3.832]	
	8-14 days	-0.469	-3	0.587	4	0.755	5
		[5.120]		[5.146]		[5.175]	
	4-7 days	-3.291	-13	-4.675	-19	-6.029	-24
		[6.502]		[6.539]		[6.718]	
	1-3 days	0.526	2	1.542	5	3.600	11
		[7.394]		[7.459]		[7.707]	
Post-1 July	1-3 days	27.096**	81	22.863**	69	20.657**	62
		[7.395]		[7.459]		[7.708]	
	4-7 days	-1.867	-7	-4.568	-18	-4.722	-19
		[6.507]		[6.537]		[6.718]	
	8-14 days	3.188	22	1.088	8	0.457	3
		[5.128]		[5.155]		[5.230]	
	15-28 days	-1.009	-14	-0.339	-5	-0.403	-6
		[3.856]		[3.895]		[3.828]	
ARIMA error structure		(1,0,1)		(1,0,4)		n/a	
Observations*		2,191		2,191		738	
R-squared		n/a		n/a		0.71	
AIC		17244.1		17251.9		n/a	

Unusually, we do not identify any significant estimates of reduced birth numbers prior to the introduction of the programme to match the spike in numbers shortly after its introduction. There are at least two possible explanations for this finding. In the first scenario, the increase of births in early July 2018 could be the result of fraud, with births being recorded as occurring in early July even if they actually occurred anywhere from a few days to some months earlier. If these dates were spread over a sufficiently wide interval, it would become impossible to identify the reduction.

A second, more simple explanation could simply be that the underlying annual pattern of births was slightly different in 2018 than in earlier years, and this, combined with shifted

births being spread over a two-to-three week period, could have resulted in the model picking up only small, non-significant effects.

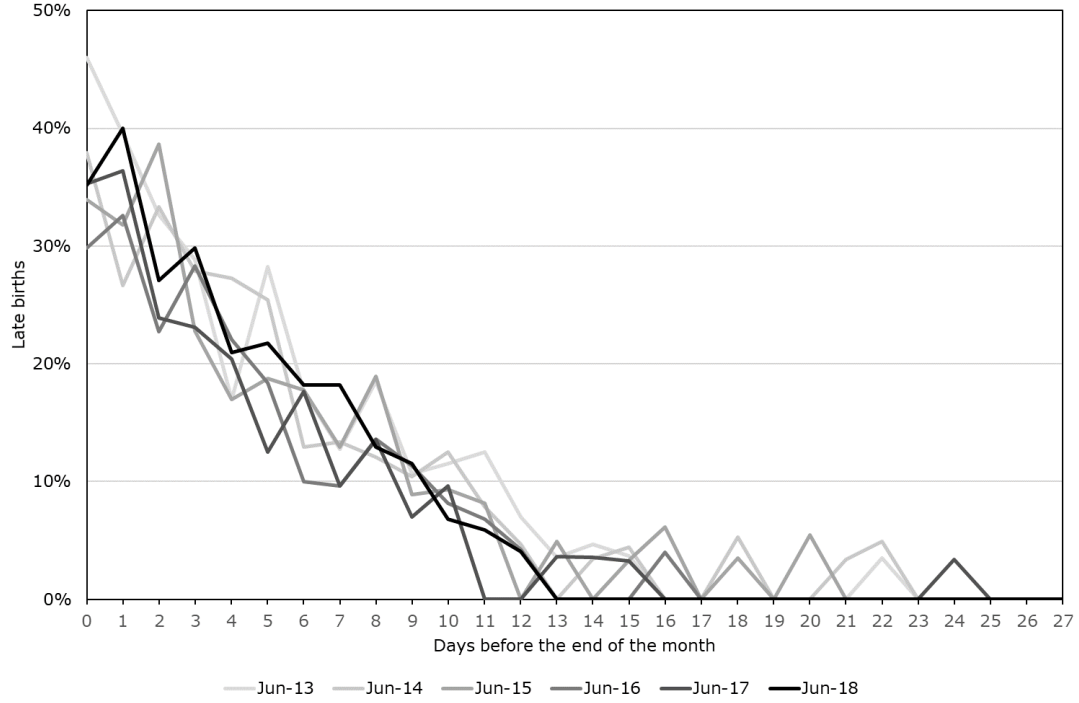
Individual level regression results

A regression analysis of daily time series data on registered births provided evidence of significant, but relatively small birth shifting effects. We were also able to access individual maternity records which included both due date and month of birth, as well as other information on the mother, the baby, and the birth. This information is sourced from data provided by the Lead Maternity Carer (LMC) and inpatient and day-patient health data sourced from the Ministry of Health's National Minimum Dataset (NMDS). We specify a linear probability model where we predict whether a birth due in a month actually occurs after the end of that month (a late birth). This is dependent on several factors, with the most important being how close the due date is to the end of the month. The model also has month and year effects, which we interact for June 2018 to test whether births in that month were more likely to be late than in other months, indicative of birth shifting due to the introduction of the Families Package.

Descriptive analysis of late births

Figure 5 shows the relationship between day of the month, here measured as days before the end of the month, and the probability that a baby is born late (in the month after its due date) for babies due in the last 28 days of June months from 2013 to 2018. As we would expect, the closer a due date is to the end of the month, the more likely the baby will be born after that month. Around 30 to 40 percent of babies due on the last day of the month are actually born after that month, while this declines until around two weeks before the end of the month. Fewer than 5 percent of births with due dates before these last two weeks are born late.

Figure 5 – Late births by day before the end of the month, June months, 2013 to 2018



Note: Dates with only a few late births are suppressed for confidentiality purposes and are shown as zeros in the graph. Where a result is at zero percent the underlying number may in fact be a small non-zero value.

Although at face value there is little evidence to indicate that June 2018 experienced a higher than usual proportion of late births, this needs to be seen in the context of a declining trend in late births in New Zealand over recent years. Just taking births due on the last three days of the month,[32] 33 percent of 2013 births were born late, however, this had dropped to 28 percent by 2018.

Some indicative evidence of a possible effect in June 2018 is provided by a closer examination of recent years' data in Figure 5. On all but one of the last 8 days of the month there were more late births than on the equivalent day in either 2017 or 2016.

Birth shifting estimation

Results from a series of regressions specified in this way are given in Panel A of Table 2. Specifications (1) to (4) include the control variables discussed above, while models (5) to (8) also include a number of other demographic control variables which we expect to have an influence on whether a baby is born later than its due date. In progressive models we widen the window in which we estimate birth shifting to occur in, excluding babies due to be born earlier in the month from the models.

The regression estimates on the impact of the Families Package on late births are then used to estimate birth shifting effects. We use our fitted model to predict the probability of each birth being late, in absence of any June 2018 effect. This is then compared with the actual observed number of late births in June 2018, to give us an estimate of the number of births shifted. These results are given in Panel B.

Although the additional demographic controls are clearly important in predicting whether a birth will be late (as indicated by the increase in the R squared values between the first and second set of models), they make little difference to our birth shifting estimates. In our model with all controls, 82 births were estimated to have been shifted (confidence interval from 44 to 119) among babies due to be born in the two weeks before the package's introduction. Slightly fewer births are estimated to have been shifted in the 28 day window than in the fourteen day window, providing no evidence that babies due before this two week period were shifted into July.

Of the births estimated to have been shifted, around a quarter were due to be born in the last 3 days of the month, and over two thirds in the last week of the month. These findings are consistent with the time series analysis presented earlier and provide independent evidence that fewer than around 120 births are likely to have been shifted following the introduction of the Families Package.

The availability of daily due births data gives us the opportunity to test one further effect of potential concern. That is that rather than births being shifted into July, it is possible that due dates were shifted into July in order for parents to become eligible for the Best Start payment, regardless of when the baby was actually born. To test this possibility, we ran the same regression approach applied to registered births in the previous section to a due date series from the maternity data. This analysis showed no evidence of an unusual number of due births on or around 1 July, making any large scale shifting of due dates highly unlikely.

[32] December due births are excluded from consideration, as we do not have 2019 births and hence cannot assess late births for December 2018. December is also unusual in that the first days of the next month are always either weekends or public holidays.

Table 2 – Regression models and birth shifting estimates of individual maternity data, 2013 to 2018

Window	3 days (1)	7 days (2)	14 days (3)	28 days (4)	3 days (5)	7 days (6)	14 days (7)	28 days (8)
Panel A: Regression model								
June 2018 due births	0.059*	0.062**	0.041**	0.019**	0.052*	0.055**	0.04**	0.019**
	[0.026]	[0.016]	[0.009]	[0.005]	[0.025]	[0.016]	[0.009]	[0.005]
Demographic controls included?	N	N	N	N	Y	Y	Y	Y
Observations	31,203	75,096	151,515	305,610	31,203	75,096	151,515	305,610
R-squared	0.01	0.03	0.08	0.13	0.04	0.06	0.09	0.14
Panel B: Estimation								
No. of June 18 due births	444	1008	2049	4122	444	1008	2046	4122
No. of June 18 late births	153	279	375	408	150	282	378	408
Expected no. of late births with no Families Package	127	217	291	330	127	226	296	331
Estimated births shifted	26	62	84	78	23	56	82	77
Confidence interval	(1,45)	(25,86)	(44,119)	(38,117)	(4,49)	(31,94)	(46,122)	(38,118)

Note: Model is a linear probability model with the dependent variable an indicator of a late birth, set to be equal to 1 where a birth occurred after the month in which it was due, and 0 otherwise. All models include control variables capturing the due year, due month, day of week the due month ended in (where the birth was due in the last 2 days), and the number of days from the due date to the end of the month. Demographic controls included were maternal ethnicity, parity, multiple births, BMI, smoking status and age.

Appendix 2 - Calculating sub-annual Working for Families tax credit income

Background

Working for Families (WFF) tax credits provide targeted income to low-income working families.[33] Payments may be received regularly during the year or as an end-of-year wrap-up and are not taxed. As such payments are not included in tax returns, personal tax summaries, or employer monthly schedules, and are not included in Statistics NZ-derived income tables in the Integrated Data Infrastructure (IDI).[34] Most payments are made and managed by Inland Revenue (IR), but those on benefit who are eligible for specific payments (Family Tax Credit or Best Start tax credit) may choose to be paid by MSD. IR does an annual reconciliation of payments made by MSD, and this may result in a debit or credit adjustment, although in practice any debit may not be collected by IR.

It is important to account for WFF tax credits in any analysis of income in New Zealand, particularly where the focus is on income adequacy or poverty, given its importance as a contribution to the incomes of low earners and their families. It is not always obvious how to accomplish this, given the omission of this source of income from standard income tables. A particular complication of the WFF data in the IDI, is that for the most part it reflects entitlements and payments across an entire tax year, making sub-annual analysis more challenging. The approach described here seeks to overcome this problem by estimating changing entitlements across the year, allocating the annual entitlement to these sub-annual periods, and using these to derive monthly entitlements.

Working for Families tax credits

The WFF tax credit system includes a range of possible payments, some of which have been disestablished, but were paid to a small number of families during our analysis period:

- Family Tax Credit (FTC) – The Family Tax Credit is paid to working parents with earnings below a set threshold. If parents are receiving a benefit, the payment may be made by MSD or IR.
- Best Start tax credit (BSTC) – The Best Start tax credit was introduced as part of the Families Package in July 2018. The payment is available to all new parents who meet residency requirements, is available once any paid parental leave (PPL) entitlements are no longer being received, and is available for one year, or up to three years for parents earning below a specified threshold. It can be paid either by IR or MSD.
- Parental Tax Credit (PTC) – The Parental Tax Credit was disestablished in July 2018 with the establishment of Best Start. The payment was a time-limited payment to parents earning below a specified income threshold following the birth of a child.
- In-work tax credit (IWTC) – The in-work tax credit is an income-tested payment for parents who are not on benefit, and are working above a specified number of

[33] The Best Start tax credit represents an exception to this, as it is not conditional on being in paid work.
[34] This is also true of other untaxed income such as the Accommodation Supplement, special needs grants, the Winter Energy Payment and the disability allowance, paid by MSD.

hours per week (20 hours for single parents, and 30 hours combined for couples). The in-work tax credit replaced the Child Tax Credit in 2006.
- Child Tax Credit (CTC) – The Child Tax Credit was similar to the IWTC, but did not include the requirement to work a set number of hours. From 2006, CTC was only available to existing recipients who continued to meet the eligibility requirements.
- Minimum Family Tax Credit (MFTC) – The Minimum Family Tax Credit is a payment that tops up the income of parents who are working and not receiving a benefit to ensure they are better off in employment than they would be on benefit.

WFF tax credit data

The IDI includes a range of WFF data tables capturing information about people's tax credit payments and entitlements. These include: a table of information about primary caregivers and their partners (fam_parents); a table of information about children connected with each primary caregiver, their date of birth, and any shared custody arrangements (fam_children); and a table of annual family WFF returns (fam_return_details) with some income information, annual entitlements by payment type, details of payments already made by either Inland Revenue or MSD, and an end-of-tax-year 'washup' debit or credit applied to that family.

There is no reliable information on the distribution of any payments made across the tax year, nor is there any indication of when any payments are made (or collected) following the end-of-year washup adjustment. While credits are likely to be paid shortly after the family return is completed, situations where an overpayment is made, and a debit is raised as a consequence, may or may not result in the debt being collected by Inland Revenue.[35]

Although we are not able to observe payments directly, we do know when annual entitlements have been assessed by Inland Revenue and the value of those entitlements, and we have information we can use to determine when these entitlements were likely to fall during the tax year, such as: details of the number and age of children, including births during the year; the presence of a partner; and any shared custody arrangements. Detailed information on WFF and benefit payments made by MSD is held in the IDI, as well as sub-annual income data from other IR data sources.

Apportioning WFF entitlements across the tax year

The approach we take is to apportion WFF entitlements calculated by Inland Revenue (and, in some cases, MSD) across the tax year in which the entitlement existed, disregarding when the payment was actually made. People have the choice to receive payments regularly through the year, or to receive a lump-sum payment at the end of the year, and there are a number of reasons which might contribute to this choice. If a person's income is particularly inconsistent or unpredictable across the year, they may wish to avoid the risk of being overpaid and incurring a debt. Alternatively, they may not be aware of their entitlement during the year.

It is important to note that this approach apportions income into the period in which the entitlement fell, even if the person wasn't paid at that time, or was paid a different amount at the time. This differs from the reporting of other income in the IDI and used

[35] In the case of an over-payment by MSD, the debt is unlikely to be collected by IR (personal correspondence).

elsewhere in the report. Nevertheless, the approach allocates payments which were available to the person at that time, and which they received either then or subsequently. We also consider that the inclusion of WFF payments on this basis is preferable to omitting them entirely.

WFF tax credits paid while a person is on benefit are ringfenced, such that income earned outside the period in receipt of benefit cannot affect a person's entitlement while on benefit. This protects people against ending up in debt after moving into employment.

Where shared care arrangements exist, and both caregivers are responsible for the child or children for at least a third of the time (expressed as being at least 5 days per fortnight), both caregivers can access WFF tax credits. In the case of Family Tax Credit, Parental Tax Credit, and Best Start tax credit, payments are reduced in proportion to the care arrangement. In the case of Minimum Family Tax Credit, Child Tax Credit and In Work Tax Credit, both caregivers can claim the full amount.

At a high level, our approach involves the following steps:

1. For families that received WFF tax credits in a tax year, identify any entitlement periods during that year.[36]
2. For each entitlement period, establish the length of the period, the number and age of children attached to the period on different days across the period, and any shared custody arrangements pertaining to the child during the period.
3. Calculate WFF tax credit entitlements for each entitlement period based on the information in 2, and on payment rates extant in the relevant tax year.
4. Calculate income earned by the primary caregiver and any partner for each entitlement period from income tax data,[37] supplemented by income from other sources recorded in fam_return_details.[38] Where there is a single entitlement period during the year and our income differs from the IR calculated income, adjust our income to match IR's income.
5. Apply appropriate abatement rates and thresholds for the tax year in question to the entitlements calculated in 3, based on the income calculated in 4.
6. Reconcile our calculated entitlements against the entitlement recorded by IR. Adjust our entitlements based on the ratio of the entitlement recorded in the family return details table to the total annual entitlement we calculated in step 5.
7. Construct monthly entitlements from entitlement period data.

More specifically, our approach varies according to the specific payment.

Family Tax Credit

The Family Tax Credit is the main Working for Families payment, being accessed by the most families, and representing by far the biggest aggregate payment made in every tax year. Entitlements are more complicated to calculate and apportion than other payments, as they vary according to both the number and age of children, and according

[36] Where a primary caregiver spent time in and out of a relationship, or with different partners, these will form different 'entitlement periods' against which income is individually assessed.
[37] We use income tables calculated by Statistics NZ, derived from employer monthly schedules, IR3 returns, IR4s returns, and IR20 returns. This may exclude some sources of income used by IR to calculate WFF entitlements, however. Where there is only one entitlement period during the year, we use total income as recorded in the family return, as this will represent a complete record of assessed income.
[38] Child support payments received and paid are added and deducted respectively. Where a primary caregiver has multiple entitlement periods during the year, we assume the income adjustments apply to all periods equally.

to shared care arrangements. In addition, payments may be made either regularly, or at the end of the year, and recipients on benefit can choose to be paid their entitlement by either MSD or by IR. Our approach involves estimating eligibility for specific sub-periods within the year using Working for Families child and parent data (including changes in shared care), alongside benefit and family tax credit data from MSD and income tax data from Inland Revenue. Where our calculated entitlement differs from IR's entitlement, but our entitlement matches the entitlement paid by MSD, and where MSD is the primary payer, we assume the MSD entitlement is correct, and adjust the IR entitlement accordingly.

In-Work Tax Credit

The In-Work Tax Credit is available to low-income earners working above a specified number of hours, but not being supported by government support (benefit, NZ Superannuation or Student Allowance). The payment is a flat amount per week, differing only with the number of children, where there are more than three children in a family. We allocate the recorded entitlement proportionately across months which have no government support recorded and where either of the parents were working, according to the number of children recorded as being part of the family during that period. We do not have a record of hours worked and consider someone to meet the employment criteria if they or their partner received income from wages and salaries, self-employment, paid parental leave or ACC weekly compensation during that month.

Child Tax Credit

As discussed earlier, the Child Tax Credit was replaced by the In-Work Tax Credit in 2006. In recent years, very few people received the payment. For our analysis, we distribute any entitlement identified evenly across periods in which a family was entitled to Working for Families.

Minimum Family Tax Credit

The Minimum Family Tax Credit tops up the income of low-income families to a specified after-tax income amount. MFTC is only available while a person is in full-time work. As we are unable to reliably measure hours worked, and therefore identify full-time work, we distribute any identified MFTC entitlement across all entitlement periods where there is evidence of employment in that month (this includes income from wages and salaries, self-employment,[39] ACC weekly compensation or paid parental leave).

Parental Tax Credit

The Parental Tax Credit is available to parents who meet eligibility criteria, following the birth of a child. In cases where a Parental Tax Credit entitlement is established for a tax year, we distribute the payment across the applicable period following the birth date. We assume shared care arrangements are unchanged across the entitlement period.

Best Start tax credit

The Best Start tax credit is available to all new parents once any PPL entitlements are no longer being received, and is available for one year for all parents, or up to three years for low-income parents. The Best Start tax credit is not abated as other WFF tax credit payments are, and there is no income threshold for eligibility. Where an entitlement for

[39] Most self-employment income is not able to be broken down beyond the tax year. As such, where a person has self-employment income, we assume they were in employment in every month of the year.

Best Start is recorded for a tax year, we distribute the payments across the period after paid parental leave is exhausted. We assume any shared care arrangements are consistent across the entitlement period.

Appendix 3 – Characteristics by control cohort

Characteristics of children born 3 months pre- and post-1 July and their parents as recorded on birth certificates, individual control years

births in 3 months either side of 1 July:	2015 Pre-	2015 Post-	2016 Pre-	2016 Post-	2017 Pre-	2017 Post-
CHILD						
Ethnic groups						
European	66%	65%	64%	63%	64%	64%
Māori	28%	29%	28%	28%	28%	28%
Pacific	15%	16%	15%	15%	15%	15%
Asian	17%	18%	19%	20%	19%	19%
MELAA	2%	2%	2%	2%	2%	2%
Other	1%	1%	1%	1%	1%	1%
No siblings	40%	41%	41%	41%	42%	42%
At least one parent eligible for Best Start tax credit	97%	97%	97%	97%	97%	97%
MOTHER/FIRST PARENT						
Age						
Under 25	22%	21%	20%	21%	20%	20%
25-29	27%	26%	27%	28%	28%	28%
30-34	30%	31%	31%	31%	31%	32%
35 and over	21%	22%	21%	20%	22%	21%
Ethnic groups						
European	63%	62%	62%	60%	61%	61%
Māori	23%	23%	23%	23%	23%	23%
Pacific	12%	12%	11%	11%	12%	12%
Asian	17%	17%	18%	20%	19%	19%
MELAA	2%	2%	2%	2%	2%	2%
Other	1%	1%	1%	1%	1%	1%
With earned income in the month prior to birth	51%	51%	52%	51%	53%	53%
Mean earned income prior to birth - 0-6 months	$11,496	$11,583	$12,177	$12,099	$12,392	$12,632
Mean total income prior to birth - 6 months to 1.5 years	$31,380	$31,722	$32,322	$32,569	$32,681	$34,211
Mean total income prior to birth - 1.5 to 4.5 years	$86,596	$86,328	$84,026	$83,041	$83,578	$86,264
Supported by benefit in the month before birth	23%	23%	21%	22%	21%	21%
Supported by benefit with partner in the month before birth	4%	4%	3%	3%	3%	4%
FATHER/SECOND PARENT						
With earned income in the month prior to birth	5%	5%	5%	5%	5%	5%
Where father/second parent recorded:						
Ethnic groups						
European	59%	58%	57%	57%	57%	57%
Māori	20%	20%	20%	21%	20%	20%
Pacific	12%	12%	12%	12%	12%	12%
Asian	15%	16%	17%	18%	17%	18%
MELAA	2%	2%	2%	2%	2%	2%
Other	2%	2%	2%	2%	2%	2%
With earned income in the month prior to birth	77%	76%	78%	77%	78%	78%
Mean earned income prior to birth - 0-6 months	$22,246	$22,267	$22,916	$22,886	$23,654	$23,914
Mean total income prior to birth - 6 months to 1.5 years	$53,165	$52,370	$54,342	$53,810	$54,744	$57,075
Mean total income prior to birth - 1.5 to 4.5 years	$130,456	$128,399	$128,585	$127,107	$129,172	$133,756
NEIGHBOURHOOD						
Deprivation quintile (NZDep2013)						
1	16%	16%	16%	16%	16%	16%
2	17%	18%	18%	17%	18%	18%
3	19%	19%	19%	20%	20%	19%
4	21%	21%	21%	20%	20%	21%
5	25%	26%	26%	26%	26%	26%
ANALYSIS SUB-GROUP (of mother/first parent)						
(1) in receipt of benefit in month prior to birth	23%	23%	21%	22%	21%	21%
(2) not in receipt of benefit and PPL eligible	51%	51%	53%	52%	54%	54%
(3) not in receipt of benefit and not PPL eligible	26%	26%	26%	26%	25%	25%
Total %	100%	100%	100%	100%	100%	100%
Total n	14,676	14,913	14,778	15,390	14,907	15,063