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The impact of Malta's free childcare scheme on female employment

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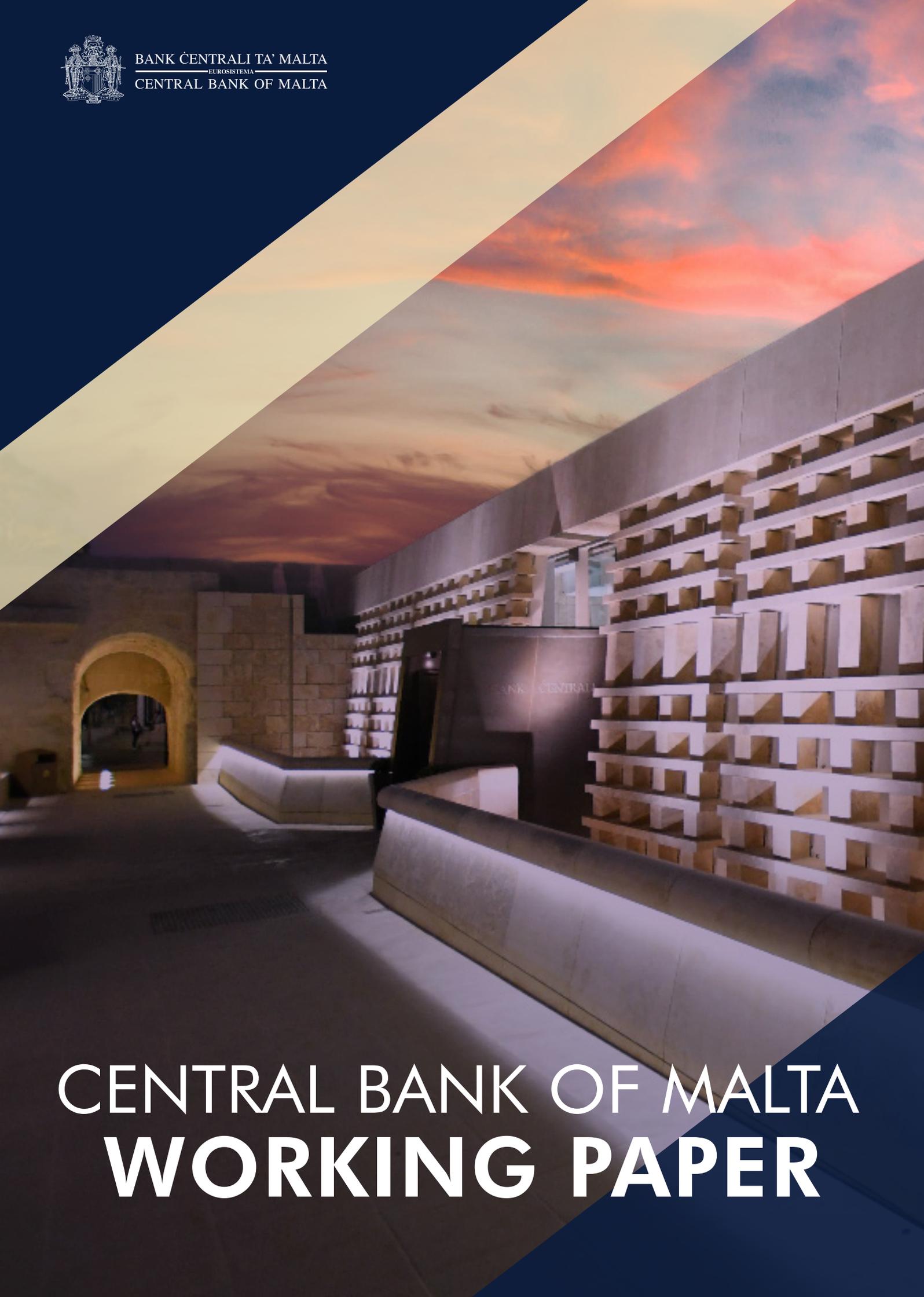
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The Impact of Malta's Free Childcare Scheme on Female Employment

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WP/01/2026

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The views expressed are those of the author and do not necessarily reflect the views of the Central Bank of Malta or any other institution. Any errors are the author's own.

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Abstract

This study evaluates the impact of Malta's 2014 childcare reform, a universal Free Childcare Scheme for children aged 3 months to 3 years, on female employment. Using a difference-in-differences (DD) framework and matched DD models, I find that the reform may have increased the probability of female employment by 1.3–2.4 percentage points, with effects growing over time. Heterogeneity analyses reveal that employment gains were disproportionately concentrated among single mothers and mothers with multiple children, i.e., subgroups that have historically faced greater structural barriers to employment. Notably, the reform likely helped reduce pre-existing employment gaps by up to 26% for single mothers, 39.3% for two-child mothers and 14.1% for mothers with three or more children. However, no persistent employment effects were observed post-eligibility, and fiscal savings were limited to reduced welfare claims among single parents. These findings contribute to the global literature on childcare subsidies by highlighting how nation-wide access, when subject to well-designed eligibility criteria, can narrow labour market disparities, while also showing that persistent employment effects require sustained support beyond the early childhood years.

JEL classification: J13, J22, I38, H31, C21

Keywords: Childcare subsidies, Female employment, Difference-in-Differences, Universal Childcare

Table of Contents

1. Introduction	4
2. Institutional Setting: The Free Childcare Scheme in Malta.....	5
3. Literature Review	9
4. Methodology.....	15
5. Data Description.....	18
6. Results.....	21
6.1 Main Results.....	21
6.2 Complementary Results	23
7. Robustness Checks	30
8. Conclusions	32
9. References	35
Appendix	38

1. Introduction

Malta, a Southern European microstate in the Mediterranean, is characterised by its dense population, strong familial ties, and a labour market that has historically exhibited one of the lowest female employment rates in the EU (Eurostat, 2025a). The country's cultural preferences for maternal childcare coupled with limited access to affordable formal care have for many years constrained the labour force participation of mothers with young children. In April 2014, the Maltese government implemented a radical policy intervention: a nation-wide, non-means-tested, Free Childcare Scheme for children aged 3 months to 3 years on the condition that both parents were employed or in education. Prior to this reform, no comprehensive public childcare scheme existed and the existing measures in place provided very limited support that left most families facing high out-of-pocket costs. The childcare reform was not merely an expansion of social welfare but a deliberate effort to stimulate female employment.

This study is the first one to evaluate the causal impact of Malta's free childcare reform on female employment. A difference-in-differences (DD) framework is employed to analyse the high-quality, administrative microdata specially requested from the Ministry for Social Policy and Children's Rights which was specially constructed for this study by the National Statistics Office (NSO) using Malta's System for the Administration of Social Benefits (SABS). The dataset comprehensively tracks the employment status, demographic characteristics and specific social benefit receipt for the entire Maltese population on an annual basis from the period 2012 to 2016.

Using a repeated cross-section approach, the study analyses 89,727 mother-year observations, comparing mothers eligible for the scheme whose youngest child is between 3 months to 3 years (treatment group) to a carefully selected control group of mothers with an older youngest child of 6 to 9 years who were never eligible during the study period. The DD strategy isolates the reform's effect by comparing changes in mothers' employment outcomes between these groups before and after the policy's implementation, while also controlling for observable confounders (i.e., age, marital status, locality, disabled child, number of children) and testing the critical common trends assumption.

The study's findings are as follows. The free childcare reform is estimated to have increased the probability of female employment by approximately 1.3 to 2.4 percentage points, with effects growing over time and peaking in 2016 at 1.7 to 3.2 percentage points. The lower-bound estimates in each range are computed using the standard empirical DD specification, while the upper-bound estimates are based on a matched DD approach. This latter approach combines the strengths of DD, by controlling for unobserved time heterogeneity and common trends over time, with matching techniques that improve the comparability of treated and control groups by conditioning on shared characteristics. The complementary analyses then reveal more nuanced patterns of the impact. Firstly, I analyse heterogeneous treatment effects and find that disadvantaged groups, particularly single mothers and mothers with multiple children, benefit most from the reform due to their significant relative rise in female employment. Secondly, mothers did not sustain a higher employment probability after their children aged out of eligibility, suggesting that the policy's impacts were confined to the eligibility period. Thirdly, the reform mainly reduced welfare dependency among single mothers, with Social Assistance for Single Unmarried Parents (SUP) claims falling by 1.2 percentage points in the post-reform period (2014-2016). Finally, the decomposition of previous employment status shows that the aggregate effect was mainly driven by new entrants into employment (+2.2 percentage-point probability to start work in 2016) rather than the retention of already-employed mothers.

This study contributes to the existing literature in several important ways. First, it exploits the rollout of a novel Active Labour Market Policy (ALMP) in a previously understudied, small EU member state with a relatively low female employment rate prior to the reform, especially among mothers with young children. Given this context, one might reasonably expect the effects on maternal employment to be substantial, at least before accounting for mitigating factors such as widespread informal care or persistent cultural norms.

Second, while numerous studies analyse the effects of heavily subsidised childcare (Bettendorf, Jongen and Muller, 2015; Givord and Marbot, 2015; De Muizon, 2020; Bousselin, 2022; and Nollenberger and Rodriguez, 2015), few estimate the impact of introducing entirely free full-time childcare on a national scale on maternal employment. Brewer et al. (2022) examine the rollout of free childcare across England, but this policy only extends the entitlement from part-time to full-time care when the eligible child turns four. Huebener, Pape and Spiess (2020) study the abolition of private childcare fees in only select German federal states, though this applies only to children in their final year of daycare before primary school. Similarly, the well-known Quebec reforms analysed by Lefebvre and Merrigan (2008) as well as Baker, Gruber, and Milligan (2008) introduced universal and highly subsidised childcare and while costs were reduced by approximately 80%, they were not entirely eliminated. Furthermore, to the best of my knowledge, this is the first study to examine the effects of a scheme that completely eliminates childcare costs while simultaneously imposing a conditional eligibility criterion requiring mothers to be employed or in education to receive such aid.

Third, this study enriches the literature by leveraging high-quality administrative microdata that captures the full population of mothers. The dataset also allows for the analysis of social benefit claims, thereby facilitating the identification of not only employment effects, but also changes in welfare dependency.

Fourth, in line with recent literature, I explore heterogeneous treatment effects across different subgroups of mothers, mainly single mothers, married mothers, mothers with a disabled child, mothers with multiple children, and low-income mothers. This allows for a more granular understanding of the reform's differential impact and, more importantly, underscores the reform's role in promoting employment equity by disproportionately benefiting more disadvantaged groups.

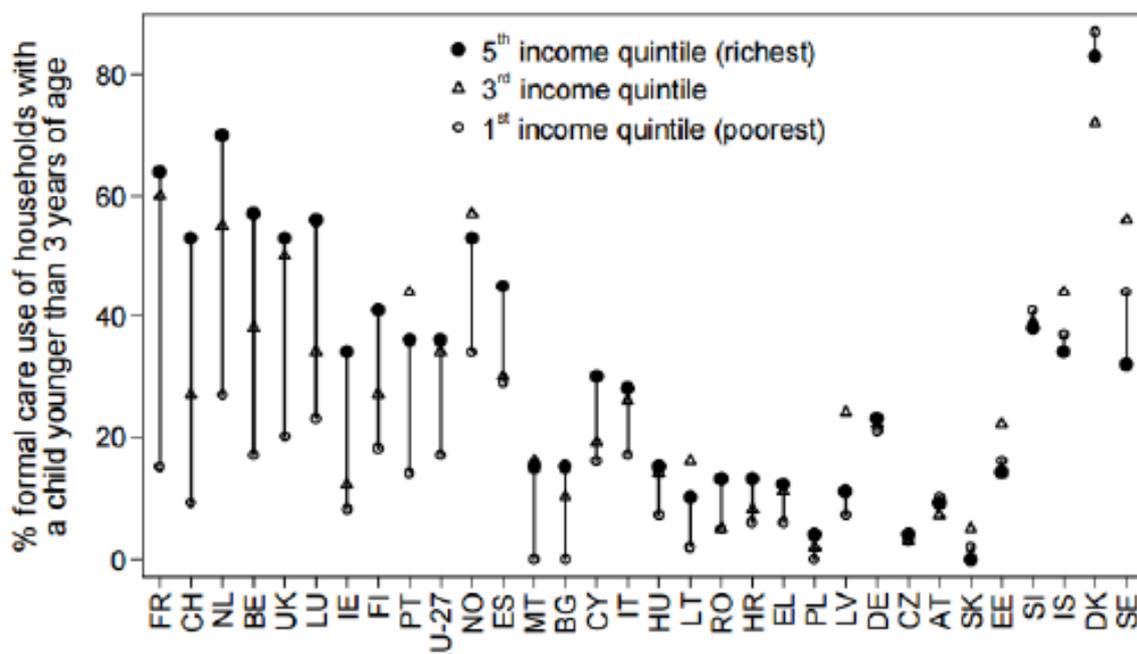
Finally, by examining both the persistence of employment effects and the mechanisms through which mothers enter or exit the labour market, the study offers deeper insights into the temporal dynamics underlying behaviour responses to the policy.

The study proceeds as follows. Section 2 discusses the institutional setting and situates the reform within the national policy context; Section 3 reviews the relevant international literature; Section 4 outlines the empirical methodology; Section 5 presents the dataset along with the descriptive statistics; Section 6 discusses the results and robustness checks; and Section 7 provides a discussion and conclusion of the study.

2. Institutional Setting: The Free Childcare Scheme in Malta

Malta has long struggled with a persistently low female labour market participation, particularly among mothers of young children. In 2012, only 45.3% of women aged 15-64 were employed in Malta (EU-27 average: 57.6%) compared to 74.0% of men (EU-27 average: 68.8%) (Eurostat, 2025a). The gender employment gap increases with age, although it remains negligible among the 15-24 age group (NSO, 2021), most likely because most women in this age-group have not yet had children. Unsurprisingly, this gap had also been reinforced by low enrolment in formal childcare prior to the reform. In 2010, around 10% of children under the age of three were enrolled in formal childcare, far below the Barcelona target of 33% (European Commission, 2013). Mills et al. (2014) further show that uptake of formal childcare was particularly low among low-income households (see Chart 1).

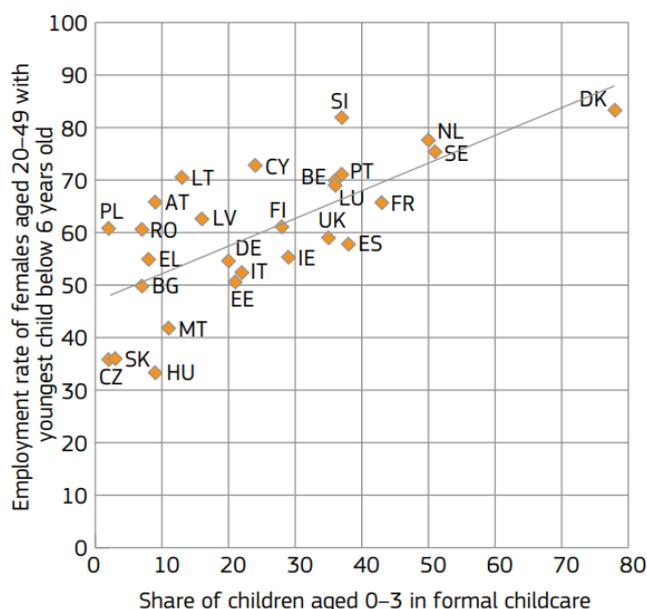
Chart 1
USE OF FORMAL CHILDCARE BY HOUSEHOLDS WITH A CHILD BELOW THE AGE OF THREE - BY INCOME QUANTILE, 2010



Source: Mills et al., 2014

Against this background, in 2013, the European Commission emphasised that access to affordable formal childcare is a key determinant of female employment (see Chart 2). The Commission had also highlighted Malta as one of the countries where insufficient childcare services were undermining female employment, reinforcing the need for policy intervention. These recommendations formed part of a broader EU-wide agenda aimed at reducing gender inequalities in employment and improving work-family reconciliation.

Chart 2
FEMALE EMPLOYMENT: 20-49 YEARS OLD AND PROPORTION OF CHILDREN IN FORMAL CARE, 2010



Source: European Commission, 2013

It was in response to these concerns, and to explicit EU Council recommendations, that Malta introduced a major childcare reform. Particularly, on 1 April, 2014, the Government of Malta launched the Free Childcare Scheme, a universal policy designed to incentivise parents, especially mothers, to re-enter or remain in employment after childbirth.² Before the reform, formal childcare came at an unsubsidised cost to families, placing a considerable financial burden on them and often acting as a barrier to maternal employment.³

The Free Childcare Scheme, which remains in place over a decade later, provides entirely free, non-compulsory childcare for children aged 3 months to 3 years, that is, until the child qualifies for free, state-provided kindergarten.⁴ In order to qualify, both parents (or the sole parent or guardian in a single-parent household) must either be employed and paying social security contributions, or enrolled in a recognised educational institution, regardless of their age. Parents using parental leave are not eligible for the scheme. The scheme's conditionality carries important economic implications. The reform not only lowers the direct cost of childcare but also creates a direct incentive for mothers to work. In this sense, the policy operates through two distinct but complementary channels: it both enables employment (by increasing a mother's free time and lowering the opportunity cost of employment) and encourages it (through the conditional eligibility). The scheme therefore embodies a hybrid model that lies between welfare support and an ALMP.

Furthermore, one may worry that the impact on female employment could be limited by constraints on childcare hours. However, this is unlikely to be the case, as childcare centres in Malta operate in line with standard working hours, with opening times varying across centres to accommodate family needs. Additionally, beneficiaries are entitled to childcare hours equivalent to the working hours of the parent with the lower workload, supplemented by 10% for flexibility, and 20 hours per month to account for commuting.

Alongside the child's age and parents' employment conditions, parents are eligible for free childcare only if they enrol their child in a childcare centre that participates in the scheme. The scheme is delivered through both private and public childcare providers. Private centres operate through a Public Private Partnership (PPP) agreement with the government, under which they are reimbursed directly by the state at a fixed hourly rate per child, initially set at €3.00 in 2014. If instead parents choose a private centre that does not participate in the scheme, they are only eligible for an annual childcare tax deduction. This tax deduction was increased from €1,300 to €2,000 per child as of 1 January 2014 (Ministry for Finance, 2015). Although both measures were targeted at the treatment group, the budgetary impulse of the Free Childcare Scheme is likely to be substantially larger than that of the tax deduction increase. Consequently, while the estimated female employment effects reflect the combined impact of both policies, they are likely driven primarily by the Free Childcare Scheme.⁵

Many mothers have benefited from the scheme since 2023 with approximately 98% reportedly in employment (Malta Independent, 2024). Chart 3 shows the number of children enrolled in the scheme split by age category. The data reveals that while 875 children were enrolled in 2014, this figure rose sharply to 2,768 in 2015 and 5,247 in 2016. A breakdown by age also indicates that in 2014, only 5

² Most information in this section is drawn from government documents: *Early childhood education and care in Malta: The way forward* by Sollars, V. (2013), and *Free Childcare Scheme Brochure* by Jobsplus (2024a).

³ However, some benefits were introduced in March 2013, mainly a €1.50 per hour subsidy for parents attending the Employment and Training Corporation (ETC) training, a tax deduction of up to €1300 for all parents incurring registered childcare expenses, and incentives for employers to invest in childcare facilities at the workplace. Nonetheless, these did not significantly reduce out-of-pocket costs for the majority of families who used formal care.

⁴ Free childcare continues until the child is eligible for free, state-provided kindergarten, irrespective of whether the child will attend a state, church or independent kindergarten. State Kindergarten has two intakes per scholastic year: on the 1st of February and the 1st of October (Jobsplus, 2024a). Children are admitted to Kindergarten on February 1st (or October 1st) if they turn three by the end of April (or December), respectively.

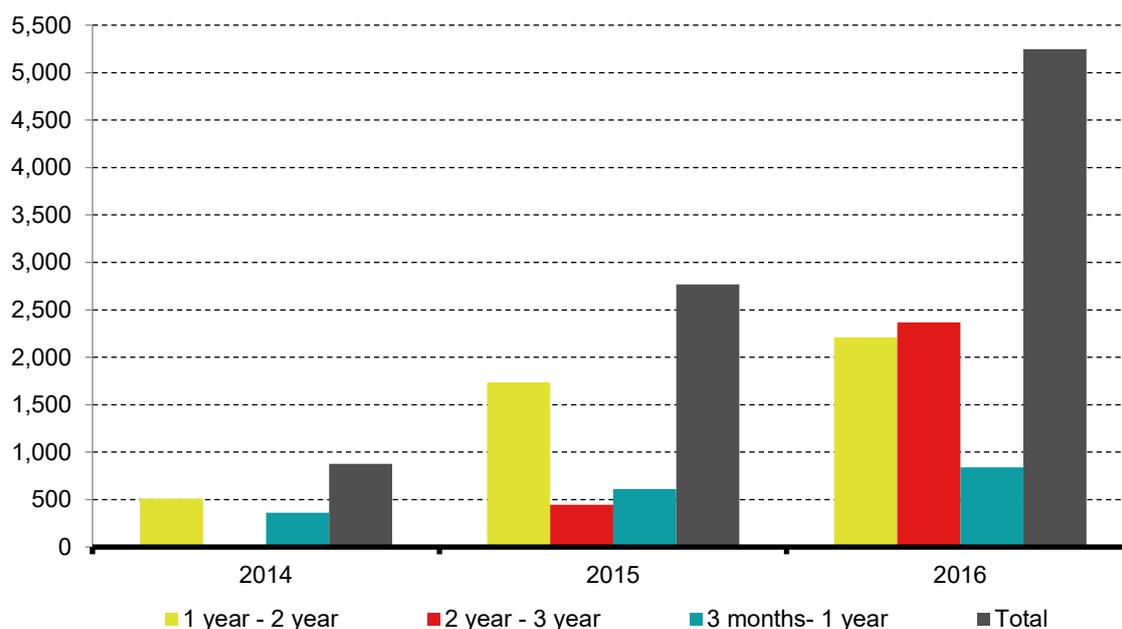
⁵ All references to the reform's impact will therefore be interpreted as capturing the joint effect of these two policy components, which are hereafter referred to collectively as the "Maltese childcare reform."

children between the age of 2 and 3 years were enrolled, increasing to 2,366 by 2016. These figures suggest that take-up of the scheme was gradual rather than immediate. For comparison, Sollars (2013) reported a total of 2,279 children enrolled in childcare in 2013, prior to the reform. This implies that not all previously enrolled children or eligible newcomers shifted immediately into the Free Childcare Scheme upon its launch on 1 April 2014. Furthermore, participation in the scheme has continued to increase in recent years with the number of children enrolled in the scheme rising from 5,939 in 2017 to 7,509 in 2021 and reaching 9,400 in 2024 (Jobsplus, 2017, 2021, 2024b).

Chart 3

CHILDREN ENROLLED IN THE SCHEME – BY YEAR AND AGE

(Number of children)



Source: Authors' calculations based on MT-HFCS data.

Note: Data for 2014 reflect enrolment figures as of 1st April 2014 aligning with the introduction of the scheme. Figures include carryovers; children enrolled across multiple years are counted once per year to reflect actual annual enrolment levels.

Additionally, provider participation also increased markedly between 2014 to 2023: the number of centres enrolled in the Free Childcare Scheme rose from 69 to 188 (Malta Independent, 2024). This rise reflects increased enrolment by centres as well as the opening of new ones. Most centres enrolled in the Free Childcare Scheme are private; there are only 13 public centres operated by the Foundation for Educational Services (FES). All-in-all, as highlighted in a European Commission (2025) report, at the time the childcare reform came into place, parents in Malta faced minimal childcare availability constraints. With no binding supply-side constraints, the reform therefore had the potential to effectively influence maternal employment.⁶

Since the implementation of the scheme, Malta has recorded one of the largest increases in formal childcare attendance in the EU, alongside the Netherlands. By 2023, 51% of children in Malta under the age of three were enrolled in formal care, surpassing the revised Barcelona target of 45% for 2030.

These developments coincided with a profound improvement in female employment. By 2024, Malta's female employment rate had risen to 72.3%, surpassing the EU-27 average of 66.2%, with Eurostat (2025a) attributing much of this rise to the Free Childcare Scheme and other "Making Work Pay"

⁶ The report highlights that all parents in Malta seeking a place for their child aged 0–2 have successfully secured one, while for children aged 3 and older, the Maltese government has sufficient capacity and resources to meet demand by establishing additional classes whenever necessary.

measures. Furthermore, Micallef (2018) observed that the female labour participation rate in Malta, which historically peaked in the early 20s and declined sharply after childbirth, recently began to peak later and remains significantly higher in the late 20s and beyond. Grech (2025) also interestingly notes that the post-birth decline in female employment is significantly smaller for women born in 1988 and after whose first childbirth coincided with the rollout of the Free Childcare Scheme (assuming a first birth at the average age of 27). This suggests a potentially targeted effect on maternal employment.

However, despite these promising trends, the absence of a suitable counterfactual means that the true causal effect of the reform on maternal employment remains uncertain. This study therefore aims to address this gap by providing the first empirical estimate of the female employment impact of Malta's entirely free childcare reform. To situate Malta's childcare policy within a broader economic context, the next section reviews the international empirical literature on how reductions in childcare costs, whether through subsidies or free provision, affect maternal employment.

3. Literature Review

Subsidised childcare has been widely shown to raise maternal employment by reducing a major cost barrier to work (e.g., Blau and Currie, 2006; Jaumotte, 2003). In fact, the expansion of affordable childcare is recognised as a key driver of rising female employment in developed countries since the 1970s, with the Scandinavian countries often cited as leaders in this regard. Inspired by their success, several countries have adopted generous childcare subsidies (e.g., Germany, Estonia) or even fully free childcare (e.g., Malta, England, Italy).

Nonetheless, the responsiveness of female employment to changes in childcare costs varies considerably across countries. In their comprehensive review of the childcare and maternal labour supply literature, Blau and Currie (2006) report that estimates of the elasticity of female labour force participation with respect to childcare price range from 0.06 to -3.60.⁷ Much of this wide variation reflects differences in empirical approaches: structural labour supply models typically produce larger elasticities, while reduced-form estimates tend to be smaller. The variation also stems from challenges such as the endogeneity of childcare prices and availability, as well as unobserved factors such as the availability of informal care. Studies relying on non-experimental or cross-sectional variation, such as those reviewed by Blau and Currie (2006), therefore tend to produce heterogeneous estimates partly because they cannot fully account for these contextual differences or for underlying selection into childcare. A further complication arises when trends in maternal employment differ systematically across comparison groups, violating the identifying assumption needed to isolate the causal effect of childcare and potentially biasing estimated impacts.

More recent research addresses these limitations by employing quasi-experimental methods that leverage policy reforms by introducing temporal or regional variation in the availability or cost of childcare.⁸ Most such studies find positive effects on maternal employment, especially in contexts with limited pre-existing childcare provision and entrenched gender norms.

Even though quasi-experimental studies are more robust to methodological limitations, they still show considerable variation in the estimated female employment impact. Givord and Marbot (2015) and De Muizon (2020) find that France's PAJE reform, which reduced formal childcare costs, only modestly increased formal childcare use and female employment, with the impact mainly detected among mothers of two children. Bettendorf, Jongen, and Muller (2015) report a similar pattern following a Dutch

⁷ This suggests that a 1% increase in the price of childcare is associated with changes in female labour force participation ranging from a negligible 0.06% increase to a substantial 3.6% decrease, though most estimates indicate a negative relationship.

⁸ All quasi-experimental studies reviewed in this section that estimate the impact of specific childcare reforms on female employment are summarised in Table 1. Table 1 draws on, and extends, the information originally compiled in the table presented by Bettendorf, Jongen, and Muller (2015).

reform that halved the average effective cost of formal childcare for working families. Smaller or null effects are also reported for Spain, Luxembourg, Switzerland, and Scandinavia, where maternal employment or public childcare coverage were already high (Nollenberger and Rodríguez-Planas, 2015; Bousselin, 2022; Felfe, Lechner, and Thiemann, 2013; Lundin, Mörk, and Öckert, 2008; Havnes and Mogstad, 2011).

On the other hand, studies looking at the introduction of fully subsidised formal care overwhelmingly find that such policies can be very effective at increasing female labour market participation. Brewer et al. (2022) find that free full-time formal care in England increased maternal labour supply and employment by at least 3 and 1 percentage points (pp), respectively, while part-time care had no significant impact due to substitution from informal arrangements. These effects are observed immediately after eligibility and become more pronounced over time. In Quebec, Lefebvre and Merrigan (2008) find that the universal, almost fully subsidised (\$5-a-day) childcare programme raised the participation rate of mothers with preschool children by roughly 8 percentage points (13%). Like Malta, Quebec's reform targeted both public and private providers and occurred in a context of relatively low female participation. However, concerns were raised about the quality of care following the rapid expansion, echoing Malta's early struggles with qualified staff (Borg, 2015). Interestingly, the authors also highlight that the creation of low-cost childcare spaces increased competition in the broader daycare market, thereby magnifying the policy's impact.

Using a different dataset to evaluate the same reform, Baker, Gruber, and Milligan (2008) report a 14-percentage point rise in the use of non-parental care and a 7.7-percentage point (14.5%) rise in maternal employment. This discrepancy once again highlights how subsidised formal care may crowd out informal care because the increase in formal childcare use exceeds the increase in employment. This implies that a substantial share of mothers merely substituted informal arrangements with subsidised formal care while maintaining the same employment status. Such cases raise the question of whether the associated fiscal benefits from higher employment are sufficient to offset the cost of the subsidy - a matter of debate also in Malta.

In Argentina, Berlinski and Galiani (2007) report that free preschool expansion raised enrolment by 18% for children aged three to five and modestly raised maternal employment. Similarly in Italy, Carta and Rizzica (2018) find that access to nearly free early kindergarten raised maternal labour force participation by 6 pp and employment by 5 pp. The authors show that the increase in employment was partly driven by a 17% reduction in the reservation wage of unemployed mothers, making them more likely to accept job offers after initiating a job search.

This variation in results suggests that the impact of childcare subsidies on female employment is likely to be non-linear in both the extent of the subsidy provided by the state and the initial female employment rate. In other words, the effectiveness of reforms is set to depend considerably on the extent of financial support provided and the pre-reform state of the female labour market.

Moreover, the design of the reform, that is, whether it attaches any employment-based eligibility conditions is bound to have profound impacts on the effectiveness of childcare subsidies. Evidence on this latter point, however, is quite scarce. In fact, most studies found in the literature, with the notable exception of Bettendorf, Jongen, and Muller's (2015), evaluate childcare reforms that do not impose an employment condition for eligibility. Therefore, the estimated treatment effects in most studies primarily capture the impact of lower childcare costs and relaxed time constraints, while abstracting from the behavioural responses that could stem from employment-based eligibility conditions.

In this light, Malta's childcare reform stands out as particularly atypical. By combining the elimination of childcare costs with an explicit requirement that parents be employed or enrolled in education to qualify, the scheme fundamentally alters the nature of the policy intervention. Unlike conventional childcare subsidies, which primarily operate as price reductions, Malta's eligibility conditions make it conceptually closer to an active labour market policy than to a pure childcare subsidy. By embedding work incentives directly into childcare provision, the resulting behavioural responses are therefore expected to be larger

and more immediate than those documented in studies examining more incremental, unconditional reforms.

Mechanisms and Heterogeneous Effects

The literature also points at important heterogeneous effects across different subgroups of mothers. Particularly, greater responses of female participation rates are observed among *single mothers* (Bettendorf, Jongen, and Muller, 2015; Huebener, Pape, and Spiess, 2020; Brewer et al., 2022; Goux and Maurin, 2010; Cascio, 2009), *mothers with multiple children* (Givord and Marbot, 2015; Nollenberger and Rodriguez-Planas, 2015), *educated mothers* (Havnes and Mogstad, 2011; Lefebvre and Merrigan, 2008; Huebener, Pape and Spiess, 2020; Carta and Rizzica, 2018; Brewer et al., 2022), *mothers with younger eligible children* (Bettendorf, Jongen, and Muller, 2015; Bousellin, 2019) and *mothers in higher income households* (Carta and Rizzica, 2018). Carta and Rizzica (2018) and Brewer et al. (2022) also report that the presence of younger, ineligible children diminishes the overall effect on maternal participation, since caregiving responsibilities persist for the younger child. In Section 5, we describe the dataset which includes some of these key variables, enabling us to explore heterogeneous effects.

The variation in the estimates of these studies can be attributed to differences in the characteristics of the populations studied, the structure of local labour markets, and the extent to which informal care options are accessible (Havnes and Mogstad, 2011). For example, mothers in regions with high vacancy rates (Carta and Rizzica, 2018), low unemployment rates (Brewer et al., 2022) and denser labour markets (Huebner, Pape and Spiess, 2020) experience larger participation and employment effects. Furthermore, overly generous parental leave that extends a mother's absence from the workforce may adversely affect her decision to return to the labour market because of a decline in labour attachment and skills (Edin and Gustavasson, 2008). In fact, Christiansen et al (2016) identify an inverted U-shaped correlation between the duration of parental leave and the probability of female employment.

In sum, the most impactful childcare policies are those that are universal, fully subsidised, and supported by accessible and high-quality childcare as well as favourable local labour market conditions. The impact of lowering childcare costs tends to be minimal in contexts where female labour force participation is already high or where childcare is already heavily subsidised (Vuri, 2016). In contrast, countries that restrict childcare subsidies to employed single parents or dual-earner households, excluding non-working parents, tend to observe more pronounced increases in maternal employment (Vuri, 2016). Additionally, the largest employment effects are observed for mothers with younger eligible children, single mothers, mothers with multiple children and/or high-educated mothers. Importantly, many reforms crowd out informal care, and their net effect depends on whether they expand total care hours or simply substitute care types.

In this light, the case of Malta's childcare reform is especially interesting and can be regarded as unique in a number of dimensions. First, there are only few documented reforms around the world that essentially fully eliminate all childcare costs in an economy that had a very low female employment rate. Secondly, most studies discussed till far, with the notable exception of Bettendorf, Jongen, and Muller's (2015), estimate the effects of childcare reforms that do not impose an employment condition for eligibility. Furthermore, Malta's case is especially interesting also because, unlike in the reform enacted in Quebec, Malta's case was not strongly affected by supply-side constraints of childcare centres.

This study therefore exploits the rollout of this uniquely structured reform, drawing on high-quality administrative data covering the entire population of mothers in Malta, to evaluate the causal impact of introducing a free childcare system on female employment. The breadth of this dataset also enables the estimation of heterogeneous treatment effects, shedding light on the distributional impacts of the scheme.

Table 1
SUMMARY OF STUDIES ESTIMATING THE EFFECT OF CHILDCARE REFORMS ON FEMALE PARTICIPATION RATE

Study	Country	Reform/ Instrument	Period	Treatment Group	Pre: PR (%)	TE:PR (pp)
<i>'Intention-to-treat' studies</i>						
Givord & Marbot (2015)	FR	Rise in childcare subsidies, 50% drop in childcare fee	05-09	Mothers y. 0-3	-	1.1***
				1-child mothers y. 0-3	-	0.2
				2-child mothers y. 0-3	-	1.6***
				3+ child mothers y. 0-3	42	1.6***
De Muizon (2020)	FR	Rise in childcare subsidies	01-09	1-child mothers y. 1-3	74	SR: -1.9 LR: -0.9
				2-child mothers y. <1	-	SR: 1.6 LR: -0.0
				2-child mothers y.1-3	47	SR: 2.2 LR: 4.2***
Bettendorf, Jongen & Muller (2015)	NL	Childcare fee from 37 to 18% Extension to guestparent care Increase EITC work. parents	95-09	Mothers y. 0-11	71	2.3***
				Mothers coupl. y. 0-11	72	2.0***
				Single moth. y. 0-11	56	4.7***
				Mothers y. 0-3	70	2.5***
				Mothers y. 4-7	69	2.8***
				Mothers y. 8-11	74	1.5**
Lundin, Mörk, and Öckert (2008)	SWE	Price cap on childcare prices, 55% drop in childcare fee	01,03	Mothers coupl. 1-9	70	-0.2
Havnes & Mogstad (2011)	NOR	Staggered intro. childcare	76,79	Married moth. y. 3-6	25	1.1***

Lefebvre & Merrigan (2008)	CAN	Expansion childcare Quebec, childcare fee from 50 to 20%	93-02	Mothers y. 1-5	61	8.1***
Baker, Gruber, & Milligan (2008)	CAN	Expansion childcare Quebec, childcare fee from 50 to 20%	94-02	Women coupl. 0-4	53	7.7***
Huebener, Pape, & Spiess (2020)	DE	Staggered abolition of subsidised childcare fees	05-13	Mothers y. 6	63	0.7
Cascio (2009)	US	Staggered introduction of public school	50-90	Married moth. y. 5 Single moth. y. 5	36 58	-1.1 6.9**
Nollenberger & Rodriguez-Planas (2015)	ESP	Expansion subs. childcare	87-97	Mothers y. 3 30 year+ mothers y. 3	29 31	2.8* 4.7**
Carta & Rizzica (2018)	IT	Staggered exten. kindergarten (subsidised daycare)	09-12	2+ child mothers y.3 Mothers y. 2	26 58	3.9** 6.0*
Brewer et al. (2022)	EN	^a Intro. of free PT care Extension of childcare hours (3h to 6.5 h daily)	00-13	Mothers y. 3 Mothers y. 4	61 61	0.5 3.5***
Felfe, Lechner, and Thiemann (2020)	SWI	Variation in after-school care across neighbouring states	00, 10	Mothers 4-12	69	0.5
Goux & Maurin (2010)	FRA	Eligibility for preschool	99	Mothers couple. 3 Single moth. 3	78 80	0.4 3.6**
Berlinski & Galiani (2007)	ARG	Stag. expansion of preschool	92-00	Mothers 3-5	39	12.4*

Bousselin (2019)	LU	Expansion of access for subsidised childcare	04-14	Mothers y. 0-13	60	3.0**
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Source: Bettendorf, Jongen and Muller (2015) with additional more recent literature added by author.

Note: Column 4 shows the sample period where e.g., 05-09 means that the data spans from 2005 to 2009 while e.01,03 means that the data covers 2001 and 2003. The treatment group is shown in Column 5, where youngest child is shortened to 'y.' and the numbers (e.g., 0–3) represent the child's age. Column (6) shows the pre-reform (Pre) or counterfactual female participation rate (PR) as a percentage; Column (7) shows the treatment effect (TE) on female participation rate (PR) in percentage points. * Denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. The short- vs long- term dimension are defined according to the respective authors' discretion.

^a Children become eligible for free PT childcare at the beginning of the school term (September, January or April) following their third birthday and then qualify for FT childcare in the September after they turn four.

4. Methodology

Identification Strategy and Treatment Definition

Given that the eligibility for the Free Childcare Scheme varies only with the age of the youngest child and calendar time, the treatment status in each year is determined mechanically by the age of the youngest child, rather than by parental choice, discretionary take-up, or policy variation across units. This rule-based assignment substantially limits scope for endogenous selection into treatment and supports the use of a DD design, with mothers whose youngest child falls just outside the eligibility window serving as a natural comparison group.

I, therefore, estimate the effect of the Maltese childcare reform on female employment using a DD strategy. This empirical strategy identifies the causal effect of the policy intervention by comparing changes in employment outcomes over time between mothers directly affected by the reform, the treatment group, and a comparable group not exposed to the reform, the control group. The purpose of a control group is to account for time-varying confounders common to both groups. Under the identifying assumption that, in the absence of the reform, employment outcomes for the two groups would have moved in tandem, the DD estimator can be interpreted as the causal impact of the reform.

Table 2
CHILD AGE COHORTS AND POLICY EXPOSURE IN A REPEATED CROSS SECTION - BY BIRTH DATE AND POST-REFORM YEAR

Post-Reform Year		2014	2015	2016
Date of Birth	31/12/2016	NA	NA	0
	30/06/2016	NA	NA	0
	31/12/2015	NA	0	1
	30/06/2015	NA	0	1
	31/12/2014	0	1	2
	30/06/2014	0	1	2
	31/12/2013	1	2	3
	30/06/2013	1	2	3
	31/12/2012	2	3	4
	30/06/2012	2	3	4
	31/12/2011	3	4	5
	30/06/2011	3	4	5
	31/12/2010	4	5	6
	30/06/2010	4	5	6
	31/12/2009	5	6	7
	30/06/2009	5	6	7
	31/12/2008	6	7	8
	30/06/2008	6	7	8
31/12/2007	7	9	9	

Source: Author's calculations

Note: The numbers indicate the age of the child at the end of the respective post-reform year based on their date of birth. The yellow boxes represent the treatment group (3 months to 3 years old); the red boxes represent the contaminated group (3 years to 6 years old); and the blue boxes represent the control group (6 years to 9 years old). Individuals in the treatment group have different treatment intensities depending on their birth year. In this analysis, I classify a child as treated in a given year regardless of treatment duration, e.g., even if a child receives only 6 months of treatment instead of 12, they are still considered treated for that year.

The treatment group comprises mothers whose youngest child is between 3 months and 3 years, corresponding to those eligible for the Free Childcare Scheme. For the control group, I select mothers with their youngest child between 6 and 9 years. This control group is chosen because, in the time period under investigation, such mothers were never eligible for free childcare, yet, they also had children whose age was very close to the those of the treated group.

Mothers whose youngest child is between 3 and 6 years do not constitute a valid control group, since they might have benefited from the policy in the years after the introduction of the reform when their children would still have been below the age of three. In other words, including this age cohort in the control group risks contamination between the treatment and control groups, except in cases where the mother birthed another younger child. Such contamination could upwardly bias employment outcomes in the control group if the policy had lasting effects, such as facilitating career entry or continuity, thereby leading to an underestimation of the true policy impact. Table 2 illustrates this point by mapping child age cohorts to the treatment, contaminated, and control groups across birth years and post-reform years.

The validity of the DD approach hinges on the common trends assumption which states that, in the absence of the policy, employment trends for both groups would have evolved similarly. As briefly mentioned earlier, several ALMPs aimed at “Making Work Pay” were introduced in 2014. These included the tapering of unemployment benefits, favourable tax incentives to take up work, the introduction of in-work benefits and the creation of Breakfast Clubs. However, while these policies likely contributed to the rise in female employment, they were implemented broadly and uniformly across the population. Therefore, it is reasonable to assume that both the treatment and control groups, as defined by the age of their youngest child, were similarly exposed. Moreover, as discussed in Section 2, the centres operating under the scheme provide substantial flexibility in operating hours. This reduces the likelihood that the estimated DD effects are driven by mechanical restrictions on childcare hours and instead supports an interpretation in terms of behavioural labour supply responses.

Still, the identifying assumption could be violated if the government was anticipating a change in behaviour when deciding to introduce the reform. Furthermore, if parents anticipated the policy and adapted their behaviour in advance, this too would affect the identification of the treatment effect. However, both of these potential issues can be reasonably judged as significantly unlikely to compromise the common trends assumption. Specifically, few details were known by late 2013 and it was not until the beginning of 2014 that details about the scheme were gradually released.⁹ Moreover, since eligibility is based on three of the most recent payslips (for employed parents) and the policy took effect in April 2014, it is rational to assume that behavioural responses primarily materialised in the year of implementation. That said, to further assess the credibility of the identification strategy, I run a robustness analysis in Section 7 using placebo tests that take into consideration only the pre-reform data.

Baseline Difference-in-Differences Specification

As in the study of Bettendorf, Jongen and Muller (2015), I estimate the treatment effect of the Maltese childcare reform by regressing female employment on year-fixed effects (a_t), group-fixed effects (γ_g), individual characteristics (X'_{it}), and a set of group-year treatment dummies (D_{gs}). The baseline DD specification is given by:

$$y_{igt} = a_t + \gamma_g + X'_{it}\beta + \sum_{s=2014}^{2016} \delta_s D_{gs} + \epsilon_{igt} \quad (1)$$

⁹ In a November 2013 news article, Mr Clyde Caruana – then Chairperson of Jobsplus – stated that “parents interested in the scheme would be given details on how to enrol early next year since a number of administrative issues are currently being addressed” (Malta Independent, 2013). This suggests that even shortly before its rollout, key logistical aspects of the Free Childcare Scheme were still being finalised.

The female employment outcome, y_{igt} , is a binary variable equal to one if mother i in group g (*treatment*) and year t enters paid employment, and zero otherwise. With regards to the regressors, the term a_t denotes year-fixed effects and captures the common employment trend of all mothers in the treatment and control groups. The term γ_g captures group-fixed effects to control for time-invariant differences in employment between the treatment and control groups. The vector X'_{it} includes individual characteristics, mainly mother's age; marital status; locality; number of children in the age bands 0-3, 3-6, 6-9 and 9-16; and an indicator for having a disabled child, to control for observable differences in the sample composition across groups and over time. The group-year dummies, D_{gs} , equal one if mother i belongs to the treatment group and is observed in year $s \in \{2014, 2015, 2016\}$. The year 2013, which precedes the introduction of the reform on 1st April 2014, serves as the reference category.

Given the binary nature of the outcome variable, I estimate a linear probability model (LPM), which performs well in estimating causal effects rather than predicting individual probabilities of employment (Angrist and Pischke, 2009). Moreover, since actual childcare take-up is not observed, the coefficients δ_s are interpreted as intention-to-treat (ITT) effects of eligibility in year s , relative to the pre-reform baseline in 2013. Equation (1) allows treatment effects to vary by year, enabling me to examine whether the policy impact evolves over time. When the annual treatment effects do not differ significantly from one another, I additionally estimate a pooled average treatment effect across the post-reform period. Equation (1) is estimated using ordinary least squares (OLS) and since I use administrative microdata that covers the relevant population, no weighting is applied. The Breusch-Pagan test indicates a rejection of the null hypothesis of homoscedasticity, and hence all regressions are estimated using robust standard errors.

Matched Difference-in-Differences

In addition to the standard DD specification, I estimate a matched DD model that combines stratification-based matching with DD. The purpose of this approach is to improve the comparability of the treated and control mothers prior to the reform, which thereby strengthens the plausibility of the common trends assumption. The intuition behind matched DD is that while the DD estimator in equation (1) differences out unobserved time-invariant heterogeneity, it may still be sensitive to imbalances in observable characteristics if there are any remaining systematic differences between the treatment and control groups before the policy change. Matching addresses this concern by reweighting the data so that the distribution of key pre-determined characteristics among control mothers more closely resembles that of treated mothers in the pre-reform period.

Specifically, I first stratify mothers into cells defined by a combination of pre-determined characteristics, namely *age band* (18-29, 30-39, and 40-52)¹⁰, *civil status*, and *the total number of children* (categorised as 1, 2, or 3 and more children). Next, using data from the pre-reform period (2012-2013), I compute the share of treated mothers relative to the total treated population of mothers for each cell. These cell-specific shares are subsequently used as weights in a weighted least squares estimation of the DD regression on the full individual-level sample. Observations belonging to cells that are more prevalent among treated mothers prior to the reform receive higher weight, while observations in cells with no treated counterparts receive zero weight. As in the baseline specification, both year-specific treatment effects and a pooled post-reform effect are estimated if the year-specific treatment effects do not differ significantly from one another. Standard errors for the matched DD models are obtained via non-parametric bootstrapping to account for the use of generated weights.

¹⁰ I use age bands rather than continuous age to reduce sparsity when constructing matched cells as exact ages vary considerably across individuals, making it difficult to find comparable untreated units for each treated individual. Similarly, I create a variable for a mother's total number of children by summing across the number of children in each age band that belong to the mother and then condensing the total number of children into three categories: 1 child, 2 children, or 3+ children.

5. Data Description

This study uses anonymised administrative microdata obtained from the Ministry for Social Policy and Children's Rights. The dataset covers the whole Maltese population included in the System for the Administration of Social Benefits (SABS). Specifically, it includes all individuals (male or female) who are recipients of any type of social security benefit in Malta, as well as any adults residing in the same household as the beneficiary. For each included individual, the dataset contains exhaustive information on their personal characteristics (age, gender, civil status, locality), household characteristics (household size, children's date of birth), employment status and social benefit receipt¹¹, recorded annually. Table A.1 in the Appendix describes the variables in the dataset.

The timeframe of this study is 2012-2016, with each year capturing the population as registered at end of year. The period of analysis is centred around the Free Childcare Scheme reform which came into effect on 1 April 2014, enabling me to study the common trend assumption crucial in a DD analysis. I restrict my analysis to two years before the reform due to major institutional shocks in earlier years such as the legalization of divorce in Malta in 2011. Data from earlier years would be subject to different collection methods or definitions, reducing comparability.

The dataset constitutes an unbalanced panel given its administrative nature and the focus on individuals receiving specific social benefits. In other words, as eligibility and take-up of social benefits change over time, individuals may enter or exit the dataset in different years. Nonetheless, individuals with continued receipt of specific social benefits can be tracked across the dataset through the use of randomly generated reference ID codes. ID codes were assigned to all observations in 2016. Hence, if the same individual appears in earlier years, the same ID was retrospectively applied. However, individuals who are not present in 2016, typically since their youngest child has reached the age of 16 and take-up of children's allowance has ended for this reason, cannot be matched to earlier records and are therefore recorded with a missing (NA) reference ID. As a result, only individuals who appear in 2016 can be reliably tracked earlier across time.

The main analysis will essentially adopt a repeated cross-section approach. To identify mothers, I restrict the sample to females receiving children's allowance. In Malta, mothers are automatically enrolled for children's allowance upon registration of their child's birth and are therefore included in the SABS database, making this a reliable and comprehensive way to identify mothers in the data by eliminating the possibility of selection bias. Mothers continue receiving children's allowance until their youngest child turns 16 years old.¹² Therefore, my selected sample constitutes mothers with a youngest child below 16 years old. From this filtered dataset, I then select my treatment group which consists of mothers with a youngest child between 3 months and 3 years old who would be eligible for free childcare. Furthermore, I restrict the analysis to mothers aged 20 to 50 years at the time of the reform¹³. This gives me around 57,854 observations for the treatment group. As a control group, I use mothers with a youngest child aged 6 to 9 years. Restricting the control group to mothers 20 to 50 years of age at the time of the reform, there are 31,873 observations in the control group.¹⁴ This control group is selected so that the youngest child is too old to have been eligible for free childcare, but as close as possible in age to the treated children so as to preserve comparability. I also consider mothers with a

¹¹ The Free Childcare Scheme involves payouts to the providers rather than the beneficiary and therefore, the benefit arising from the scheme is not observed in the data.

¹² If a mother's youngest child turns 16 years old, the mother receives no other type of social security benefit and neither does any other adult sharing the same household as her, that mother is dropped from the dataset.

¹³ A similar age category is selected in the papers of Bettendorf, Jongen & Muller, 2015; Boussein, 2019; Berlinski and Galiani, 2017; and Nollenberger & Rodriguez-Planas, 2015.

¹⁴ Treatment and control groups are filtered through dates of birth of the youngest child and not age, which is important as the reform occurred on 1 April (not 1 January). Furthermore, treatment and control groups are constructed for each year from 2012 to 2016, allowing for comparisons both before and after the 2014 reform.

youngest child of 9 to 12 years; however, as I shall show below, it is not a valid control group as they have a different pre-reform trend than my treatment group.

My outcome variable is a binary indicator capturing the mother's employment status, which is defined as being engaged in either full-time or part-time employment. The set of control variables used in this study include the age of the mother and the number of children in each of four different age bands (0-3; 3-6; 6-9; and 9-16 years old)¹⁵ along with four dummy variables.

The first two dummy variables are mutually exclusive dummies and created to capture the mother's civil status. The dummies are (i) *Married* which equals 1 if the mother is married and 0 otherwise and (ii) *Single* which equals 1 if the mother is single and 0 otherwise. The reference group (i.e., when both dummies are equal to 0) consists of mothers who are previously married, specifically those who are Divorced, Widowed, Legally Separated, De Facto Separated, or Separated Maintaining Wife.¹⁶ I include civil status as a control variable because married versus single mothers often face different caregiving and financial constraints such that civil status is likely to be associated with a mother's employment decisions.

The third dummy variable *Locality* accounts for geographical labour market differences between the islands of Malta and Gozo. This variable is equal to 1 if the mother resides in Malta and 0 if in Gozo. I include this control since Gozo is known to have relatively limited job opportunities and higher commuting costs to Malta, which may in turn affect the employment probability of mothers residing in Gozo.

The final dummy variable *DCA* (that is, *Disability Child Allowance*) is readily available in the dataset and equals 1 if the mother receives DCA and 0 otherwise. Receipt of DCA indicates that one of the mother's children, at minimum, is disabled. This is a relevant control because mothers of disabled children may face greater caregiving responsibilities and time constraints which can reduce their probability of being employed compared to mothers of non-disabled children.

Table 3 presents descriptive statistics for the treatment and control groups in both the pre-reform period (2012-2013) and the post-reform period (2014-2016).¹⁷ The year 2014 is part of the post-reform period because the reform came into effect during the year. Between these periods, the employment probability of mothers in the treatment group increased by roughly six percentage points, compared to a five-percentage-point increase among mothers in the control group. The "raw" DD estimate of one percentage point provides initial evidence that the Free Childcare Scheme may have positively influenced employment outcomes for the targeted population. A preliminary inspection of the table suggests that changes in control variables evolve similarly across the treatment and control groups.

For example, mothers in the control group are slightly more likely to receive DCA relative to the treatment group, both before and after the reform. There are also observable demographic differences between the groups. Firstly, and in line with expectations, mothers in the treatment group are, on average, five years younger than those in the control group. Secondly, the likelihood of being married is marginally lower among the treated group, while the probability of being single is much higher. Overall, civil status and locality evolve the same for both groups over time, with the exception of the single status for the control group which increases by a significant two percentage points after the reform. Finally, the treatment group has the highest number of children in the 0-3 years age category while the control group has the highest number of children in the 6-9 age category, as expected.

¹⁵ I specially construct controls for the number of children in different age bands, based on their age at the end of each calendar year. This is done using *Child_Post2008 X DoB* and *Child_Pre2009 X DoB* variables, as all children under 16 belonging to a mother are recorded in her row with their DoBs.

¹⁶ Mothers with an Undefined (U) status are dropped as there are only 69 of such mothers. This leaves me with 66,471 Married mothers; 17,040 Single mothers; and 6,216 mothers who are Divorced, Widowed, Legally Separated, De Facto Separated or Separated Maintaining Wife.

¹⁷ Table A.2 in the Appendix provides summary statistics for both the treatment and control groups, aggregated across the years 2012-2016.

Naturally, the control group has no mothers with a youngest child between 0-3 or 3-6 years. Overall, the number of children in the different age groups evolve the same over time, with a statistically significant decrease after the reform in the number of children between 9-12 years for both groups. This is expected given Malta's declining fertility rates.

Table 3
SUMMARY STATISTICS FOR TREATMENT AND CONTROL GROUPS BEFORE AND AFTER THE CHILDCARE REFORM

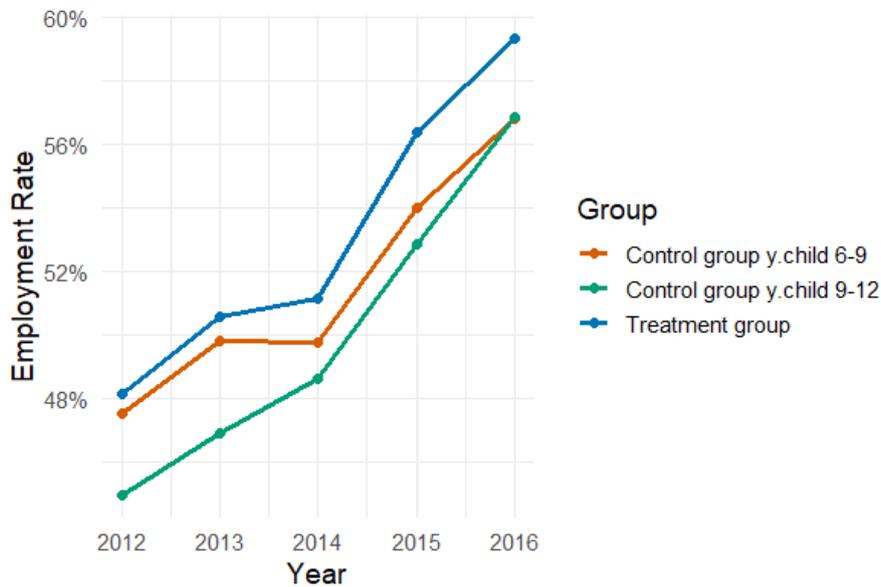
	Before	After	Difference
Mothers with a youngest child of 3 months – 3 years (N=57,854)			
Employed	0.49	0.56	0.06***
Has DCA	0.01	0.02	0.00***
Age (years)	31.51	32.31	0.80***
Married	0.73	0.74	0
Single	0.21	0.21	0
Locality: Malta	0.93	0.93	0
Number of Children 0-3	0.81	0.82	0.01***
Number of Children 3-6	0.48	0.46	-0.01**
Number of Children 6-9	0.19	0.2	0
Number of Children 9-16	0.2	0.17	-0.03***
Mothers with a youngest child of 6 years – 9 years (N=31,873)			
Employed	0.49	0.53	0.05***
Has DCA	0.03	0.04	0.01***
Age (years)	36.62	37.16	0.54***
Married	0.76	0.75	-0.01*
Single	0.14	0.16	0.02***
Locality: Malta	0.94	0.94	0
Number of Children 0-3	0	0	0
Number of Children 3-6	0	0	0
Number of Children 6-9	1.08	1.08	0
Number of Children 9-16	0.63	0.58	-0.05***

Source: Author's calculations

Note: All values have been rounded to two decimal places. * Denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

The DD method relies on comparing how outcomes evolve over time for the treatment and control groups. Chart 4 illustrates this comparison by plotting the employment rate of mothers in the treatment group (youngest child 3 months – 3 years), the control group (youngest child 6 – 9 years) and a potentially alternative control group (youngest child 9 – 12 years) over the years 2012 to 2016. The plot suggests that, prior to the reform, both the treatment and control groups exhibit similar upward trends in employment, consistent with the assumption that, absent the reform, the two groups would have evolved similarly. While the employment rate is always higher for the treatment group than the control group, this does not violate the identifying assumption, which concerns the evolution of outcomes over time rather than their absolute levels. This level difference is due to cohort effects: younger mothers (in the treatment group) generally have higher employment rates than older mothers (in the control group), consistent with the broader trends of rising female employment in Malta. In contrast, the rate of growth for the 9-12 group evolves differently from the treatment group in the pre-reform period, suggesting that this potential control group does not satisfy the common trends assumption and is therefore not a suitable control group.

Chart 4
RAW EMPLOYMENT TRENDS BY TREATMENT AND CONTROL GROUPS
(2012-2016)



Source: Author's calculations

6. Results

This section is structured into two main parts. First, I present the main findings, which estimate the treatment effect of the Maltese childcare reform on the employment probability of all treated mothers. Then, I provide four complementary analyses to deepen the interpretation of the main result. These include estimating heterogeneous treatment effects across subgroups of mothers, assessing whether the reform had permanent effects on employment, evaluating the reform's impact on social benefit receipt, and exploring the transition into employment by decomposing the overall increase in employment into the probability of maintaining employment for previously employed mothers and the probability of entering employment for those initially not employed.

6.1 Main Results

In this section, I estimate the effect of the Maltese childcare reform on the employment probability of all treated mothers. I start my analysis by estimating Eq. (1) to capture the effects of the Maltese childcare reform on female employment across both the aggregate post-reform period (2014-2016) and the individual post-reform years, using a DD framework. Therefore, I estimate two models: Model A where I restrict the treatment effect to be equal across all post-reform years¹⁸ and Model B where I allow for year-specific treatment effects. In both models, 2013 serves as the reference year. Column (1) of Table 4 presents the results for these models, while excluding individual and household characteristics. The estimated average treatment effect on the treated (ATT) across all post-reform years is 1.4 percentage points (pp) and statistically significant at the 10% level. The year-specific treatment effects grow over time, from 0.7 pp in 2014 to 1.8pp in 2016, with the latter estimate also significant at the 10% level.

¹⁸ I perform an F-test of the null hypothesis that the year-specific treatment effects are equal and fail to reject the null. This suggests that the differences in the year-specific coefficients are not statistically significant and provides justification for presenting the aggregate treatment effect across 2014-2016.

In Column (2), I augment the model to include individual-level covariates, mainly mother’s age, civil status, number of children by age band, locality, and an indicator for having a disabled child. The results are similar. The estimated ATT remains similar in both magnitude (1.3 pp) and statistical significance. The year-specific estimates follow a similar upward trend, with the 2016 effect (1.7 pp) remaining statistically significant. As this is a linear probability model, all coefficients are interpreted as percentage point changes in the probability of employment.

Overall, the results in Columns (1) and (2) suggest a modest and increasing positive effect of the Maltese childcare reform on mothers’ employment in the immediate post-reform period. Additionally, the insignificant employment effect in 2014 aligns with the evidence discussed in Section 2 which shows that take-up of the scheme was gradual rather than immediate. It is also very likely that most mothers who benefitted from free childcare in 2014 already had their child enrolled in paid childcare prior to the reform, limiting the potential for immediate labour market responses.

Table 4
TREATMENT EFFECTS ON FEMALE EMPLOYMENT PROBABILITY: ALL MOTHERS

	(1) OLS No covariates	(2) OLS Covariates	(3) M-DD Covariates
Model A			
treat 2014-2016	0.01377* (0.00710)	0.01279* (0.00698)	0.02449*** (0.00897)
Model B			
treat2014	0.00664 (0.00959)	0.00484 (0.00941)	0.00894 (0.01205)
treat2015	0.01652* (0.00943)	0.01647* (0.00928)	0.03275*** (0.01170)
treat2016	0.01805* (0.00954)	0.01692* (0.00942)	0.03175*** (0.01223)
Observations	89,727	89,727	89,727

Source: Author’s calculations

Note: *treat 2014-2016* reports the average treatment effect on the treated (ATT) across all post-reform years (2014–2016), relative to the base year 2013. *treat2014*, *treat2015*, and *treat2016* report year-specific treatment effects, each relative to 2013. For Columns (1) and (2), two separate regressions are estimated: one for aggregate ATT and one for the year-specific treatment effects. Robust standard errors are shown in parenthesis; standard errors in column (4) are bootstrapped. * Denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. All regressions include individual characteristics (excluding Column (1)), as well as group- and year- fixed effects, though these are not reported in the table. The number of children in the age band 0-3 years is excluded as a covariate to avoid high correlation with the treatment status.

The matched DD results are reported in Column (3) and indicate highly statistically significant and positive effects in 2015 and 2016 of 3.28 and 3.18 pp respectively, while the 2014 effect remains statistically insignificant. The aggregate treatment effect is also statistically significant at the 1% level and suggests that the probability of female employment increased by 2.4 percentage points following the reform. Therefore, after improving the covariate balance through weighting, the Maltese childcare reform is associated with substantially larger increases in the employment probabilities for treated mothers in the medium run.

Taken together, the results point to the possibility that the initial unweighted DD estimates may have understated the reform's true impact due to residual differences between the treated and control groups. The stronger results obtained by the matched DD model point to a more substantial impact of the reform on mothers' employment once improved group comparability is ensured. These findings reinforce the conclusion that the policy had a meaningful and growing effect on the employment of mothers with young eligible children over time.

6.2 Complementary Results

Heterogeneous Treatment Effects across Subgroups of Mothers

While average treatment effects provide a broad overview of the aggregate impact of the Maltese childcare reform, they may obscure substantial variation across different subgroups of mothers who face different structural barriers to entering or remaining in employment. By estimating heterogeneous treatment effects, my study contributes to the previous literature reviewed in Section 3 while also offering a more nuanced understanding of the policy's distributional impacts.

I look at heterogeneous treatment effects for different subgroups of mothers, mainly single mothers; married mothers; mothers with a disabled child; and mothers with multiple children. To do so, I extend Eq. (1) and estimate the following specification:

$$y_{igt} = a_t + \gamma_g + X'_{it}\beta + \sum_{s=2014}^{2016} \delta_s D_{gs} + \theta_0(treat_g \times Z_i) + \sum_{s=2014}^{2016} \theta_s(D_{gs} \times Z_i) + \epsilon_{igt} \quad (2)$$

In this model, Z_i denotes the subgroup of interest (e.g., single mothers). Importantly, Z_i is a subset of the broader set of covariates X_{it} . Therefore, the interaction term, of $treat_g$ and Z_i , accounts for baseline differences between the subgroup and the reference group prior to the reform. The interaction terms, of D_{gs} and Z_i , capture how the treatment effect in each post-reform year $s \in \{2014, 2015, 2016\}$ differs for the subgroup relative to the reference group. The coefficients θ_s on these interaction terms therefore reflect the *differential* treatment effect for the subgroup, while δ_s captures the treatment effect for the reference group. As such, the total treatment effect for a given subgroup in year s is the sum of $\delta_s + \theta_s$. By estimating heterogeneous treatment effects in this way, I am able to comprehensively test for statistical differences between subgroups (e.g., single vs non-single mothers).

The results of Eq. (2) are presented in Table 5. Column (1) indicates that, among treated mothers, single mothers experienced a significantly greater increase in employment probability – between 2.5 and 3.3 pp – compared to non-single mothers (i.e., married or previously married). Consistent with this, Column (2) shows that while married mothers had overall positive treatment effects, these were generally smaller than those of single mothers and were not statistically significant across all post-reform years.

In contrast, Column (3) shows no statistically significant differential effects between mothers with and without a disabled child, suggesting that the reform was relatively accessible to children with special needs, thereby equalising its impact across families. However, the limited number of observations for mothers with disabled children may have reduced statistical power and obscured the true effects. Columns (4) and (5) examine heterogeneity by the number of children. While no significant differences emerge in 2014 and 2015, by 2016, mothers with two children and mothers with three or more children were respectively 4.4 and 3.7 pp more likely to be employed than mothers with only one child.

Overall, these findings suggest that the Maltese childcare reform had a particularly pronounced effect for subgroups of mothers who, as identified in the literature, face greater barriers to employment. Consistent with prior studies (e.g., Bettendorf, Jongen and Muller, 2015; Huebener, Pape and Spiess, 2020; Brewer et al., 2022; Goux and Maurin, 2010; Cascio, 2009; Givord and Marbot, 2015; and Nollenberger and Rodriguez-Planas, 2015), I find that single mothers and, to a lesser extent, mothers with multiple children experienced a larger increase in their employment probability relative to married

mothers and single-child mothers. The delayed significance for the latter group, only emerging in 2016, may reflect delayed adjustment to formal childcare access or evolving family dynamics over time.

Given the large heterogeneous effects for single mothers and mothers with multiple children, the reform appears to have generated disproportionately stronger employment gains among disadvantaged subgroups, specifically among those with a lower pre-reform employment rate relative to their respective reference groups. As such, it is informative to assess the extent to which the reform contributed to narrowing the pre-existing employment disparities, thereby shedding light on its equity implications. To this end, I compare the estimated differential treatment effects (θ_s) to the size of the pre-reform employment gap observed in 2013 between each subgroup and its reference group. This yields the share of the original employment gap that was effectively “closed” by the reform.

The results indicate that single mothers were 11.5 percentage points less likely to be employed than non-single mothers prior to the reform. Therefore, based on the estimated treatment effects, approximately 21.7 to 26.1 % of this employment gap was closed following the reform. Among mothers with multiple children, those with two and three or more children were, respectively, 11.2 and 26.2 percentage points less likely to be employed than single-child mothers before the reform. The reform closed these gaps by as much as 39.3% and 14.1 % respectively. The full results for the disadvantaged subgroups by treatment year are reported in Table A.4.

It is also interesting to examine whether the Free Childcare Scheme had differential effects on low-income mothers. To identify low-income mothers in my sample, I exploit the type of Children’s Allowance received. In other words, I classify mothers as low-income if they are in receipt of the means-tested Children’s Allowance. Whether a mother receives the means-tested or fixed-rate Children’s Allowance depends on whether her total household income two periods prior fell below a certain threshold (e.g., €23,983 in 2012). This two-year lag is crucial as it ensures that the income status is pre-determined and not affected by the introduction of the scheme, thereby preserving the exogeneity required for causal interpretation.¹⁹ Following this logic, I drop 2016 from the analysis since the income status classification would reflect income measured in 2014 which corresponds to the year when the reform was introduced.

However, despite addressing the endogeneity concerns in the definition of the low-income status, applying the interacted DD framework in equation (2) for this subgroup of mothers remains problematic. Income is a variable that tends to revert to the mean, meaning that mothers classified as high income before the reform are mechanically more likely to see their income fall over time, while low-income mothers are more likely to improve, regardless of the policy. As a result, the differences in the post-reform outcomes across the income groups may reflect mechanical reversion rather than the effect of the Free Childcare Scheme, which in turn undermines the common trend assumption required for the interacted DD estimation. To avoid this issue, I estimate treatment effects separately for low-income and non-low-income mothers using split-sample regressions. This approach focuses on within-group changes over time and avoids relying on potentially misleading cross-group comparisons driven by mean reversion.

Table 6 reports the split-sample DD estimates obtained by estimating equation (1) separately for low-income and non-low-income mothers. For both groups, the estimated post-reform treatment effects are small and statistically insignificant. This suggests that, when treatment effects are identified with the income groups, there is no strong evidence that the Free Childcare Scheme had a differential impact on employment by income status.

¹⁹ Using income in period t to define the income status would create a bad control problem because income is likely to respond to the reform itself. In other words, if the reform encourages mothers to enter employment, this will mechanically raise their earnings and may shift some mothers out of the low-income category. Therefore, conditioning on income measured after the reform would amount to conditioning on an outcome of the policy, which might bias estimates by partially absorbing the treatment effect or by inducing spurious correlations.

Table 5

HETEROGENEOUS TREATMENT EFFECTS ON FEMALE EMPLOYMENT PROBABILITY: SUBGROUPS OF MOTHERS

	(1)	(2)	(3)	(4)	(5)
Subgroup	Single	Married	Presence of disabled children	Two children	Three or more children
Variable					
Reference group	<i>Non-single</i>	<i>Non-married</i>	<i>No disabled child</i>	<i>One child</i>	<i>One child</i>
treat2014	0.00065 (0.00987)	0.02364* (0.01312)	0.00500 (0.00943)	0.00515 (0.01106)	0.00515 (0.01106)
treat2015	0.01092 (0.00969)	0.03530*** (0.01297)	0.01631* (0.00931)	0.00865 (0.01074)	0.00865 (0.01074)
treat2016	0.01128 (0.00981)	0.02221* (0.01335)	0.01723* (0.00945)	-0.00736 (0.01073)	-0.00736 (0.01073)
treat2014_subgroup	0.02450* (0.01369)	-0.02393* (0.01262)	-0.01592 (0.04988)	0.00057 (0.01222)	0.01730 (0.01668)
treat2015_subgroup	0.03253** (0.01334)	-0.02412* (0.01233)	-0.00780 (0.04389)	0.01149 (0.01178)	0.01865 (0.01636)
treat2016_subgroup	0.02593* (0.01366)	-0.00899 (0.01260)	-0.04022 (0.04338)	0.04423*** (0.01175)	0.03651** (0.01792)
Observations	89,727	89,727	89,727	89,727	89,727

Source: Author's calculations

Note: Estimates for Columns (5) and (6) are estimated in the same regression. Similar to the main analysis, 2013 serves as the reference year in all models. * Denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. All regressions include individual characteristics as well as group- and year- fixed effects, though these are not reported in the table. The number of children in the age band 0-3 years is excluded as a covariate to avoid high correlation with the treatment status.

Table 6
HETEROGENEOUS TREATMENT EFFECTS ON FEMALE EMPLOYMENT
PROBABILITY: LOW-INCOME AND NON-LOW-INCOME MOTHERS

	(1) Low-income mothers	(2) Non-low-income mothers
treat2014	0.00582 (0.01265)	-0.00336 (0.01231)
treat2015	0.00640 (0.01313)	0.01800 (0.01182)
Observations	34,730	37,163

Source: Author's calculations

Note: *treat2014* and *treat2015* report year-specific treatment effects relative to 2013, estimated separately for low-income and non-low-income mothers. I use the type of Child Allowance (CA) received (means-tested or fixed-rate) as a proxy to distinguish low-income from non-low-income mothers. Since CA eligibility is based on household income lagged by two years, the 2016 classification reflects income in 2014, after the childcare reform was introduced. Given the likely endogeneity between post-treatment income and employment, including 2016 could bias the analysis. Therefore, the income subgroup analysis is restricted to the 2012–2015 period. Robust standard errors are shown in parenthesis. *Denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. All regressions include individual characteristics, as well as group- and year- fixed effects, though these are not reported in the table. The number of children in the age band 0-3 years is excluded as a covariate to avoid high correlation with the treatment status.

Permanent Employment Effects

In light of the fact that the Free Childcare Scheme applies to a maximum period of approximately three years, it is particularly interesting to understand whether such temporary access to free childcare can have a lasting effect on the employment probability of treated mothers. To this end, I estimate a complementary DD model motivated by the hypothesis that initial exposure to subsidised childcare may induce permanent changes in labour market attachment. To conduct such an analysis, I consider mothers whose youngest child was eligible for the scheme in 2014, and possibly 2015, but who aged out of eligibility by 2016.

For this purpose, I construct a new treatment group consisting of mothers whose youngest child was born between 1st April 2011 and 31st December 2012. I therefore include mothers whose children were between 3 months and 3 years of age when the reform was implemented on 1st April 2014, making them eligible for free childcare at the time, but who would have dropped out of the eligibility window by 1st January 2016. I then check whether these mothers, despite no longer having access to free childcare in 2016, exhibit a greater increase in employment levels between 2013 (pre-reform) and 2016 (post-reform) compared to mothers in the control group, thus helping me shed light on whether this reform has had a persistent effect on maternal employment rates.

The control group remains the same as in the main analysis and includes mothers whose youngest child was between 6 and 9 years of age, and therefore, never eligible to free childcare. To control for potential pre-existing (or baseline) differences in employment levels between the treatment and control groups, I use 2013 as the pre-treatment year. Importantly, I construct a placebo treatment group for 2013 using an analogous window of child birthdates (1st April 2008 to 31st December 2009), simulating a policy that had been hypothetically introduced three years earlier.

The estimation results for this model are presented in Column 1 of Table 7. This model includes the conventional binary treatment indicator (*treat x post*) and is used to estimate whether the reform had a

permanent effect of the employment probability of treated mothers.²⁰ The statistically insignificant coefficient estimate of 0.0048 implies that the employment rate of previously treated mothers was not statistically different from that of mothers who were never eligible to free childcare, suggesting that the reform did not have a discernible lasting effect on employment once eligibility ended.

Table 7
PERMANENT EFFECTS OF PARTIAL EXPOSURE TO THE REFORM ON FEMALE EMPLOYMENT PROBABILITY

	(1) Binary Treatment Model OLS	(2) Exposure Fraction Model OLS
treat	-0.01002 (0.02735)	
post	0.06156*** (0.00888)	0.06580*** (0.00818)
treat x post	0.0048 (0.01351)	
exposure_frac		-0.01951 (0.02018)
exposure_frac x post		-0.01015 (0.02113)
Observations	21,673	21,673

Source: Author's calculations

Note: Analysis is restricted to solely 2013 and 2016. Both models control for individual characteristics but are not reported in the table. Robust standard errors are shown in parenthesis. * Denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. The number of children in the age band 0-3 years is excluded as a covariate to avoid high correlation with treatment status.

In a further effort to refine the analysis, I estimate a second model that explores heterogeneous treatment intensity. In particular, through this model I aim to capture whether a longer exposure duration to the treatment is associated with more persistent employment outcomes. To this end I construct a proxy for treatment intensity that captures the extent of overlap between the youngest child's age and the eligibility window. Since children are eligible for subsidised childcare from age 3 months to 3 years, I calculate the fraction of this eligibility window that coincided with the policy period of interest (1st April 2014 to 31st December 2015). The maximum exposure intensity of mothers during this period is of 21 months. On average, mothers are found to have been exposed to around 10.7 months of free childcare, equivalent to 51.3% of the full eligibility window.

The results for this second model are presented in Column 2 of Table 7. Here the interaction term (*exposure_frac* × *post*) estimates the marginal effect of additional exposure months on the employment probability after eligibility elapses.²¹ The statistically insignificant estimate of -0.01015 implies that mothers who were fully exposed to the policy (i.e. for 21 months) did not experience a statistically significant change in employment probability after the reform, though again this effect is statistically

²⁰ The coefficient on *treat* reflects the pre-existing differences in employment between mothers whose youngest child was eligible for free childcare in 2014–2015 (treatment group) and mothers in the control group, as measured in the pre-reform year 2013. Meanwhile, the coefficient on *post* captures aggregate changes in female employment between 2013 and 2016 that are common to both groups, reflecting economy-wide trends or other time-specific factors unrelated to the reform.

²¹ The coefficient on *exposure_frac* × *post* measures the probability of employment increases per unit decrease in the exposure fraction. For example, if the mother's youngest child was eligible for 10.5 months of free childcare, *exposure_frac* = 0.5 and therefore that mother is estimated to have experienced a 0.505 pp (1.01 × 0.5) decrease in employment probability after exposure.

insignificant. This means that there is no evidence that mothers who benefitted from longer exposure while eligible exhibit a higher employment probability once eligibility has expired.

However, since both models rely on employment outcomes in a single post-treatment year (i.e., 2016), the statistical power of this analysis is inherently limited compared to the benchmark model. As a result, small or gradual persistent effects may go undetected due to the narrow observation window after eligibility ends.

Social Benefit Receipt Effects

Beyond facilitating mothers with young children to enter employment, the childcare reform may have also generated fiscal externalities through potential government savings from decreased social benefit claims. Assessing these broader fiscal implications is critical for evaluating the overall cost-effectiveness and long-term sustainability of the reform. To this end, I examine the effect of the Maltese childcare reform on the probability of receiving specific social benefits. Specifically, I estimate Eq. (1) including covariates but replace the binary employment outcome with four alternative binary variables capturing whether a mother receives Social Assistance (SA), Unemployment Assistance (UA), Social Assistance for Single Unmarried Parents (SUP), or the Special Unemployment Benefit (SUB).²² The results of these four separate binary regressions are presented in Table 8.

Table 8
TREATMENT EFFECT ON PROBABILITY OF BEING A SOCIAL BENEFIT
RECIPIENT: ALL MOTHERS

	(1) SA	(2) UA	(3) SUP	(4) SUB
Model A				
treat 2014-2016	-0.00535* (0.00277)	-0.00078 (0.0005)	-0.01218*** (0.00301)	-0.00078 (0.00050)
Model B				
treat2014	-0.00165 (0.00392)	-0.00088 (0.00070)	-0.01281*** (0.00411)	-0.00022 (0.00041)
treat2015	-0.01152*** (0.00342)	-0.00073 (0.00064)	-0.01127*** (0.00411)	0.00023 (0.00036)
treat2016	-0.0026 (0.00337)	-0.00075 (0.00067)	-0.01249*** (0.00399)	-0.00066* (0.00038)
Observations	89,727	89,727	89,727	89,727

Source: Author's calculations

Note: Treat 2014-2016 reports the average treatment effect on the treated (ATT) across all post-reform years (2014–2016), relative to the base year 2013. Treat 2014, Treat 2015, and Treat 2016 report year-specific treatment effects, each relative to 2013. For each column, two separate regressions are estimated: one for aggregate ATT and one for the year-specific treatment effects. Robust standard errors are shown in parenthesis. * Denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. All regressions include individual characteristics, as well as group- and year-fixed effects, though these are not reported in the table. The number of children in the age band 0-3 years is excluded as a covariate to avoid high correlation with the treatment status.

²² These social benefit schemes are selected as outcomes because they are available in the dataset. Information on each social benefit scheme can be accessed on <https://socialsecurity.gov.mt/>.

The most consistent and statistically significant reduction in benefit receipt following the childcare reform is observed for the SUP. In Model A, which estimates a pooled treatment effect across all post-reform years (2014-2016)²³, the probability of receiving SUP decreases by approximately 1.22 pp, significant at the 1% level. Model B, which allows for year-specific treatment effects, confirms that this negative effect persists across all three post-reform years. These findings suggest that the reform was particularly effective in enabling single mothers below a certain capital-means test to exit welfare dependency.

For the general SA, Model A indicates a modest but significant reduction (−0.54 pp) while the disaggregated year-specific treatment estimates in Model B show that only the 2015 effect is statistically significant (−1.12 pp), with smaller and statistically insignificant effects in 2014 and 2016. There is no evidence of significant changes in the receipt of Unemployment Assistance (UA) or the Special Unemployment Benefit (SUB), as the coefficients in both models are close to zero and lack statistical significance. The absence of effects on UA and SUB may be because these benefits are typically claimed by the registered unemployed head of the household, i.e., typically the father.

Overall, these results imply that the childcare reform not only supported female employment but also contributed to slight moderate fiscal savings by reducing dependence on certain targeted social assistance programs, particularly among single-parent households.

Decomposing Employment Effects: Entry vs Retention

In order to better understand the dynamics behind the aggregate change in the employment probability of treated mothers, I decompose the overall increase in maternal employment into two distinct transitions: (1) the probability that previously employed mothers continue employment and (2) the probability that previous non-employed mothers enter employment, in the following year. This decomposition allows me to assess whether the observed increase in employment probability, discussed in Section 6.1, was driven more by retention of existing working mothers or entry of previously inactive mothers into employment.

I make use of the panel structure of my dataset by dropping all individuals with an invalid ID reference code and then sort the data by ID and year. I proceed to create a lagged variable, *empl_nextyear*, which captures the employment status of each mother in the subsequent year. This step necessarily results in missing values for the final year of the panel (2016), as employment in the next year (2017) is not observed. I then retain only observations for which the next year's data is available and corresponds to the following calendar year, ensuring a clean year-on-year transition.

Next, I split the sample into two groups. First, I identify mothers who were employed in the current year and had non-missing data for the next year. For this subgroup, I estimate a DD model using Eq. (1) on the outcome *empl_nextyear* to capture the reform's effect on the probability of continued employment for mothers already in employment. The results, presented in Column (1) of Table 9, show positive but small and statistically insignificant estimates. This suggests that the reform did not significantly affect the probability of continued employment among already employed mothers.

Second, I repeat the same procedure for mothers who were not employed in the current year to estimate the reform's effect on the probability of entering employment in the next year. The results in Column (2) show a different pattern. While there is no significant effect in 2014, the coefficient for 2015 is positive and statistically significant, suggesting that the reform increased the likelihood of employment entry in 2016 among non-employed mothers by approximately 2.2 pp.

These findings imply that the observed aggregate rise in employment among treated mothers was primarily driven by employment entry of treated mothers who were previously not employed. This

²³ I perform an F-test of the null hypothesis that the year-specific treatment effects are equal and fail to reject the null for all of the four outcomes.

indicates that the policy has been particularly effective on the extensive margin, with a more limited impact on the intensive margin, in boosting female employment rates. It also aligns with the heterogeneous treatment effects discussed earlier, suggesting that the reform was particularly effective in enabling more vulnerable subgroups, such as single mothers and multiple child mothers who were less likely to be employed prior to the reform, to enter employment.

Table 9
TREATMENT EFFECT ON PROBABILITY OF CONTINUED EMPLOYMENT (COLUMN 1) VERSUS EMPLOYMENT ENTRY (COLUMN 2)

	(1) Employed Mothers	(2) Non-Employed Mothers
treat2014	0.01 (0.00831)	0.00227 (0.01049)
treat2015	0.00387 (0.00883)	0.02167** (0.01075)
Observations	26,265	24,374

Source: Author's calculations

Note: The sample is restricted to mothers with valid identifiers and non-missing employment data in both the current and following year. Robust standard errors are shown in parenthesis. *Denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. All regressions include individual characteristics, as well as group- and year-fixed effects, though these are not reported in the table. The number of children in the age band 0-3 years is excluded as a covariate to avoid high correlation with the treatment status.

7. Robustness Checks

While visual inspection of the raw employment trends presented in Chart 4 seems to support the common trends assumption, I conduct several robustness checks using both formal pre-trend tests as well as a statistical power test to further verify the validity of the identifying assumption.

Firstly, I run a formal pre-trend test using only pre-reform data from 2012 and 2013. Specifically, I estimate a DD specification where 2012 serves as the reference year and 2013 is treated as a “placebo” post-treatment year. The model includes year-fixed effects, a treatment-group indicator, and an interaction between the treatment indicator and a 2013 dummy (i.e., *Treat x 2013*), which captures any differential change in employment between treatment and control groups before the reform. A significant coefficient would suggest diverging trends in the pre-period. However, as shown in Appendix Table A.3, the placebo coefficient is close to zero and statistically insignificant, both without controls and with controls, providing no evidence of differential pre-trends.

Second, I conduct an event study as a more encompassing test to analyse dynamic treatment effects by allowing for varying coefficients for both the leads and lags surrounding the reform. The year 2013, which precedes the reform, serves as the reference period, meaning that all coefficients for the interaction term reflect the treatment effect in their respective year relative to 2013. I control for year-specific shocks by including year-fixed effects in the model. Table 10 presents the results. Particularly, the table shows that the difference between treatment and control groups in the pre-reform year 2012 relative to the reference year 2013 is statistically insignificant, once again supporting the common trends assumption. It also shows that during the first year of the reform (2014), employment increased marginally and by 2015 and 2016, the employment effect became both larger and statistically significant at the 1% level, indicating that the reform's impact strengthened over time. A graphical representation of the event study is also provided in Chart 5.

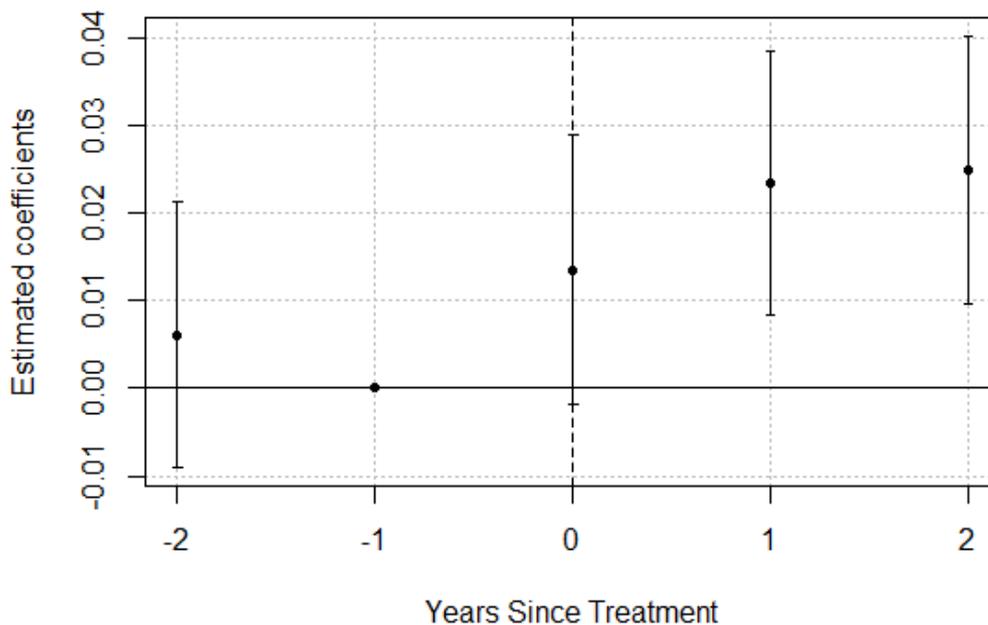
Table 10
EVENT STUDY RESULTS

Year relative to the reform	Estimate (Std. Error)
2 years before	0.0061 (0.0078)
Year of reform	0.0136 (0.0078) *
1 year after	0.0235 (0.0077) ***
2 years after	0.0250 (0.0078) ***
Year-Fixed Effects	YES
Adj. R ²	0.0061
RMSE	0.4978

Source: Author's calculations

Note: The reference year is 2013, with coefficients for each treatment year relative to 2013. The purpose of the event study is to check parallel trends so individual-level controls are not needed here. Robust standard errors are shown in parenthesis. * Denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Chart 5
EVENT STUDY PLOT: DYNAMIC EFFECT OF THE MALTESE CHILDCARE REFORM ON FEMALE EMPLOYMENT



Source: Author's calculations

Finally, Roth (2022) argues that the standard significance tests and event study plots often exhibit limited statistical power such that they fail to detect statistically significant pre-trend differences unless these are relatively large. Therefore, to strengthen the credibility of the common trends assumption, he

recommends conducting a statistical power test. For this I use the *pretrends* package in R.²⁴ This test evaluates whether my empirical design has sufficient power to identify meaningful violations of common trends in the pre-reform period. Specifically, it computes the minimum slope of a linear deviation from common trends that would be detectable with 80% power. In my case, the result is 0.0218²⁵, meaning that my design can reliably detect annual differences in employment trends of at least 2.18 pp 80% of the time. Larger deviations are more likely to be identified, while smaller ones may remain undetected. Since my treatment estimates exceed this level, this result is reasonable and supports the idea that deviations from common trends do not render the causal effects insignificant.

8. Conclusions

The results of this study demonstrate that the Maltese childcare reform likely led to a measurable improvement in the probability of mothers' employment, especially over the medium term. The overall average treatment effect of approximately 1.3 to 2.4 percentage points suggests a modest but meaningful policy impact during the initial post-reform period. These findings confirm that the scheme's work-conditional design effectively incentivised employment entry among eligible mothers, though by construction, its benefits were confined to those already attached or willing to attach to the labour market. Notably, the main effects appear to grow over time, with larger and statistically significant effects emerging by 2016, supporting the interpretation that labour market responses to childcare subsidies may take time to materialise due to both behavioural and logistical adjustments.

When placed in the context of international evidence, our Maltese estimates fall near the middle of the distribution of impacts observed in comparable childcare reforms. The magnitude of the effects is similar to those reported by Bettendorf, Jongen, and Muller (2015), Brewer et al. (2022), and Nollenberger and Rodriguez-Planas (2015). They are, however, smaller than the sizeable impacts documented in Baker, Gruber, and Milligan (2008) and Lefebvre and Merrigan (2008), yet larger than those for countries such as Sweden (Lundin, Mörk, and Öckert, 2008), Norway (Havnes and Mogstad, 2011), France (Givord and Marbot, 2015), and Germany (Huebener, Pape, and Spiess, 2020). Nonetheless, direct comparisons across studies remains challenging due to differences in policy design and treated populations.

A plausible explanation for the relatively modest impacts in Malta lies in the country's strong informal care networks, where grandparents, relatives, neighbours, and friends frequently provide care. In a small, geographically compact, and close-knit society, such informal arrangements are both accessible and culturally embedded. In fact, at the time the scheme was introduced, the formal employment rate among older females was very low, with less than a quarter of women above age 55 reported in employment (Eurostat, 2026a). Consequently, although the scheme sharply reduced the cost of formal childcare, its marginal impact on total childcare use, and thus female employment, may have been dampened by the presence of informal care alternatives. In future years, the higher employment rates for younger cohorts of women will lower this availability.

The findings from the matched DD model, which yields larger employment effects, highlight the importance of ensuring close comparability between treated and control groups. Taken together, the main results suggest that unweighted DD estimates may understate the true causal effect of the reform due to residual pre-treatment differences that matching helps to mitigate.

²⁴ The *pretrends* package, developed by Jonathan Roth (2022), facilitates power analysis for event study designs in DD settings. Although it is not yet available on CRAN, it can be reliably installed from GitHub using the *remotes* package. Full documentation is provided on the package's [GitHub repository](#).

²⁵ I coded the power test using the tips of Arutyunov, V. (n.d.). *Testing for pre-trends in DiD and Event Studies*. Available at: <https://tilburgsciencehub.com/topics/analyze/causal-inference/did/pretrends/> [accessed on May 9, 2025].

Importantly, the complementary analysis of heterogeneous treatment effects in this study reveals that the reform was likely more effective for certain subgroups of mothers. Single mothers and mothers with multiple children experienced substantially larger employment gains than their respective counterparts. This heterogeneity aligns with prior research on childcare policy and maternal employment while also novelly demonstrating that universal designs, when tied to employment activation, can achieve equitable outcomes without the stigma or bureaucratic hurdles of means-tested programmes. The reform also appears to have contributed to closing pre-existing employment gaps for these subgroups, suggesting that it performed well on equity grounds.

Conversely, there is no evidence of statistically significant differential employment effects by income status. Additionally, the absence of significant differential effects for mothers with a disabled child, while encouraging in terms of equal access, warrants cautious interpretation due to the limited number of observations for this subgroup which may reduce statistical power, potentially masking real effects.

Additionally, the study finds that the policy's effectiveness stemmed from enabling employment entry rather than retention, suggesting that it successfully reached previously inactive mothers during the eligibility period. Furthermore, while fiscal spillovers through reduced welfare claims, such as a 1.2 percentage point decline in the probability of being a SUP recipient, were non-trivial, the absence of persistent effects post-eligibility underscores a structural limitation of work-conditional designs: once childcare ceases to be free outside formal employment, mothers may revert to inactivity if jobs are unstable or poorly paid. This therefore implies that the long-term impacts of the Free Childcare Scheme depend critically on complementary policies that extend support beyond the narrow window of eligibility.

Despite this study's important contributions, there are several limitations in this study that could potentially be addressed in future research. First, the analysis relies on short- to medium-term data from the immediate post-reform period (2014–2016). Future research could extend this study by lengthening the observation window to assess the longer-term impacts of the reform. This extension would be particularly relevant given the steady increase in the scheme's enrolment rates over time which, amongst other things, likely also reflects greater uptake of free childcare by foreign nationals working in Malta, following the significant influx of foreign workers since the mid-2010s. Consequently, a promising direction for future research would be to disaggregate the effects of the scheme for Maltese and non-Maltese mothers, thereby shedding light on whether the policy's labour market impacts differ by nationality. Second, future research could consider broader economic outcomes beyond female employment, such as earnings, hours worked, and fertility rates. Data from the Labour Force Survey (LFS) indicate that the share of employed women aged up to 39 working part-time declined from around 22% in 2013 to approximately 17% in 2024 (Eurostat, 2026b). An important avenue for future research would therefore be to assess the extent to which this reduction in part-time work, and the associated increase in hours worked, can be causally attributed to the scheme.

Third, the absence of consistent childcare uptake data before and after the reform constrains the ability to implement a two-stage model that could trace the full causal pathway from the policy intervention to maternal employment outcomes. This limitation arises because, before the introduction of the scheme, childcare services were not publicly subsidised, and as such, no systematic records were collected by public authorities. Without comparable pre-reform data on formal childcare usage, it is difficult to empirically assess whether changes in employment were directly mediated by increased access to or uptake of formal childcare services.

In sum, this study provides robust empirical evidence that the introduction of the Free Childcare Scheme in Malta led to meaningful improvements in maternal employment, particularly among socioeconomically disadvantaged groups who were able to meet the scheme's employment-related eligibility conditions. While the short- and medium-term impacts on overall employment rates and fiscal returns appear modest, the findings highlight the potential of universal, work-conditional childcare policies to promote labour market inclusion and reduce structural inequalities. Importantly, the observed medium-term gains and increasing enrolment over time suggest that the full benefits of the scheme

may only emerge gradually. Although data limitations constrain a full understanding of the underlying mechanisms, the evidence points to childcare affordability and conditional access as key levers for activating latent female labour supply. In this regard the study contributes to a growing body of literature affirming that well-designed childcare policies can function as both social and economic investments. Looking ahead, continued monitoring of long-term outcomes, particularly those relating to the long-term impacts of the reform on female employment, earnings, hours worked, fertility, and child development, will be essential for a comprehensive evaluation of the scheme's broader societal impact. Ultimately, the success of such initiatives in Malta will depend not only on reducing out-of-pocket childcare costs. Rather, it will also depend on creating a wider enabling environment that supports sustainable employment for all parents, ensuring that work-conditional childcare remains a genuine pathway to long-term labour market attachment rather than a temporary incentive.

9. References

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Appendix

Table A.1
DESCRIPTION OF DATASET VARIABLES

Variable Name	Description	Data Type
Cross-Year ID	Unique identifier assigned to individuals that allows tracking across different years (datasets)	Numerical (ID)
House#	Identifier for the household to which the individual belongs	Categorical (ID)
h_size	Total number of individuals living in the household but excluding children of any age.	Numerical
locality	Town of residence (in Malta or Gozo)	Categorical
gender	Gender of the individual (M = Male, F = Female)	Categorical
status	Civil status (S = Single, M = Married, W = Widowed, L = Legally Separated, D = Divorced, F = De Facto Separated, P= Separated Maintaining Wife[1], U = Unknown)	Categorical
DoB	Date of birth (DoB) of the individual	Date
age	Age of the individual	Numerical
has_ca	Receipt of means-tested children's allowance (CA)	Binary (0/1)
has_cafr	Receipt of fixed rate children's allowance (CAFR)	Binary (0/1)
has_ua	Receipt of unemployment assistance (UA)	Binary (0/1)
has_sa	Receipt of social assistance (SA)	Binary (0/1)
has_sup	Receipt of social assistance for single unmarried parents (SUP)	Binary (0/1)
has_sub	Receipt of special unemployment benefit (SUB)	Binary (0/1)
has_dca	Receipt of disability child allowance (DCA)	Binary (0/1)
total_num_benefits	Total number of difference benefits received (sum of has_* variables)	Numerical (integer)
is_employed	Whether the individual paid a contributory wage (national insurance) greater than zero	Boolean
Child_Post2008 X DoB	Date of birth for each child born after 2008	Date
Child_Pre2009 X DoB	Date of birth for each child born in or before 2008 but restricted to up to 16 years of age.	Date

Source: Author

Table A.2
DESCRIPTIVE STATISTICS OF TREATMENT AND CONTROL

Observations	Treatment N= 57,854	Control N= 31,873
Employed	30,735 (53%)	16,435 (52%)
Has DCA	870 (1.5%)	1,162 (3.6%)
Age	32.0 (5.1)	36.9 (5.4)
Married	42,560 (74%)	23,911 (75%)
Single	12,253 (21%)	4,787 (15%)
Locality	53,919 (93%)	29,847 (94%)
Number of Child 0-3		
0	13,657 (24%) ^[28]	31, 873 (100%)
1	41,349 (71%)	
2	2,762 (4.8%)	
3	85 (0.1%)	
4	1 (<0.1%)	
Number of Child 3-6		
0	33,386 (58%)	31, 873 (100%)
1	21,917 (38%)	
2	2,468 (4.3%)	
3	80 (0.1%)	
4	3 (<0.1%)	
Number of Child 6-9		
0	47, 203 (82%)	
1	10,109 (17%)	29,323 (92%)
2	531 (0.9%)	2,460 (7.7%)
3	11 (0.1%)	85 (0.3%)
4		5 (<0.1%)
Number of Child 9-16		
0	49,553 (86%)	15,783 (50%)
1	6286 (11%)	13,361 (42%)
2	1,730 (3.0%)	2, 375 (7.5%)
3	232 (0.4%)	319 (1.0%)
4	48 (<0.1%)	32 (0.1%)
5	4 (<0.1%)	3 (<0.1%)
6	1 (<0.1%)	

Source: Author's calculations

Note: Means and standard deviations are reported for continuous variables, while counts and percentages are shown for categorical variables.

^[28] Given that the Free Childcare Scheme was rolled out on 1st April 2014 and applied to children aged 3 months to 3 years, there is a slight mismatch between the composition of the treatment group and the number of children aged 0-3. The treatment group includes any mother whose youngest child was eligible for the scheme at any point during the calendar year, regardless of treatment intensity. In contrast, the number of children aged 0-3 is based on end-of-year birthdates. For instance, the 2014 treatment group consists of mothers whose youngest child was born between 01/04/2011 and 30/09/2014 inclusive, while the corresponding number of children includes those born between 01/01/2012 and 30/12/2014 inclusive. Consequently, children born between 01/04/2011 and 31/12/2011 are included in the treatment group but excluded from the 0-3 child count for 2014. This discrepancy applies similarly across all relevant years (2012-2016).

Table A.3
PRE-TREND TEST: DD REGRESSION USING PRE-REFORM
DATA (2012–2013) WITH 2013 AS PLACEBO POST-
REFORM YEAR

Model	Placebo (Treat x 2013) Coefficient
(1) No controls	0.00171 (0.01102)
(2) With controls	-0.00109 (0.01074)

Source: Author's calculations

Note: This table reports the coefficient on the placebo interaction term, which captures the DD estimate for the year before the reform (2013) relative to the base year 2012, among the treatment and control groups. The purpose is to test for differential pre-trends. Both regressions include year-fixed effects (in this case, only 2012 and 2013), a treatment group indicator, and in specification (2), the individual-level control variables. However, the number of children in the age band 0-3 years is excluded as a covariate in (2) to avoid correlation with treatment status. Robust standard errors are shown in parenthesis. * Denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.4
SHARE OF PRE-REFORM EMPLOYMENT GAP CLOSED BY
THE REFORM FOR DISADVANTAGED SUBGROUPS - BY
YEAR

Subgroup of Mothers	Pre-Reform Gap (pp)	% Gap Closed
Single vs. Non-Single	-11.5	2014: 21.7 2015: 26.1 2016: 22.6
Disabled vs Non-Disabled Child	-14.5	2014: 10.3 2015: 5.5 2016: 27.6
Two vs One Child	-11.2	2014: 0.9 2015: 9.8 2016: 39.3
Three + vs One Child	-26.2	2014: 6.5 2015: 7.3 2016: 14.1

Source: Author's calculations

Note: The table reports the share of the pre-reform employment gap that was closed by the reform, calculated separately for each subgroup and year. The pre-reform employment gap is defined as the difference in average employment rates in 2013 between each disadvantaged group and its respective reference group: single vs non-single mothers, mothers with disabled children vs those with non-disabled children and mothers with two or three+ children vs those with one child. The share of the gap closed is computed by dividing the estimated differential treatment effect (θ_s) for each subgroup-year combination by the corresponding 2013 employment gap. The pre-reform gap is calculated as the mean employment rate for the subgroup with indicator value 1 minus the mean employment rate for the subgroup with value 0. The pre-reform employment gap is measured using 2013, as it is the last pre-treatment year and ensures consistency with the main DD setup where 2013 served as the reference year.