From Pensions to Pupils? Schooling, Resource Constraints and Old Age Pensions in Ireland 1901–11*

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Abstract

Can cash transfers given to the elderly improve school enrolments in multigenerational households? I answer this question by studying the 1908 Old Age Pension and school enrolments in Irish multigenerational households, using individual-level data from the 1901 and 1911 censuses. My findings show that co-residing with a pensioner increased the likelihood of enrolment, but these effects differed by the gender of the child and pension recipient. Boys benefited most overall. For girls, enrolments rose in the poorest households when the recipient was female but declined in the wealthiest households when the recipient was male. My results are consistent with cash transfers lowering the opportunity costs of schooling, while intra-household bargaining processes favoured boys over girls.

Keywords: schooling, poverty, gender, old age pension, cash transfer, economic history of Ireland.

JEL Codes: H55, I25, J16, J18, J24, N33.

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1 Introduction

A large literature argues that resource constraints inhibit human capital accumulation when poor households cannot afford the direct or indirect costs of school. Even when the returns to education are high, households stuck in poverty traps will often rely on child labour income to meet subsistence costs, with children either attending school irregularly or withdrawing from school entirely (Edmonds and Schady, 2012). Cash transfer programmes are thought to address these problems and have become the cornerstone of public strategies to improve human capital accumulation by reducing the opportunity costs and improving the demand for school (Aygün et al., 2024).

Although the link between increasing incomes and the accumulation of human capital is well established (Becker, 1992; Becker and Chiswick, 1966), the impact of Unconditional Cash Transfers (UCTs) on schooling remains ambiguous, since there is no guarantee that households will allocate the transfer towards schooling. Indeed, when households are treated with new income, this income can be subject to intra-household bargaining processes that not only moderate the effect of the transfer, but also reproduce structural disadvantages against certain children, such as females and those with learning disabilities (Akresh et al., 2013).

To bypass these moderating factors, policymakers condition eligibility and payments on school enrolment and attendance, making conditional programmes the most prevalent type of cash transfer in low- and middle-income countries (García and Saavedra, 2023). Unsurprisingly, conditional cash transfer programme evaluations dominate the literature, while the effects of large-scale UCT programmes are relatively understudied (Chong and Lau, 2025; Baird et al., 2014). Moreover, UCT literature tends to evaluate temporary experimental or pilot programmes across a wide range of socio-economic outcomes, or comparing conditional and UCT treatment arms. Accordingly, there are few evaluations of large-scale programmes that do not explicitly target young people or households containing children (Baird et al., 2019; Haushofer and Shapiro, 2016; Benhassine et al., 2015).

I exploit the opportunity to evaluate a large-scale UCT provided by history—the 1908 Old Age Pension (OAP) in Ireland, as an examplar of a UCT that did not target children, implemented in a low-income, highly gendered society with significant constraints on schooling demand. The OAP revolutionised social welfare provision in Britain and Ireland at a time when there was no comparable policy to improve conditions for the elderly (Ó Gráda, 2002; Budd and Guinnane, 1991). Although policymakers in London did not intend to disproportionately alleviate Irish poverty, Ireland undoubtedly became the policy's greatest beneficiary. Lower wages and output per capita increased the marginal benefit of an OAP

claim in Ireland compared to Britain, while Irish administrative constraints allowed the number of claimants to rapidly increase (Ó Gráda, 2002). Moreover, claims were financed centrally by the UK Treasury, while increases in income tax to fund the policy were borne almost exclusively by the UK's top one per cent of earners (Giesecke and Jäger, 2021). By 1911, the number of Irish OAP claims per capita was nearly three times higher than in Britain and represented more than a fifth of Irish public expenditure (Dunraven, 1912) compared to around 6 per cent of UK-wide level (Giesecke and Jäger, 2021). As such, the OAP is comparable to modern "Big-Push" initiatives, facilitated a substantial redistribution of wealth across the Irish Sea, and serves as an ideal natural experiment for study.

I explore the spillover effect of the OAP on schooling in multigenerational households by comparing the enrolment rates of children living with an OAP claimant to those who do not. Using full-count census returns from 1901 and 1911, I take advantage of the declared ages and intra-household relationships to detect multigenerational households and focus on the schooling outcomes of older children for whom the opportunity costs of school are highest. The richness of census data allows the determination of a child's schooling or employment status, while I make a unique data contribution by synthesising census data with new, high-resolution income estimates. Together, census and income data allows the calculation of the policy's average income boost and exploration of its impact on schooling by gender and age across the income distribution.

My identification strategy exploits the OAP's age-based and means-test criteria to assign treatment to eligible individuals residing in multigenerational households. I isolate the causal effect of the policy using a linear probability model and Difference-in-Difference (DiD) framework, controlling for a wide range of confounding variables. The primary threat to identification is that the OAP may have driven changes in household composition, particularly through age-misreporting or by enabling the formation of new, incomparable multigenerational households. I address this source of endogeneity by estimating propensity scores for each child and screening children who reside within incomparable treated and control households. Moreover, using this screened sample, I show that pre-intervention eligibility had no effect on enrolment trends, consistent with my modelling assumptions.

My headline finding is that while the OAP increased the likelihood that older children were enrolled in school, this effect was strongly moderated by gender. On average, the policy raised enrolment rates by 2 percentage points, with larger effects when the pension claimant was female. This average effect was driven primarily by older boys, for whom the opportunity costs of school were highest, increasing enrolments by more than 5 percentage points. In contrast, the effect for girls was more nuanced. Although generally smaller than for boys, the magnitude of the effect varied by both the gender of the OAP claimant and the household's

position in the income distribution. For girls in the poorest households, enrolments increased only when the claimant was female. Conversely, when girls in the richest households live with a male claimant, enrolment rates dropped sharply by 12 percentage points. I interpret these findings as evidence that, in a highly gendered society, the unconditional nature of the OAP cash transfer enabled it to reinforce prevailing gender norms. Indeed, I show that in the richest households, girls living with male claimants were proportionally more likely to report no occupation in the census, suggesting a shift away from schooling towards domestic work.

This paper contributes to the literature on the effects of UCTs on schooling, which has presented evidence largely from short-term experimental or pilot programmes in African and South American contexts, often evaluated across a range of outcomes including education, health, consumption, and investment. Many of these UCT programmes are limited by policy design, sample size, programme attrition, and selection issues (Baird et al., 2014), while findings tend to be mixed and context specific. For example, Handa et al. (2018) finds that of two Zambian UCTs, only one improved schooling while Haushofer and Shapiro (2016) finds that a UCT in rural Kenya had no effect on education. That said, a recent systematic review of the UCT literature finds that although they can improve schooling, the effect-size between studies is significant and cannot be explained by programme characteristics such as the transfer size, whether the UCT was a pilot, or sex of the claimant (Chong and Lau, 2025). Moreover, although UTCs have been found to be less effective than programmes with schooling conditions (García and Saavedra, 2023; Baird et al., 2011), some evidence suggests that conditionality only matters for children who are less likely to go to school, such as girls and those with disabilities (Akresh et al., 2013).

I build on this literature by evaluating a long-term and highly generous national programme that provided weekly payments to eligible individuals. This differs from much of the experimental literature since neither eligibility nor payment was determined by the presence of a school-age child in the household, nor were households advised, either actively or passively, how to spend the transfer. Only two other quasi-experimental studies estimate the effect of public pensions on schooling—Filho (2012) and Edmonds (2006), both of which use survey data. Although both studies find a positive effect on schooling, the gendered nature of their findings differs. Filho (2012) finds that the effects are only positive for girls, and suggests that male claimants benefit girls less than female claimants. Edmonds (2006) on the other hand, finds that the South African public pension eroded cost constraints in black-headed households and led to improvements in schooling for rural boys, particularly when the claimant is male. This paper improves the approach of these authors by leveraging full-count census data, enabling a richer analysis of heterogeneity throughout the socio-economic

spectrum. The effect of OAPs on other child outcomes such as health and nutrition has also been studied by Duflo (2003) and Case (2004).

The second contribution of this paper is to offer the first policy evaluation of the 1908 OAP in Ireland using full-count census data and newly developed income estimates. Giesecke and Jäger (2021) conduct the only other evaluation of the 1908 OAP using full-count census data, but unlike this paper, their focus is on it's effect on labour supply in England and in Wales. Moreover, I improve the identification of the OAP in an Irish context by directly accounting for the policy's means-test and focus on its spillovers on schooling in multigenerational households rather its direct effect on the elderly. Other historical OAPs have been studied across a number of countries, but none evaluate their effect on schooling, such as 1935 Old Age Assistance programme in the USA (Galofré-Vilà et al., 2022), the 1913 Swedish compulsory public pension (Andersson and Eriksson, 2015) and the 1891 German old age assurance (Guinnane and Streb, 2021).

Finally, this paper contributes to literature that explores the effect of a cash transfer in a context where there are clear demand constraints on schooling, contrasting with literature that emphasises supply side constraints (Cinnirella and Hornung, 2016; Easterly, 2007). In Ireland, children frequently left school as soon as employment became available, indicating high opportunity costs of education and household's unwillingness to forgo earning opportunities. Studies such as Aygün et al. (2024) and Kilburn et al. (2017) suggest that cash transfers can erode these demand constraints and lead to substantial improvements in schooling. In the former case, the authors find large and uniformly positive effects of large-scale UCTs on schooling for refugee children within extremely resource-constrained households in Turkey. Although this paper also focuses on a large-scale intervention, it differs in that households containing children do not effect eligibility or the size of OAP awards, nor do Irish households live in such extreme, anomalous conditions.

This paper proceeds as follows. Section 2 provides the historical context of the OAP and schooling in Ireland; Section 3 describes my data; Section 4 introduces my DiD framework, schooling model and discusses my identification assumption. Section 5 continues with baseline results, before analysing socio-economic heterogeneity by income quintile and sex, and testing robustness. Finally Section 6 discusses results and concludes.

2 Historical Context

2.1 The Old Age Pension (1908)

The OAP Act (1908) introduced a means-tested, non-contributory pension for all over seventy-year-olds in the UK. The policy was introduced as a central pillar of a series of socio-economic reforms and was the first to target the elderly during a period of limited social protection (Purdue, 2011; Casson, 1908). Individuals applied for an OAP by completing a standardised application form in their local post office which was then assessed by a Treasury Pension Officer (Casson, 1908). Eligibility was determined by four criteria; that the applicant was at least seventy years old, earned less than £31.50 per annum, was a permanent resident, and was of sound character.[1] Using this criteria, a Pension Officer would recommend whether the claim should be approved or rejected to one of the 54 local Pension Committees.[2] Any decision made by a Pension Committee could be appealed by the applicant or the Pension Officer, with the Local Government Board (LGB) of Ireland overseeing and enforcing the appeals process. Should a claim finally be rejected, an applicant must wait four months before submitting a new claim (Casson, 1908).

The first OAPs were paid on the 1st of January 1909, with claimants collecting their pensions from their local post office.[3] Uptake was high given the generous and anomalous nature of the policy. At the design phase, a Parliamentary Select Committee estimated that by 1907, total OAP eligibility would be around 387,000 persons across the UK (Casson, 1908). By the end of 1909, however, the total number of claimants across the UK was nearly double this, with Ireland standing out for its exceptionally high uptake rates. Three months after payment began, the total number of active Irish claims and appeals exceeded the total number of over 70-year-olds indicated by the 1901 census, and on the eve of the 1911 census, 201,783 OAPs were payable in Ireland (British Parliamentary Papers, 1913b).[4] Consequently, the number of OAP claims per capita in Ireland was 2.7 times larger than in England and Wales, and 2.4 times larger than in Scotland. Indeed, Irish claimants represented 26 per cent of all OAP claims, despite accounting for only 14 per cent of the total number of over 70s in the UK (Ó Gráda, 2002).

The Irish accounted for a disproportional number of OAP claims for several reasons.

[1] Individuals of "Bad" character were those who frequently received Poor Relief, had no recent work history, were sectioned under the 1890 Lunacy Act (Ireland), or who had a criminal record.

[2] 394 Pension sub-committees were also established to process the high volume of applications.

[3] At the time this was a public spectacle and dubbed by the national press as "Pension Day" see Ó Gráda (2002).

[4] In 1901, there were 189,300 persons who declared themselves to be over 70. By March 1909, 183,500 OAPs were payable and an additional 13,700 were subject to appeal.

Firstly, excess mortality during "an Gorta Mór"–the Great Famine– and high levels of out-migration in the latter half of the nineteenth century led to the premature ageing of the Irish population (Colvin et al., 2024), meaning Ireland had a high old-age dependency ratio for its level of economic development. Secondly, the policy was designed without regard for Irish administrative limitations, particularly the lack of reliable demographic information available to officials. Birth registration became compulsory in England and Wales in 1837, seventy-one years before the OAP act was passed. In Ireland, birth registration became compulsory nearly thirty years later, meaning birth certificates could not be used for age verification. Instead, Irish officials relied on manual searches of the household returns from the 1841 and 1851 census to verify applicant ages (Ó Gráda, 2002; British Parliamentary Papers, 1909). Not only were these searches costly and unreliable,[5] demographic data was poor since nineteenth century Irish census returns were prone to age heaping (Budd and Guinnane, 1991). Therefore, officials also accepted baptismal and marriage certificates or even baptismal certificates of children dated before 1860 as proof of age. Of the 43,475 appeals addressed by March 1911, 54 per cent regarded the age of the applicant.

By its own admission, the Irish LGB took a more lenient approach to OAP claims than was provided in the legislation (British Parliamentary Papers, 1909). In addition to difficulties with age verification, the board only partially applied the means-test, considering the inclusion of all recent asset transfers– including land– as incompatible with land inheritance customs (British Parliamentary Papers, 1911c). Instead, land transfer issues were assessed on a case-by-case basis (Ó Gráda, 2002; British Parliamentary Papers, 1909). As early as 1909, the Irish LGB admitted that OAP applications were only rejected in the most obvious cases of fraud, and "borderline" applications were given the benefit of the doubt (British Parliamentary Papers, 1909). Unsurprisingly, administrative limitations and the reliance on nineteenth century census returns led to widespread age misreporting in the 1911 census as individuals sought to align their census-reported age to their OAP-reported age, or to fabricate an older age so as to qualify for the OAP prematurely (Ó Gráda, 2002; Budd and Guinnane, 1991). Although this OAP quirk in Ireland requires careful consideration in my identification strategy, the generous approval of claims in Ireland improves the likelihood that I can accurately predict which individuals received the OAP.

To conclude this policy overview, it is worth noting that income tax rates in Ireland were not substantially changed to fund the policy, as most of the population was too poor to meet the tax threshold (Giesecke and Jäger, 2021; Ó Gráda, 2002). Alternative pensions existed, but, like in Britain, were concentrated at the upper end of the income spectrum and limited

[5]By 1922, nearly 30,000 searches were carried out by administrators, with most of these occurring between 1917 and 1921. Only 75 per cent of searches resulted in a match (Ó Gráda, 2002).

to public sector workers or some private sector white-collar workers. The Poor Law was the only alternative social policy that aimed to ameliorate poverty within a workhouse or through out-relief.[6] Although poor relief did not target the elderly, the economic insecurity of old age meant that most workhouses and out-relief lists were heavily populated with the elderly (Gilleard, 2017). Prior to 1911, any individual who had received assistance from the Poor Law since 1908 was disqualified from an OAP. However, in January 1911, three months before the 1911 census, the disqualification criteria was abolished. While this window is brief, the time required to process an application is unknown. If unaccounted for in the identification strategy, Poor Law substitution effects may bias my results, particularly if, within this short window, the policy encouraged the formation of new, incomparable households.

2.2 Irish National Schools

The National Schools of Ireland (NSI) were established in 1831 and aimed to provide a centralised and secular system of primary education to address high rates of illiteracy and innumeracy (Walsh, 2016; Ó Gráda, 2010; British Parliamentary Papers, 1902). It was overseen by the Commissioners of National Education in Ireland (CONEI) and while its initial expansion was hampered, by 1867 the Government was satisfied that there was an adequate supply of primary schooling throughout Ireland (Dale, 1904). Even so, the NSI accelerated its expansion throughout the 1870s and 1880s by improving teacher remuneration, providing teaching pensions, and reducing the financial burden of establishing new teaching training colleges and schools . Between 1873 and 1901, the number of NSI teachers increased by over 70 per cent to 11,800 while the number of operational schools increased by 21 per cent to 8,700 (British Parliamentary Papers, 1901c).

This expansion of school provision occurred during a period of substantial out-migration. As the number of children under fifteen fell by 26 per cent between 1881 and 1901, the number of schools grew by 50 each year on average (British Parliamentary Papers, 1901c). By 1895, there were 17,950 vacant student places in schools (British Parliamentary Papers, 1897b)[7] and by 1903, administrators began to amalgamate or close small, infrequently attended schools (British Parliamentary Papers, 1905). Closures were carefully planned to ensure that there would be no adverse effect on schooling provision and, in cases where schools had

[6] A workhouse provided accommodation, clothed and fed those living in absolute poverty in exchange for labour. Residents were called inmates, and living conditions were poor (Crossman, 2006). Out-relief was primarily granted to household heads who, in exchange for labour on public works, were permitted to remain at home and receive a small weekly cash or payment in–kind.

[7] This figure emulates the CONEI estimates of school capacity and allows 8 square feet of classroom for each student. Taking into account the total size of the school estate in 1895, the NSI had the capacity to accommodate 843,996 children. The NSI reported an actual average enrolment of 826,046 pupils that year.

no children on the rolls, closure was only a formality. Despite the net closure of 430 schools by 1911, NSI administrators continued to develop the existing school estate with the number of vacant student places growing by a factor of thirteen to over quarter of a million (British Parliamentary Papers, 1912c).

Low demand for school was a perennial problem for educators. Irish school attendance figures began to diverge from that of Great Britain in 1880 (Fahey, 1992) and despite the introduction of compulsory attendance for all children aged six to fourteen in 1892, it was infrequently enforced and over a quarter of children still failed to attend regularly. This problem was amplified by the fact that older children left school prematurely—usually as soon as work became available (Dale, 1904). Of the six academic grades, 42 per cent of children were enrolled in the first grade on average in 1901–11.[8] 70 per cent of children were enrolled in the three junior grades combined, while in some of the poorest areas older children were completely absent from the classroom (British Parliamentary Papers, 1913d; Dale, 1904). Such was the extent of disruption that many children could not progress to higher grades since they did not complete sufficient schoolwork to qualify for examination (Dale, 1904).

Children were generally inducted to school at the age of six; however, it was not unusual for younger children to be enrolled in an infant school if one was available. Although learning in junior grades was mainly directed towards the "three Rs"—Reading, Writing, and Arithmetic, children were also taught basic science and geography, and if teachers were appropriately qualified, could also take languages and intermediate mathematics classes (British Parliamentary Papers, 1908a).[9] That said, many aspects of NSI curriculum were gendered—girls were not allowed to progress beyond a third-grade science class and instead were instructed in domestic sciences, needlework, cooking, and laundry work. Indeed, even female trainee teachers sat different examinations from their male counterparts and, when qualified, were systematically paid less than males of identical age and experience (British Parliamentary Papers, 1908a; Dale, 1904).

[8]Enrolment figures for the first grade have been adjusted to exclude infant classes. In total, 45 percent of all children enrolled in school were in infant class or first grade

[9]Languages included Irish, French and Latin, whereas intermediate mathematics covered the study of algebra and geometry.

3 Data

3.1 Data Sources

3.1.1 Census Data

This paper's primary data source is the 1901 and 1911 Censuses of Ireland. These censuses were conducted exactly ten years apart and collected information on the occupants of each household including their address, names, ages, sex, intra-household relationships, religion, birthplace, occupation, literacy, marital status, ability to speak Irish, and disability status.[10] A week before the census date, an Enumerator visited the household to deliver the census form and record the name of the household head. Households were instructed to complete the form only for individuals residing there on the night of the census, including visitors, boarders, and servants. The forms were collected the following week and forwarded to the office of the Registrar General for Ireland for tabulation. If households were unable to complete the form, Enumerators inquired the information from the household head and completed the census on their behalf. Any households that refused to participate or Enumerators who wilfully shirked their duties could be convicted and fined £5 (Census (Ireland) Act, 1910). This was a substantial fine worth around 12 per cent of average earnings.

To measure the effect of OAP on schooling outcomes, I focus on household returns only and isolate multigenerational households. Firstly, households containing transient relationships such as boarders, visitors, and servants are dropped since it is unclear how these individuals may affect schooling outcomes. Secondly, due to data limitations, it is not possible to distinguish individual households within tenements, so all such dwellings are also excluded.[11] From the remaining households, multigenerational households are detected by exploiting the recorded intra-household relationships, which have been recorded with respect to the household head e.g. daughter, father-in-law, grandchild.

Multigenerational households are those which contain at least one child under the age of 16 and at least one "other" relative such as the child's grandparents, great aunt/uncle or first cousins (once removed). To address comparability issues arising from age heaping in 1901 and age misreporting in 1911, I also require these other relatives to be at least 50 years

[10] For married couples, the 1911 Census also enquired the number of years married, number of children born alive, and the number of children still living. Given no comparable question was included in the 1901 Census, responses for these questions are excluded from this paper.

[11] While a "tenement" census return did not exist, these dwellings are detected through returns that contain multiple household heads and over 20 members. This is fine-tuned using a measure of surname dispersion where households that contain more than 10 members and where more than 70 per cent of the surnames are different are dropped (2,988 households)

old. Finally, I note that the high labour mobility, traditional naming conventions, and the Gaelic Revival movement prevent us from linking households between censuses.[12]

3.1.2 Income Data

As income is not recorded in the census, I reconstruct household incomes using a combination of occupational information and demography, where the weekly wages of an individual assumes the standard functional form of $wage_i = f(occupation_i, age_i, literacy_i, sex_i)$. Census recorded occupations are standardised using a modified Historical International Standard of Classification of Occupations (HISCO), where occupations are grouped into five categories: farm labourers, urban labourers, farmers, skilled workers, and white-collar workers. Incomes are assigned within group using a wide range of historical data sources across seventy-three unique occupations.

The largest occupational categories are labourers (19 per cent), farmers (46 per cent) and skilled workers (22 per cent). Data on labourers wages and skilled craftsmen were collected frequently throughout this period by the Board of Trade and Department for Agriculture and Technical Instruction (DATI) the period and allow us to assign wages by age, sex, and locality. Although farmers did not earn a weekly wage, weekly income is approximated using the annual value of agricultural output from annual DATI reports across 158 geographical areas, accounting for the marketable share of product, prevailing prices, and average farm sizes.

Occupations with no direct match are assigned the average income of their respective occupational category. If not already adjusted, wages are then calibrated by age, sex, and literacy, to account for work experience, the gender wage gap, and human capital differences. Individual incomes are then aggregated into a measure of household income. To account for households who's members failed to report any occupation, a small subsistence income is assigned to all households. Subsistence income is approximated using county-level variation in farm labourers' wages from a DATI report on Agricultural Labourers (1910-1911), exploiting the differences between full-waged, half-board, and full-board rates to isolate the weekly component intended to cover the costs of rent and food. A full description of my data generation procedures, income estimates, and methodology is available in Appendix B.

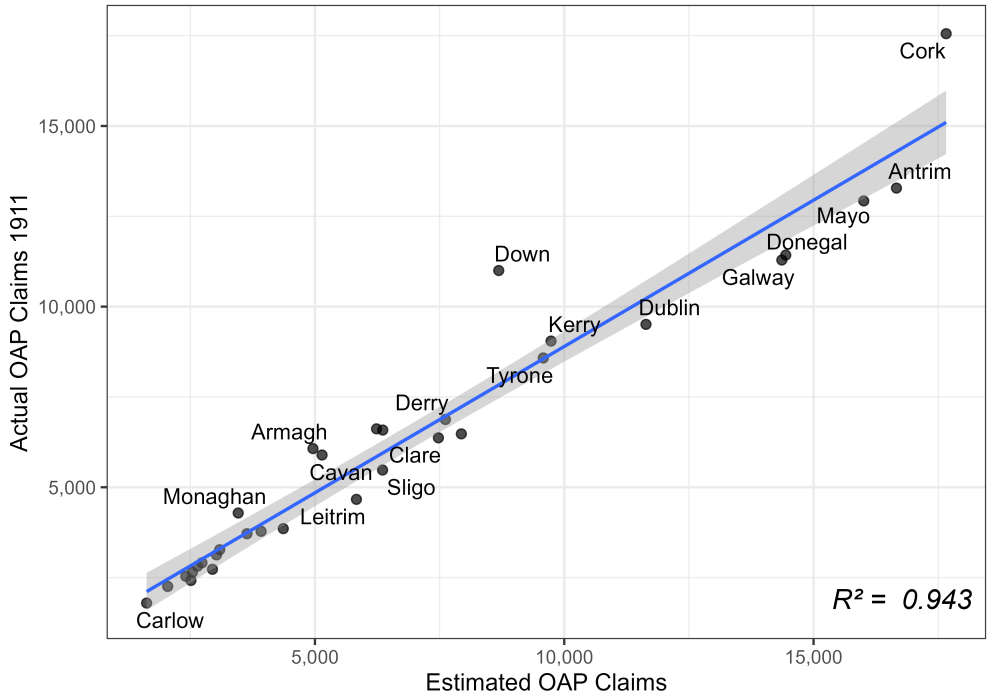
[12] As a form of protest against the UK, some households returned the 1911 Census form written in Irish, including the Irish translation of their name. Furthermore, the Gaelic revival had encouraged the restoration of "O" and "Mac" surnames such as O'Casey and MacDonagh.

3.2 Descriptive Statistics

3.2.1 Old Age Pensions

Comprehensive data on OAP claims at an individual level no longer exists, so I assign pensions to eligible individuals that meet the policy's age and financial means criteria i.e. are at least 70 years old and earn less than £31.50 per annum. Given that over 96 per cent of all Irish claims were at the full rate, eligible individuals are treated with a full pension income of £0.25 (5-shillings) per week. Across the full 1911 census, 218,265 OAPs are assigned to eligible individuals, modestly overestimating actual claims by 8 per cent.[13] Indeed, some over-assignment should be expected since I cannot account for delays in the application or appeals process for newly eligible individuals.[14] As a sanity check, Figure 1 regresses the number of actual OAP claims at county level ($n = 32$) against assigned OAPs and show that it is a strong predictor of actual OAP claims ($R^2 = 0.94$).

Figure 1: Sanity Check of OAP Claims ∼ Estimated Claims at County level



Note: Bivariate regression where the total number of counties is 32. Names omitted for several counties to improve visualisation.

Dividing all households into income quantiles allows an exploration of the relative OAP

[13] Without the means-test, treatment would be assigned to 282,760 individuals, overestimating actual claims by over 40 per cent.

[14] In March 1911 there were 201,783 active claims in Ireland.

income boost from the poorest to the richest households. Table 1 describes the OAP boost and shows that it increased household incomes by nearly 5 percentage points on average, comparable to most contemporary cash transfer experiments.[15] The OAP had the most transformative impact on the poorest, boosting household incomes by around 30-percentage points on average. Typically, these poor households live at subsistence level, having only have only one member who reported an occupation from an agricultural labourer or poor farming background. The OAP income boost diminishes with household income, increasing incomes by approximately 8 and 4 percentage points in the second and third quintiles, while having only a modest effect at the top of the income distribution.

Table 1: OAP income boost by household income quintile

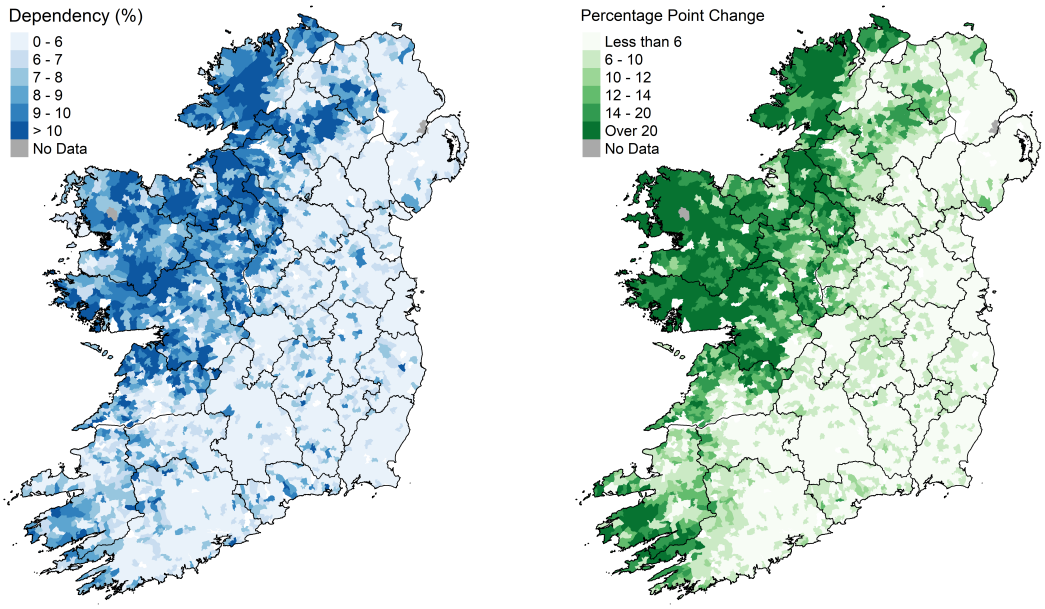
Quintile	Household Income (£)	OAP Boost
1 (poorest)	0.17 – 0.64	0.270
2	0.64 – 0.90	0.075
3	0.90 – 1.31	0.038
4	1.31 – 1.98	0.030
5 (richest)	1.98 – 3.50	0.013
Average	1.55	0.047

OAP boost is the average log point change in each quintile between 1901 and 1911. Incomes are top-coded at £3.50 per week to mitigate the effect of a small number of extremely wealthy households.

The relative size of the OAP boost in Irish households highlights Ireland's status as an income-poor country. Further evidence of the high marginal benefit of the OAP is apparent in the spatial distribution of OAP claims and its associated income boost. Figure 2 maps this distribution at District Electoral Division (DED) level and shows that the western periphery was the main beneficiary of the OAP. There is an obvious positive correlation between the number of claims per capita and the average income boost, confirming that the marginal benefit of receiving an pension was likely higher in the west. These western districts were the poorest areas of Ireland that were devastated by the Great Famine (1845-1852) and endured repeated outbursts of economic crises in the latter half of the 19th century (Ó Gráda, 1994). In many of these western districts, more than 10 per cent of the population were assigned an OAP boosting total DED income by over 20 per cent, compared to the 4 per cent boost on average for the whole of Ireland.

[15] Baird et al. (2014) compares thirty-five contemporary cash transfer programmes and finds that they boosted incomes on average by 5.66 percentage points.

Figure 2: Map showing spatial distribution of claimants and boost to household income



Note: Left showing the number of OAPs per capita at District Electoral Division level (DED) and right showing the average boost to DED level income in percentage points. County borders shown in black.

3.2.2 Multigenerational Households

I aim to measure the effect of the OAP on the schooling outcomes within multigenerational households. As discussed in my Context section, schooling was compulsory for children aged six to fourteen, but it was infrequently enforced. Indeed, 1901 enrolment figures (baseline) begin to systematically decline after the age of eleven in line with the reports from the national school administrators of children leaving school prematurely and joining the labour force. Consequently, I focus on the schooling outcomes of children aged thirteen to sixteen, for whom schooling was largely optional and opportunity costs were highest. Table 2 sets out summary statistics for all variables in this study.

The first two columns of the table describe the differences between multigenerational and non-multigenerational households at baseline in 1901. Around 13 per cent of older children lived in multigenerational households and in socio-economic conditions similar to the rest of the population. Nearly all older children had some ability to read and/or write, while only 3 per cent were illiterate. Over three quarters of children were Roman Catholic, nearly half were female, and were 14 and a half years of age on average. Although multigenerational households earned around 6 per cent more than non-multigenerational households on average, once the larger size of multigenerational households are considered, their equvilised incomes

are practically identical at around £0.44 per week per person (8 shillings and 10 pence).

That said, multigenerational households do differ from the rest of households. At baseline, 54% of children in multigenerational households were enrolled in school, modestly higher than the enrolment rate in non-multigenerational households, 18% were employed while the remaining 28% did not specify any occupation. Given that individuals were instructed not to report an occupation if they primarily performed household duties, it is reasonable to assume that most of these children were performing domestic duties at home. Although incomes were broadly similar to all other households, there are some indications that socio-economic conditions were worse for multigenerational households. Households were 9 per cent larger, 7 percentage points more likely to be a farming household, 5 percentage points more likely to have an illiterate household head and more likely to be located within the western periphery, as indicated by the higher rate of bilingualism. Unsurprisingly, multigenerational households are older with an average household head age of 56 compared to 50 for non-multigenerational households, and are much more likely to contain an elderly relative over the age of 70.

Columns 3 and 4 describe summary statistics between treated and untreated multigenerational households, where both 1901 and 1911 census' have been pooled. In this case all treated households contain an eligible pensioner in 1911 while control households are all households in 1901 and untreated households in 1911. There are obvious differences between treated and control multigenerational households. Treated children are 7 percentage points more likely to be enrolled in school, 6 percentage points less likely to be employed, are younger, much more likely to be bilingual and Catholic. Treated multigenerational households are much poorer on average with household incomes nearly 30 per cent less than untreated households. Even when OAP income is included in the measure of equivilised income, treated multigenerational households are still 13 per cent poorer than untreated households and are worse off by around 1 shilling and 2 pence worse per person per week. Treated households are also more likely to be larger, rural, farming households, with over a quarter containing an illiterate household head.

In short, these differences indicate that treated households are more likely to be poorer than untreated households, and again highlight the higher marginal benefit of obtaining a pension for these poorer households. If poorer multigenerational households are more likely to select into the policy, then any strategy that uses all untreated multigenerational households is flawed since this is an incomparable control group that differs systematically from the treated group. The presence of these richer households will bias my results upwards because, as I show in Appendix A, children in richer households are less likely to be enrolled in school at ages 15 and 16 since they tend to finish school on schedule at the age of 14.

Table 2: Summary Statistics

Characteristic	Baseline (1901)		Treated Multi-Gen	
	Not Multi-Gen	**Multi-Gen**	**0**	**1**
	N = 237,078	N = 34,175	N = 50,546	N = 19,536
Treatment				
Relatives Aged 70+	0.03 (0.18)	0.53 (0.59)	0.40 (0.55)	1.14 (0.36)
OAP Claimants	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	1.09 (0.30)
OAP Income	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.27 (0.08)
Children aged 13-16				
Enrolled in School	0.52 (0.50)	0.54 (0.50)	0.55 (0.50)	0.62 (0.48)
Employed	0.19 (0.40)	0.18 (0.39)	0.18 (0.38)	0.12 (0.32)
Age	14.51 (1.11)	14.48 (1.11)	14.49 (1.11)	14.40 (1.11)
Eldest Child	0.71 (0.45)	0.75 (0.43)	0.76 (0.43)	0.73 (0.44)
Female	0.49 (0.50)	0.49 (0.50)	0.49 (0.50)	0.49 (0.50)
Illiterate	0.03 (0.16)	0.03 (0.17)	0.02 (0.15)	0.02 (0.13)
Bilingual	0.13 (0.33)	0.16 (0.36)	0.16 (0.37)	0.25 (0.43)
Roman Catholic	0.77 (0.42)	0.77 (0.42)	0.76 (0.43)	0.80 (0.40)
Church of Ireland	0.11 (0.31)	0.11 (0.31)	0.11 (0.31)	0.09 (0.28)
Presbyterian	0.10 (0.30)	0.10 (0.30)	0.11 (0.31)	0.08 (0.28)
Household				
Urban	0.23 (0.42)	0.17 (0.38)	0.21 (0.40)	0.17 (0.37)
Household Size	6.96 (2.36)	7.58 (2.68)	7.53 (2.64)	7.72 (2.73)
Sibling Over 16 Employed	0.32 (0.47)	0.34 (0.47)	0.35 (0.48)	0.27 (0.44)
Household Income	1.56 (1.07)	1.65 (1.11)	1.70 (1.14)	1.23 (0.93)
Equivalised Income	0.45 (0.29)	0.44 (0.29)	0.46 (0.29)	0.33 (0.23)
Equivilised Income + OAP	0.45 (0.29)	0.44 (0.29)	0.46 (0.29)	0.40 (0.24)
Labourer Household	0.18 (0.39)	0.16 (0.36)	0.17 (0.37)	0.14 (0.35)
Farmer Household	0.49 (0.50)	0.56 (0.50)	0.52 (0.50)	0.54 (0.50)
Skilled Worker Household	0.22 (0.42)	0.18 (0.38)	0.18 (0.39)	0.13 (0.34)
White-Collar Household	0.07 (0.25)	0.05 (0.23)	0.06 (0.24)	0.04 (0.19)
Illiterate Household Head	0.20 (0.40)	0.25 (0.43)	0.22 (0.42)	0.28 (0.45)
Age Household Head	49.96 (9.48)	56.37 (13.74)	56.46 (13.03)	59.97 (15.31)

Sample means with standard deviation in parenthesis. Children in non-multigenerational households that include someone over 70 are typically those with an older father. Household income includes a subsistence income that has been approximated from labourer's wages. Equivilised income is calculated using OECD Modified weights. All differences between treated and untreated multigenerational households are statistically significant at 1%, with the exception of the share of females.

4 Identification Strategy

4.1 Estimation Framework

The an ideal approach to measure the effect of the OAP on the schooling outcomes of older children is within a Difference-in-Difference (DiD) framework, holding individual and year fixed effects and measuring the differences in schooling outcomes between treated and untreated children before and after their household receives an OAP.[16] However, linking households across Irish census' is fraught with difficulty and prone to type 1 and type 2 errors given the mobility of the population, age heaping/misreporting and the homogeneity of Irish names (Ó Gráda et al., 2023). To address this limitation, I pool all multigenerational households across both censuses and introduce a simple dummy that indicates whether the household is in 1911 or not. Since no households are treated with an OAP in 1901 and only eligible households are treated in 1911, this 1911 dummy absorbs unobserved characteristics correlated with the post-treatment period and allows the identification the effect of the OAP on schooling in eligible households. This DiD approach can be surmised by the following regression equation:

$$\text{School Enrolment}_{it} = \alpha + \beta_1 \sum_{j \in H_{it}} \text{Pension}_{it} + \beta_2 \text{c1911}_i + X_{itk} + \delta_{DED} + \varepsilon_{it} \tag{1}$$

where school enrolment status of individual i at time t is a dichotomous dummy variable that takes the value of 1 if an older child is enrolled at school, 0 otherwise. $\sum_{j \in H_{it}} \text{Pension}_{it}$ is a continuous variable that measures the amount of OAP income allocated to eligible individuals in the household in GBP (£) per week. This variable is 0 for all households in 1901 since no one has yet been treated, and increases by £0.25 (five shillings) for every eligible pensioner in the child's household in 1911. In this respect, the control group is all older children in 1901 and those that did not live with an eligible pensioner in in 1911. $c1911_i$ is a 1911 census dummy and equals 1 in 1911 during the post-treatment period, 0 otherwise.

X_{itk} is a vector of covariates that controls for all observable characteristics that are likely to influence schooling outcomes in a household. This includes household income in GBP (£), where income contributions from children under 16 and adults over 70 have been subtracted

[16] Other studies of the OAP such as Giesecke and Jäger (2021) use a regression discontinuity design to identify the effect of the OAP in England by exploiting variation around the age criteria. This is ill-advised in the Irish context since many individuals manipulated the age threshold by lying to administrators about their age.

to account for the mechanical link between income and labour supply decisions.[17] Controls include the child's age to account for rising opportunity costs, as well as dummy variables to indicate whether the child is illiterate, the eldest child, female, Catholic, or lives in an urban district. Differences in household preferences for schooling are accounted for by controlling for whether the child lives in a farming household, the literacy status of the household head, and whether the child is bilingual (Irish and English).[18] Finally, controls for other ambient confounders are inserted such as household size, the gender of the eligible elderly person, the age of the oldest household member and its square to account for non-linearities associated with older households. δ_{DED} is a set of District Electoral Division fixed effects to account for unobserved shocks that affect individuals similarly, and ε_{it} is the remaining error term. Assuming that responses to treatment within the household are not independent, standard errors are clustered at the household level.

4.2 Balance Tests and Identification

In this paper's Data section, I showed that there are systematic differences between treated and control groups. Although baseline differences are not an identification assumption for a DiD approach, these differences become problematic if treatment is systematically correlated with changes in observable household characteristics. This would suggest that the policy affected household composition either by changing existing multigenerational households or by incentivising the formation of new, incomparable households. The latter is the most likely source of endogeneity—in the appendix I show that between 1901 and 1911 the share of multigenerational households increased throughout the income spectrum, poorer quntiles tend to report larger increases.

Since treatment is also non-random, poorer households are more likely to be positively selected into the treatment group. This increases the possibility that treatment is correlated with some other unobservable characteristic that affects the probability that an older child is enrolled in school, making my estimates endogenous. To confirm that poorer households are selected into the treatment group, I regress an OAP treatment dummy against all observable household characteristics. Even after accounting for the labour supply decisions of children and the elderly, treatment is correlated with lower household incomes, larger household sizes,

[17] If left unadjusted, household income acts as a collider—both influencing a child's likelihood of being enrolled in school and being linked mechanically to OAP payments. In treated households, the OAP may reduce the need for the elderly to work, while also allowing children to leave the workforce and return to school.

[18] Bilingual children may have a higher preference for school since in districts where Irish was the predominant language, households relied on schools to provide basic instruction in English in the hopes of improving their children's socio-economic opportunities (Fernihough et al., 2024)).

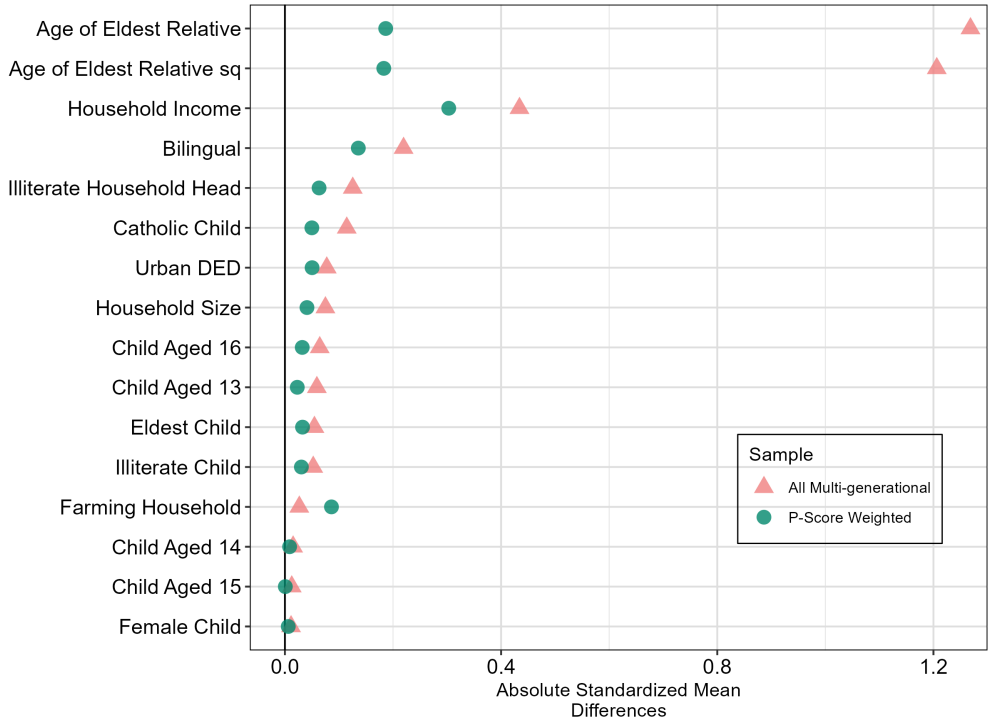
and whether the household head is illiterate or not. Treated households are more likely to be Roman Catholic, less likely to be bilingual, and less likely to contain 13 year old children. These results are reported in full in Appendix A.

4.2.1 Propensity Score Screening

To address the endogenous effect of new multigenerational households and changes in household composition on school enrolments of older children, I use a logit to predict the probability that an older child lives with an OAP recipient, conditional on a multivariate covariate vector of observable child and household characteristics which are not mechanically related to treatment. As a precursor to the DiD model specified in Equation 1, I screen all children who have a low or high probability of being treated i.e. $0.15 < p(X_i) < 0.85$, ensuring that no extrapolation is required for observations where there is no overlap in the distribution between treatment and control. This method has been shown to address the issue of non-random treatment by Angrist and Pischke (2009) and Crump et al. (2009) and will remove incomparable households from the sample, decreasing the likelihood that unobserved differences associated with the OAP confound my estimates.

After screening, the total number of observations are reduced from 67,472 to 38,400, primarily by removing a large number of children from untreated households with a low probability of containing an OAP recipient. As such, control group households become notably more comparable to treated households. To minimise the remaining absolute differences between treated and control, I use the probability that children live with an OAP recipient to weight the regressions. **??** is a covariate balance plot and shows that after weighting, most absolute differences between treated and control move closer to zero, improving the balance between treatment groups.

Figure 3: Covariate balance before and after weighting



4.2.2 Parallel Trends

In Equation 1, β_1 can be interpreted as the causal effect of living with a treated pensioner on schooling provided that in absence of treatment, school enrolments would not have evolved differently. As this counterfactual cannot be directly observed, I test the parallel trend assumption indirectly using a placebo test. I isolate all multigenerational households in the matched 1901 sample—prior to the intervention—and assign a placebo OAP to all eligible individuals. Given that none of these individuals were actually treated, this placebo should be uncorrelated with the error term and have no significant impact on schooling outcomes for children living with an eligible pensioner. For clarity, the placebo model is specified by the following regression equation:

$$\text{School Enrolment}_i = \alpha + \beta_1 \sum_{j \in H_i} \text{Placebo}_i + X_{ik} + \delta_{DED} + \varepsilon_i \tag{2}$$

where $\sum_{j \in H_i} \text{Placebo}_i$ is a continuous variable that measures the amount of placebo OAP income allocated to a household in GBP (£) per week. This variable is 0 for households with no eligible persons and increases by £0.25 for every eligible pensioner in the household. All other variables are defined as before. The results are reported below in Table 3 and

show that once household characteristics are accounted for, the placebo has no effect on the probability that older children are enrolled in school. This evidence is consistent with the assumption of parallel trends, affirming that my empirical approach is valid on the screened sample of children.

Table 3: Parallel Trends– Placebo test

	Child Enrolled in School			
	(1)	(2)	(3)	(4)
Placebo Pension	-0.076*	-0.012	-0.014	-0.010
	(0.039)	(0.047)	(0.048)	(0.037)
Child Controls	✓	✓	✓	✓
Household Controls		✓	✓	✓
Age Trends			✓	✓
R^2	0.425	0.426	0.426	0.365
Observations	16,009	16,009	16,009	32,539

DED fixed effects used in all models. P-screened sample in 1901 used for models (1) - (3) where 13,263 placebo pensions have been assigned (82% of total), with 2,746 control children. P-screening has dropped 1901 households with a low likelihood of treatment, hence the majority of observations here are "eligible" for a placebo. Model (4) uses all multigenerational households in 1901 and shows that results are unchanged. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

5 Results

5.1 Baseline Results

To examine the impact of the OAP on schooling I estimate the linear probability model as described by Equation 1, where the dependent variable is a dummy indicating whether a child is enrolled in school. The results are reported in Table 4—column (1) uses the full sample of multigenerational households and controls for all observable child characteristics. Column (2) restricts observations to the screened sample, column (3) introduces household characteristics controls to the model, and column (4) accounts for age trends.

Table 4: Baseline results

	Child Enrolled in School			
	(1)	(2)	(3)	(4)
Pension	0.090***	0.047*	0.087***	0.089***
	(0.017)	(0.027)	(0.030)	(0.031)
Household Income	0.035***	0.025***	0.027***	0.023***
	(0.003)	(0.004)	(0.005)	(0.005)
1911 Census	0.040***	0.050***	0.040***	0.039***
	(0.004)	(0.008)	(0.009)	(0.009)
Age 14	-0.192***	-0.173***	-0.173***	-0.173***
	(0.004)	(0.006)	(0.006)	(0.006)
Age: 15	-0.418***	-0.404***	-0.404***	-0.405***
	(0.005)	(0.007)	(0.007)	(0.007)
Age: 16	-0.605***	-0.596***	-0.595***	-0.596***
	(0.005)	(0.007)	(0.007)	(0.007)
Female Child	0.013***	0.008*	0.008	0.008
	(0.003)	(0.005)	(0.005)	(0.005)
Child Characteristics	✓	✓	✓	✓
Household Characteristics			✓	✓
Age Trends				✓
R^2	0.329	0.357	0.358	0.358
Observations	67,472	38,400	38,400	38,400

DED fixed effects used in all specifications. Enrolment rate of control group is 58%. Standard errors clustered at the household level. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

The OAP has a positive and statistically significant effect on schooling in all specifications. As expected, after screening out incomparable households from the sample, the average pension effect falls, demonstrating that retaining incomparable control households biases estimates upward. After eliminating these households from the sample, the average pension effect remains positive and statistically significant, with little change in the size or significance of the coefficient as more controls are introduced. The fully specified model (4) indicates that for every £1 of weekly OAP income, the probability that older children are enrolled in school increases by 8.9 percentage points (pp) compared to untreated households (95% $CI[0.03, 0.15]$). As the average OAP income for the period corresponds to £0.25 per

week, I divide this coefficient by four to obtain the true OAP effect. This suggests that the OAP increased the probability of enrolment by just over 2 pp and improved actual enrolments rate of older children by around 4 percent.

This baseline model also confirms that the probability of enrolment is a decreasing function of age in line with the increasing opportunity costs of school. Compared to the reference category of children aged thirteen, the probability that children aged fourteen are enrolled in school falls by 17 pp, 40 pp for children aged fifteen, and 60 pp for children aged 16. Furthermore, although the female enrolment rate in 1901 was 3 pp higher than that of males, the baseline results show that female children had no overall schooling advantage over their male peers. This suggests that by 1911, any earlier female advantage had been eroded.

5.2 Child Characteristics

5.2.1 Sex

In early 20th-century Ireland, traditional gender roles were deeply ingrained. Although the industrial revolution provided new opportunities for women in urban areas and women had won the right to vote and stand in local government elections in 1898, most women were limited to the domestic sphere (Daly, 1981). Indeed, as discussed in this paper's Context section, female children were taught a different curriculum at school with a focus on domestic sciences, cooking, laundry and sewing. To explore whether the effect of OAP treatment varied by sex I estimate two variants of Equation 1.

First, to explore whether the sex of the elderly recipient matters I interact a household's weekly OAP income with a binary variable that indicates whether the eligible elderly person is female.[19] This interaction term captures the extent to which the effect for elderly female recipients differs from that of elderly male recipients. Secondly, to explore whether the sex of the child mattered, I interact a female child indicator variable with OAP income. In both cases, the original pension variable now captures the male-specific OAP effect on schooling. These results are reported in Table 5.

[19]Eligibility rather than actual treatment status is used here to ensure that elderly females residing in multigenerational households in 1901 are correctly identified.

Table 5: Effect of the OAP by sex of elderly recipient and older child

	Child Enrolled in School		
	(1)	(2)	(3)
Pension	0.089***	0.031	0.152***
	(0.031)	(0.041)	(0.035)
Pension × Female Eligible		0.082**	
		(0.040)	
Pension × Female Child			-0.126***
			(0.032)
Female Child	0.008	0.008	0.026***
	(0.005)	(0.005)	(0.007)
Female Eligible	-0.021***	-0.031***	-0.021***
	(0.006)	(0.008)	(0.006)
Full Specification	✓	✓	✓
R^2	0.358	0.358	0.358
Observations	38,400	38,400	38,400

74.9% of OAP claimants in the screened sample were female, identical to the share of female claimants in the full multigenerational household sample. Standard errors clustered at the household level. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

I find that the effect of OAP treatment is highly gendered. Column (2) explores whether the sex of the elderly recipient mattered, and there is strong evidence that the OAP effect is driven by elderly female recipients. Every £1 of OAP income claimed by female recipients increases the probability that an older child is enrolled in school by 8.2 pp (95% $CI[0.00, 0.16]$), or around 2 pp for an average 5-shilling OAP. Although the male recipient effect is also positively correlated, it is statistically insignificant and less than half the size of the female recipient effect.

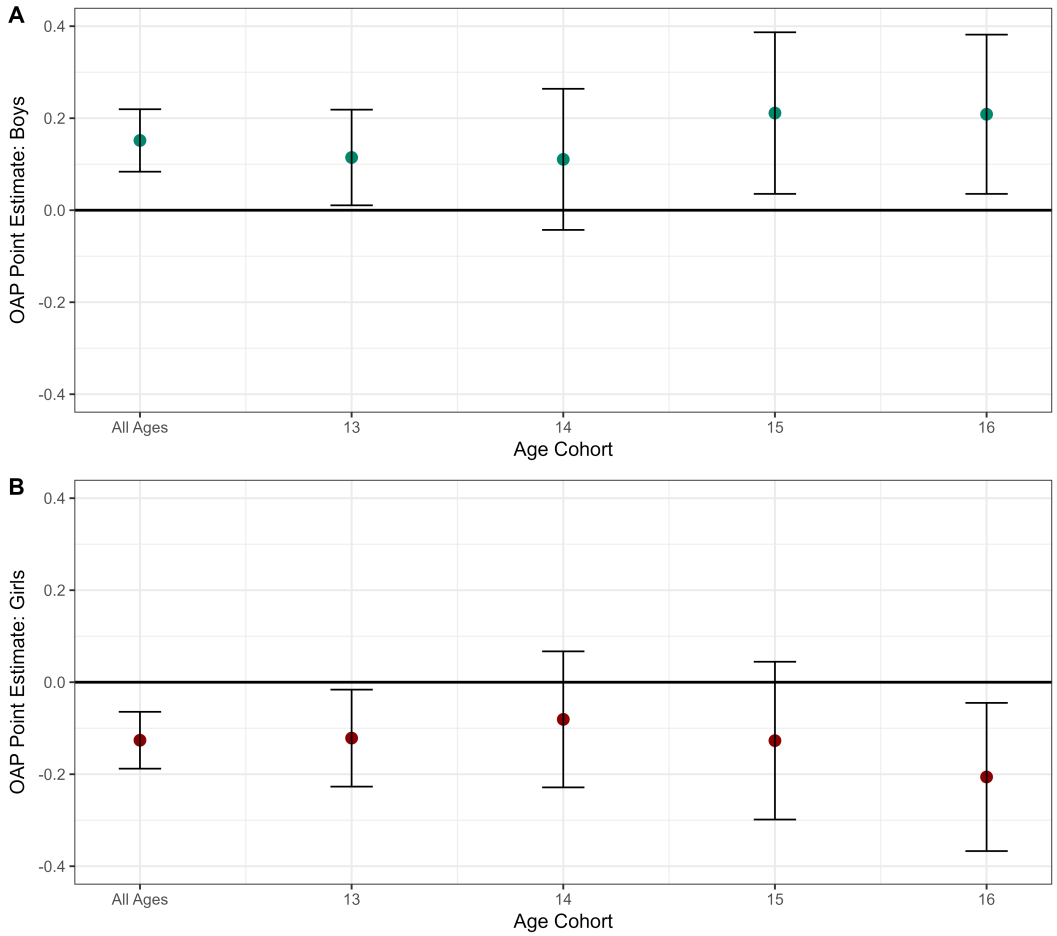
The eligible female dummy now measures the effect of a household containing an elderly, untreated female. Interestingly, the probability that older children were enrolled in school was 3.1 pp lower for households containing an elderly untreated female than elderly untreated males. Given the lack of socio-economic opportunities for women and general insecurity of old age, it is encouraging that the OAP appears to have mitigated this disadvantage, corroborating other cash transfer studies that find that targeting cash transfers towards women is more effective in improving socio-economic outcomes within in the household.

Column (3) explores whether the sex of the child mattered and I find strong evidence that it did. Female children were much less likely to benefit from the OAP than their male peers—every £1 of OAP income corresponds to an increase in the probability of enrolment 12.6 pp lower than their male peers (95% $CI[-0.19, -0.06]$). Given that a 5-shilling OAP increased the probability that male children were enrolled in school by 3.8 pp, this corresponds to an average increase in the probability that female child were enrolled by 0.65 pp, around a sixth of the size of the male effect. Notably, when this interaction is accounted for, female children in untreated households are 2.6 pp more likely to be enrolled in school compared to their male peers. As such, while both male and female children benefit from living with an OAP recipient, male children benefit disproportionally, more than offsetting their initial disadvantage compared to female children.

5.2.2 Age

Although schooling was compulsory for all children under the age of school children began to leave school around eleven years old and typically left as soon as job became available. In resource-constrained households that rely on child labour, the decision to withdraw children from school is determined by the opportunity cost of schooling. Given that baseline results confirm that these opportunity costs increase with age, I split the screened sample by age cohort and re-estimate Equation 1 on each age cohort. Since treated female children attenuate the average treatment effect, I retain the interaction between the female child dummy and the pension to explore if female enrolments differ from males by age. These results are reported in Figure 4 while Table A2 in Appendix A reports our results in tabular form.

Figure 4: Effect of OAP on schooling by age cohort and sex



Note: Panel A is the effect of the OAP for boys and Panel B is the interaction between the OAP and the presence of a female child and showing the treatment difference when the child is a girl.

The OAP mostly increased the probability of enrolment for the oldest male children for whom the opportunity costs of school were highest. For boys aged fifteen and sixteen, every £1 of OAP income increases the probability of being enrolled by 21.1 and 20.9 pp respectively (95% $CI[0.03, 0.39]$ and $[0.04, 0.38]$) , corresponding to an increase of 5.3 and 5.2 pp for a 5-shilling OAP. Thirteen year old boys also benefited from the policy, with a 5-shilling OAP increasing enrolments by 2.9 pp on average while it had no effect on enrolments of fourteen year old male children.

Comparatively, the effect for female children by age is more nuanced. Although there is little evidence that the OAP treatment effect differed for female children aged fourteen and fifteen, the coefficients of the female interaction term are negative and statistically significant for thirteen and sixteen year olds. For thirteen year old girls, the coefficient of -0.126 (95%$CI[-0.23, -0.2]$) corresponds to an overall decrease in the likelihood of enrolling

in school by 0.2 pp, compared to the 2.9 pp increase in likelihood for boys of the same age. This, along with the fact that thirteen year old untreated female children were no more likely to be enrolled in school than their male peers indicates that the OAP may actually have modestly reduced the probability of enrolment for some female children.

A similar picture emerges for sixteen year old girls, where the OAP effect relative to their male peers is practically zero. Given that sixteen year old untreated females are more likely to be enrolled in school than their male peers, this indicates that the OAP completely eroded this advantage by disproportionally increasing the enrolment rate of sixteen year old boys. In Appendix A I widen the propensity score sample to all school age children aged 6—16 and repeat the age cohort analysis. Figure A.7 reports the results and shows that the policy only begins to positively affect boys after the age of 13, whereas it has no consistent or significant effect on lower male age cohorts. Comparatively the relative treatment effect for girls is negative and increases with age. As such, I conclude that the primary beneficiary of the policy were older male children.

5.2.3 Household Equivilised Income

The results of this paper so far suggest that the OAP increased school enrolments of older males and had a relatively muted effect on female children. Although this source of heterogeneity improves our understanding of how the OAP affected schooling, it does not explain why this cash transfer to the elderly affected enrolments in multigenerational households. Given that older male children were the largest beneficiaries of the OAP, it seems likely that the OAP reduced the opportunity costs of school for the poorest. To explore this mechanism, I measure how the OAP affected enrolments at different points of the income spectrum and test whether relaxation of household resource constraints enabled children to remain enrolled in school for longer.

Using a measure of Household Equivilised Income (HEI), I split the screened sample into five equal-sized quintiles. As discussed in the Data section, HEI is calculated using the OECD Modified Scale for simplicity, and accounts for differences in household size and demography.[20] Since the OAP influenced labour supply decisions in the household, total labour and OAP income is used to prevent treated households from mechanically falling down the income distribution. The results are reported in Table 6.

[20]The first adult in the household is assigned a weight of 1, each subsequent adult is assigned 0.5, and children (under the age of 13) are assigned a weight of 0.3.

Table 6: Effect by household income quintile

	Child Enrolled in School					
	(1)	(2)	(3)	(4)	(5)	(6)
Pension	0.152***	0.185	0.295***	0.196**	0.028	0.137
	(0.035)	(0.126)	(0.094)	(0.095)	(0.096)	(0.085)
Pension × Female	-0.126***	-0.069	-0.133*	-0.102	0.056	-0.285***
	(0.032)	(0.080)	(0.078)	(0.083)	(0.086)	(0.084)
Female	0.026***	0.015	0.016	0.021	-0.017	0.078***
	(0.007)	(0.015)	(0.017)	(0.018)	(0.018)	(0.016)
HEI Quintile	All	1	2	3	4	5
Full Specification	✓	✓	✓	✓	✓	✓
R^2	0.358	0.441	0.504	0.513	0.534	0.513
Observations	38,400	7,680	7,680	7,680	7,680	7,680

Female is a dummy variable that indicates if the child is female. Quintile 1 is the poorest households and quintile 5 are the wealthiest. For quntiles 1—5, enrolment rate of the control group was 58.4%, 64.7%, 64.5%, 58.9% and 45.9% respectively. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

For males, although I find no evidence that the OAP increased enrolments in the most resource-constrained households on average, there is strong evidence of positive effects in the second and third income quintiles. This indicates that the OAP income boost was not sufficient to increase enrolments for the poorest male children for whom the opportunity costs of school were high, but it was sufficient for males in moderately better off households. I find that £1 of OAP income increased the likelihood of enrolment for males in the second income quintile by 29.5 pp ($95\% CI[0.11, 0.48]$), corresponding to an actual 7.4 pp increase in the likelihood with a 5-shilling OAP. The effect for boys in the third quintile is lower, and corresponds to an actual increase in the likelihood of enrolment by 4.9 pp. In contrast, the policy had no effect on enrolments for boys in the richest two income quintiles.

For female children, the effect of the policy on enrolments by income quintile is muted, with the coefficient on the interaction term negative across all specifications. That said, there is an overall positive effect on the likelihood of enrolments for female children in the second and third income quintiles by around 4-5 pp for a 5-shilling OAP.[21] The most substantial deviation from the male treatment effect is for females within the richest income quintile. For these female children, the likelihood of enrolment is around 7 pp less than the male effect (95% $CI[-0.45, -0.12]$) and corresponds to an overall decrease in the probability of

[21]For the third quintile, the effect for treated girls does not differ significantly from that of boys.

enrolment by 4 pp. This female effect is striking, as there is no contemporaneous change in male enrolment rates, making it unlikely that intra-household bargaining is disadvantaging female children in richer households to invest in male schooling. In fact, untreated female children in this quintile are nearly 8 pp more likely to be enrolled than their untreated male counterparts, suggesting that the OAP nearly halved this female advantage.

To conclude this results section, I explore why female children from richer households were less likely to be enrolled in school if they lived with an elderly OAP claimant. Given that that the effect of the policy is moderated by both the gender of the claimant and the child, I use a three-way interaction of weekly OAP income, the gender of the child, and the gender of the claimant to develop a richer understanding of these gender effects. To assist with interpretation, the original pension variable can be considered as the effect of an OAP where the recipient and the child are both male, pension interacted with a female child measures the effect of the OAP on female children when the claimant is male and the three-way interaction represents the difference from this effect when both the child and the claimant are female. Table 7 reports the results where interactions have been renamed for clarity. Column (1) uses the full sample and column (2)—(6) divides the sample by HEI quintile.

For the full sample, after accounting for the moderating effect of the child's and claimant's sex, the effect of the policy remains positive and statistically significant for male children living with female claimants. For every £1 of OAP income, the likelihood that male children living with a female claimant are enrolled in school increases by 14.2 pp 95% $CI[0.03, 0.26]$. Since this captures an effect in addition to the baseline pension effect when both child and claimant are male, it corresponds to a 5.3 pp increase in the likelihood of enrolment for a 5-shilling OAP. Although the three-way interaction is negative, it is not statistically significant, suggesting that on average, female children living with female claimants are no more or less likely to be enrolled than their male counterparts. Similarly, while the average effect of the policy for girls living with male claimant is lower, this difference is not statistically significant.

That said, these effects are heterogenous across the income distribution. Column (2) explores the effect of the OAP on the poorest households and confirms, regardless of the sex of the claimant, that the OAP had no significant effect on the enrolment rate of boys. In comparison, there is some evidence that the policy increased enrolments when girls in the poorest households live with a female claimant. The coefficient of this three-way interaction is -0.423, is statistically significant 95% $CI[-0.85, -0.04]$, and describes the differential effect of the policy on girls and boys living with male claimants and boys living with female claimants. This corresponds to a net increase in the likelihood of enrolment of 7.4 pp, or by 1.9 pp for a 5-shilling pension. Interestingly, girls living with untreated female claimants were 5.8pp

more likely to be enrolled than their boy peers, indicating that the OAP increased this female schooling advantage among the poorest.

Column (6) describes the effects of the policy on the richest income quintile and confirms that the policy had a large, statistically significant negative effect on the enrolment of female children. When the claimant is male, every £1 of OAP income reduces the likelihood that female children are enrolled in school by 39 pp compared to their male counterparts 95% $CI[-0.77, -0.01]$, implying a 12.5 pp decrease for a 5-shilling pension. Given the effect of untreated elderly males increases the enrolment rate of girls by 6.1 pp, this confirms that the OAP not only negates this advantage, but systematically reduces the enrolment rate of girls in the richest households. Comparatively, girls living with female claimants experience no significant difference to enrolments, while boys living with female claimants see an increase of around 11 pp. Figure A.5 in Appendix A provides a graphical representation of these results.

To enrich our understanding of this negative OAP effect on girls residing with male claimants in the richest households, all male children are dropped from the sample to explore how the OAP changed the likelihood that girls were (1) enrolled in school, (2) were employed, and (3) were primarily engaged in domestic housework.[22] Table 8 reports the results and shows that practically the entire negative effect on schooling is explained by a proportional increase in the share of girls doing domestic work. Comparatively, when the claimant is female, both the change in schooling and domestic work are close to zero. This suggests that in richer households with retired men, female children were expected to stay home and assume additional domestic responsibilities that, during this time, were traditionally carried out by women.

[22]In the census, households were instructed to leave the occupation question blank for individuals who were primarily engaged in domestic work.

Table 7: Three way interactions of OAP payments, child and claimant sex

	Child Enrolled in School					
	(1)	(2)	(3)	(4)	(5)	(6)
Boys aged 13 - 14 living with						
Untreated Elderly Female	-0.050***	-0.064**	-0.023	-0.023	-0.073**	-0.096***
	(0.011)	(0.027)	(0.030)	(0.033)	(0.031)	(0.027)
Male Claimant	0.068	0.065	0.300**	0.290*	-0.082	-0.108
	(0.056)	(0.174)	(0.146)	(0.151)	(0.164)	(0.151)
Female Claimant	0.142**	0.157	-0.081	-0.012	0.214	0.563***
	(0.059)	(0.155)	(0.144)	(0.160)	(0.174)	(0.171)
Girls aged 13 - 14 living with						
Untreated Elderly Male	-0.001	-0.033	-0.017	0.018	-0.025	0.061**
	(0.011)	(0.027)	(0.030)	(0.029)	(0.029)	(0.024)
Untreated Elderly Female	0.036**	0.058*	0.029	0.000	0.029	0.062*
	(0.014)	(0.033)	(0.039)	(0.039)	(0.039)	(0.036)
Male Claimant	-0.049	0.275	-0.094	-0.201	-0.102	-0.390**
	(0.068)	(0.167)	(0.164)	(0.176)	(0.195)	(0.194)
Female Claimant	-0.075	-0.423**	0.035	0.148	0.153	-0.012
	(0.080)	(0.195)	(0.191)	(0.208)	(0.228)	(0.230)
HEI Quintile	All	1 - poorest	2	3	4	5 - richest
Full Specification	✓	✓	✓	✓	✓	✓
R^2	0.212	0.343	0.404	0.414	0.408	0.388
Observations	38,400	7,680	7,680	7,680	7,680	7,680

Interaction have been renamed for clarity. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

Table 8: Effect of the OAP on schooling, employment and domestic work

	School	Employed	Domestic
	(1)	(2)	(3)
Pension (Male Claimant)	-0.475**	0.053	0.422**
	(0.186)	(0.191)	(0.180)
Pension × Female Eligible	0.420**	-0.031	-0.389**
	(0.199)	(0.204)	(0.182)
Female Eligible	-0.023	0.016	0.007
	(0.035)	(0.034)	(0.026)
Full Specification	✓	✓	✓
R^2	0.473	0.454	0.409
Observations	3,650	3,650	3,650

Sample is for female children in the richest households only. Domestic work is indicated by leaving the occupation category blank in the census return in accordance with the instructions provided to households. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

5.3 Robustness

For identification to be valid, changes in multigenerational household characteristics should not be correlated with OAP eligibility. To ensure this, I employed a propensity score screening technique to address the problem of some unobservable characteristics correlated with treatment from biasing my estimates. However, propensity score screening only approximates a randomised experiment, and as Figure 3 shows, some imbalance remains. Indeed, since the screening is done through a rule of thumb– namely selecting observations with $0.15 < p(X)_i < 0.85$– King and Nielsen (2019) show that excessive screening may actually increase imbalance, model dependence, and bias.

To show that my results are not dependent on the choice of matching, I employ a range of alternative matching techniques including nearest neighbour with and without a caliper, mahalanobis distance, and exact matching. With the exception of exact matching, children are matched using the same set of observable characteristics used to generate propensity scores. To retain explanatory power for the exactly matched sample, I exchange the age of the eldest person in the household and its square with a simple dummy that indicates whether a relative over the age of seventy is present.

Regression results are reported in Table A3 in Appendix A and show, with the exception of the exactly matched sample, that the effect of the OAP is robust with little difference in

the size or significance of the coefficient. For the exactly matched sample, the size of the OAP effect on male children more than doubles and is statistical significant ($p < 0.05$) whereas the effect for female children remains negative but is no longer statistically significant. The larger effect for males and insignificant negative effect for females is likely because in the exactly matched sample, treated households are 15% poorer than in the full multigenerational household sample. As shown in this paper's main results section, most of the positive effect of the policy is driven by male children lower down on the income spectrum, while the female child negative effect is driven by female children in the richest households. Therefore, I conclude that these results are fully robust to alternative matching techniques.

Although these matching techniques ensure that treated and untreated households are more comparable, the extensive nature of age misreporting in the 1911 census to obtain an OAP deserves explicit attention. Age misreporting could create a source of endogeneity if households that were more likely to misrepresent their ages to an enumerator were also more likely to lie about whether their children were enrolled in school or not. To show that these results are explicitly robust to age misreporting, I develop a measure of age misreporting at Poor Law Union level ($n = 158$) by taking the difference between the share of the total population where were aged between fifty-five and sixty-four in 1901, and the share of the population aged sixty-five to seventy-four ten years later. This measure is normalised between 0 and 1 and exclude children residing in the top 30% of age misreporting areas. Results are reported in Table A4 in Appendix A and are fully robust to dropping high age misreporting areas. I also show that results are robust to the exclusion of cities and urban areas, as well as areas where a high portion of returns were made in Irish.

Finally, as noted in the data section, the total number of OAP claims across all households, both multigenerational and otherwise, are overestimated by 8 per cent. In Appendix A, Figure A.3, I show that while the number of assigned OAPs is uncorrelated with age misreporting intensity, the gap between actual and assigned OAPs is modestly correlated at county level, suggesting over-assignment in areas with high age misreporting. Although my results are robust to age misreporting, the presence of children in the treatment group who were not actually treated will bias estimates downwards toward zero. I confirm if this is the case by dropping all children who reside in counties where treatment has been over-assigned. These results are reported in Table A5 in Appendix A and show that while treatment group contamination does reduce the magnitude of the coefficients for both male and female children, the size of this difference is small and corresponds to only 0.25 pp difference in likelihood for boys and 0.03 pp for girls.[23] Consequently, treatment group

[23]In percentage terms, the difference for boys is around 6 percent of the original effect size and 4 per cent for girls.

contamination is not problematic for the scale, direction, or significance of the results, and likely reflects an over-assignment of treatment to non-multigenerational households.

6 Discussion and Conclusion

This paper has shown that following a substantial cash transfer that targeted the elderly, school enrolments increased for children living in multigenerational households containing an elderly pensioner. Although transfers to female claimants were more effective in translating into improved schooling outcomes on average, the sex of the child further moderates this effect. For girls, while transfers to claimants in the poorest households increased enrolments when the claimant was female, enrolments fell when transfers were made to male claimants in the richest households.

In contrast, boys are the primary beneficiaries of the policy, particularly older boys from poorer socio-economic backgrounds. The OAP increase the enrolment rate of boys in the second and third income quintiles, and although the estimated effect of female claimants is smaller than for male claimants, this difference is not statistically significant. That said, in the richest households, boys living with female claimants are much more likely to enrol in school. In the poorest households, the transfer had no significant effect on boy's enrolment, regardless of the recipient's sex, unlike for girls. This indicates that the OAP was not sufficient to induce the poorest to remove male children from the labour, but was sufficient to invest in female schooling.

The evidence presented in this paper aligns with an opportunity cost hypothesis that is moderated by gender and intra-household bargaining processes. In cases where poor households cannot actualise their demand for schooling, a UCT like the OAP will allow households to invest in schooling for those children on the margins of attending school. Boys, even those from the poorest backgrounds, had more socio-economic opportunities than girls who were banned from holding many occupations and were systematically paid less (Bourke, 1993). By relaxing household resource constraints, the OAP reduced the opportunity costs of attending school for older boys, who's schooling prior to the intervention was cost constrained. Given that this did not affect boys in the poorest household implies that the transfer was not sufficient relax the opportunity cost constraint on school.

As the policy enabled elderly claimants to retire (Giesecke and Jäger, 2021) it may have been unrealistic for the poorest households to remove children from work. A 5-shilling OAP was valued around 1.6 times the average weekly wage of a typical child labourer meaning a removal of these children from the workforce could have led to a sharp fall in

living standards.[24] This aligns with other studies that identify stronger effects of anti-poverty studies when targeted at moderately wealthier households rather than the poorest (Balboni et al., 2022). Finally, it is noteworthy that although female children were more likely to be enrolled in school before the intervention, intra-household bargaining processes and the subsequent prioritisation of male schooling eroded this gap throughout the income distribution. Indeed, since the negative effect of the policy on female enrolments in the richest households was eight times larger when the claimant was male suggests that the policy likely entrenched pre-existing gender norms as female children were taken out of school to assist in the household.

This paper offers several policy-relevant insights in contexts where there are deeply rooted gender norms. Firstly, it reiterates the finding that cash transfers allocated to female household members are more effective at improving wider household socio-economic outcomes. Secondly, unconditional cash transfers that target the elderly can be effective at improving schooling outcomes, particularly when schooling has been disrupted and demand is constrained. However, since the effect of the policy is further moderated by gender and intra-household bargaining processes, applying conditions to payments that require households to send non-marginal children to school may bypass these biases. Finally, this paper shows that even in response to a highly generous intervention, children in the poorest households still may not enrol in school, suggesting that the costs of correcting the misallocation of children to the labour force exceed the value of the OAP transfer. However, this does not preclude the possibility that the poorest households benefitted in other ways, particularly through improved consumption and access to nutrition.

There are a number of potential avenues for future research. Although this paper has shown that the OAP created more opportunities for male children to enrol in school, it is unknown whether this corresponded to an increase in school attendance or human capital. Future work could integrate school-level attendance figures for the period and explore, at a district level, whether the OAP also improved school attendance. Moreover, there are ample opportunities to explore the policy's effects on socio-economic status, poverty, and wider labour supply. Indeed, the anticipated release of the 1926 Irish Free State census household returns offer an opportunity to explore the long-run effect of the OAP on schooling.

Socio-economic context clearly matters. In early 20th-century Ireland, gender norms were deeply rooted and were a powerful institution that moderated socio-economic behaviour. The drought of economic opportunities for women reinforced by a gendered schooling system,

[24]News vendor wages taken from the 1902 Inter-Departmental Committee on the employment of children. The report surveyed several hundred children employed in street trading. The given figure is the average wage of news-vendor boy at Chancery Lane, Dublin.

undoubtedly entrenched the constraints that limited most women to the domestic sphere. Unsurprisingly, allocating an unconditional cash transfer such as an OAP to households can entrench and reproduce these gender norms. That said, unconditional cash transfers that target the elderly can be effective at improving schooling for children already on the margins of attending school, the remaining challenge for policymakers is to ensure that these policies do not reinforce societal prejudices and allow children who are less likely to attend school to also benefit.

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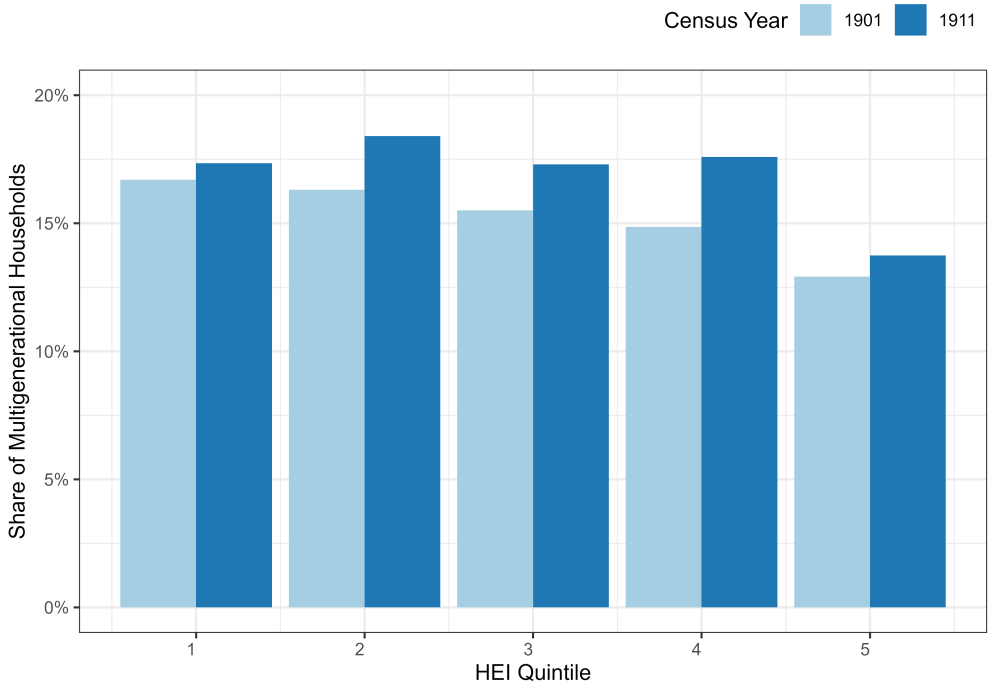
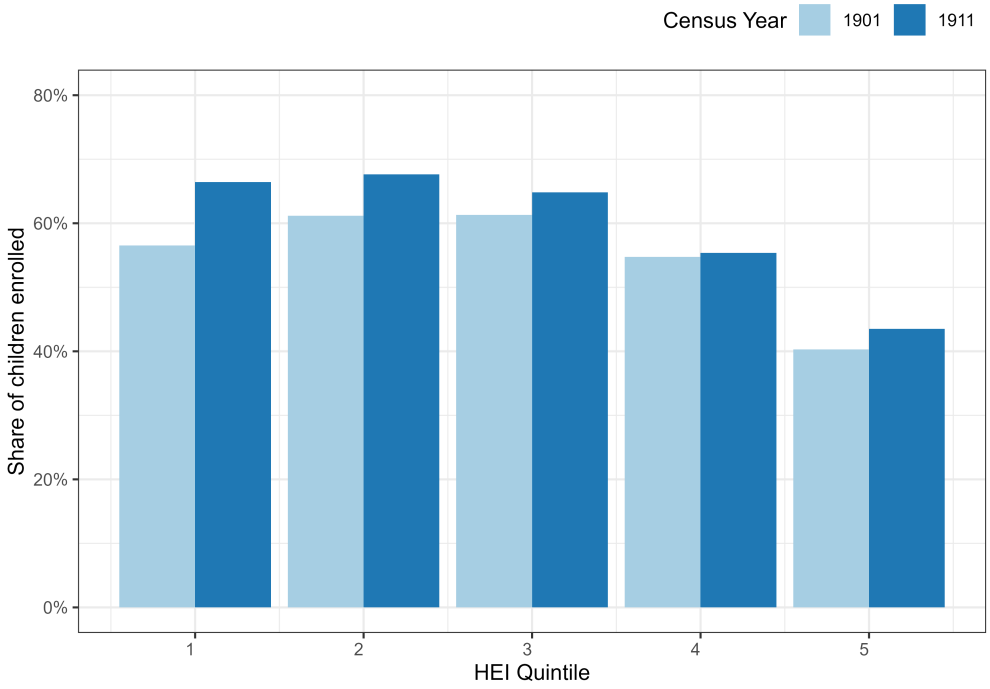
A Appendix

Figure A.1: Frequency of multigenerational household by income quintile



Note: Share of multigenerational households increases modestly. For quntiles 1—5 change is 0.65 pp, 2.09 pp, 1.75 pp, 2.74 pp and 0.82 pp respectively.

Figure A.2: Share of children enrolled in school by income quintile



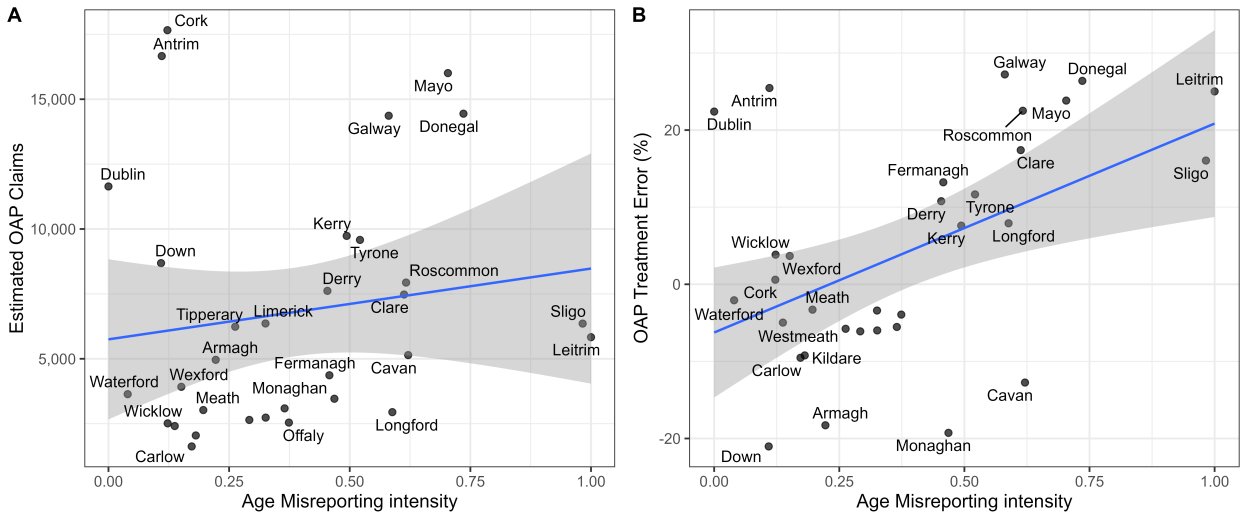
Note: School enrolment declines with household income. By focusing on older children aged 13 to 16, I capture those no longer compelled to attend school. In wealthier households, schooling was less disrupted by the agricultural calendar, and children were more likely to complete schooling by the age 14.

Table A1: Correlation between treatment and covariates before screening

	Pension (1)	70 y/o (2)	13 y/o (3)	14 y/o (4)	15 y/o (5)	16 y/o (6)
Pension		0.756***	0.039**	0.002	-0.011	-0.029
		(0.015)	(0.018)	(0.019)	(0.019)	(0.018)
Household Income	-0.010***	0.010***	-0.003	-0.004	-0.002	0.010***
	(0.001)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
1911 Census	0.134***	-0.082***	-0.015***	-0.005	0.011***	0.010**
	(0.001)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Child Enrolled in School	0.003***	0.000	0.275***	0.093***	-0.095***	-0.273***
	(0.001)	(0.002)	(0.003)	(0.004)	(0.004)	(0.003)
Child Age: 14	-0.001	-0.001				
	(0.001)	(0.003)				
Child Age: 15	-0.002**	-0.005*				
	(0.001)	(0.003)				
Child Age: 16	-0.003**	-0.003				
	(0.001)	(0.004)				
Sex: Female	0.000	-0.002	0.001	-0.010***	-0.001	0.010***
	(0.001)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Illiterate	0.002	0.018**	0.062***	0.015	0.000	-0.077***
	(0.002)	(0.007)	(0.010)	(0.012)	(0.012)	(0.012)
Bilingual	-0.003**	-0.001	-0.021***	-0.006	0.014**	0.013***
	(0.001)	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)
Farming Household	0.000	-0.002	-0.008*	-0.005	0.006	0.007
	(0.001)	(0.003)	(0.004)	(0.005)	(0.004)	(0.004)
Roman Catholic	0.004***	0.003	0.000	0.009	-0.005	-0.004
	(0.001)	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)
Household Head Illiterate	0.008***	0.009***	0.016***	0.015***	-0.013***	-0.019***
	(0.001)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Eldest Child	0.004***	-0.001	-0.286***	-0.175***	0.182***	0.279***
	(0.001)	(0.002)	(0.004)	(0.005)	(0.004)	(0.003)
Urban Household	-0.003	0.014*	0.002	0.010	-0.007	-0.005
	(0.003)	(0.008)	(0.010)	(0.011)	(0.011)	(0.010)
Age of Eldest	0.003***	0.051***	0.005***	-0.001	-0.002	-0.003**
	(0.000)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Age of Eldest^2	0.000**	0.000***	0.000***	0.000	0.000	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Household Size	0.000***	0.001*	-0.010***	-0.006***	0.009***	0.007***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Eligible Female	0.087***	0.252***	0.010**	0.002	-0.004	-0.007*
	(0.001)	(0.003)	(0.004)	(0.005)	(0.004)	(0.004)
Full Specification	✓	✓	✓	✓	✓	✓
R^2	0.596	0.783	0.242	0.088	0.093	0.240
Observations	67,472	67,472	67,472	67,472	67,472	67,472

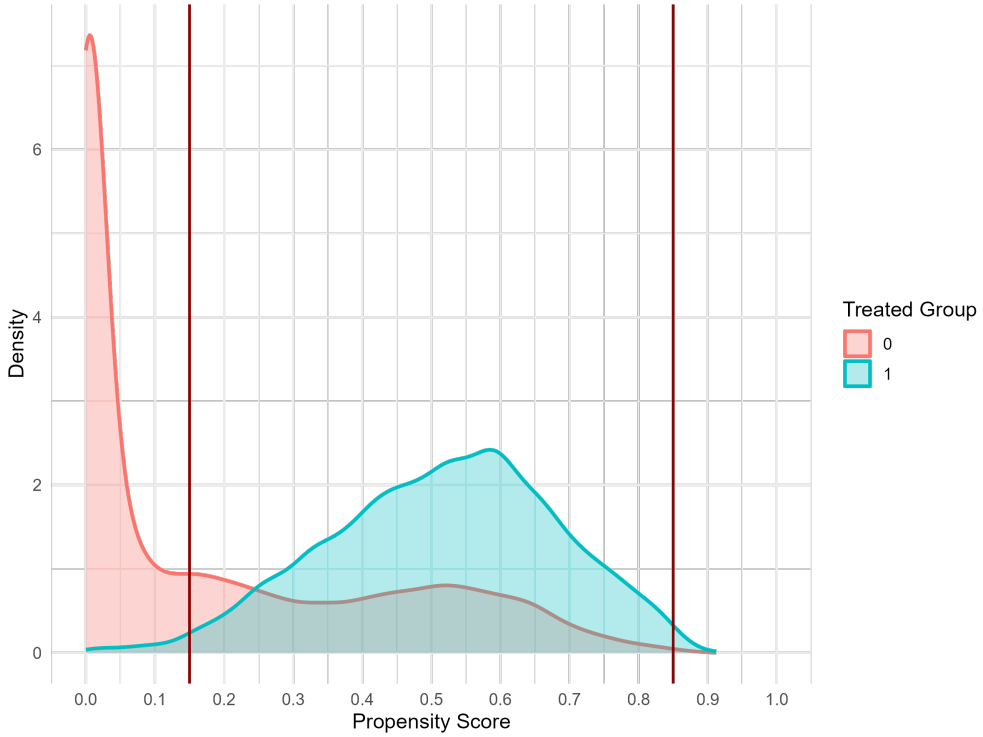
70, 13, 14, 15, and 16 y/o are dummies that indicate whether households contain an over 70 year old or a 13, 14, 15 or 16 year old child. Significance levels: ***p<0.01, **p<0.05, *p<0.1

Figure A.3: Bivariate regressions of estimated OAP claims and error $\sim$ age misreporting intensity



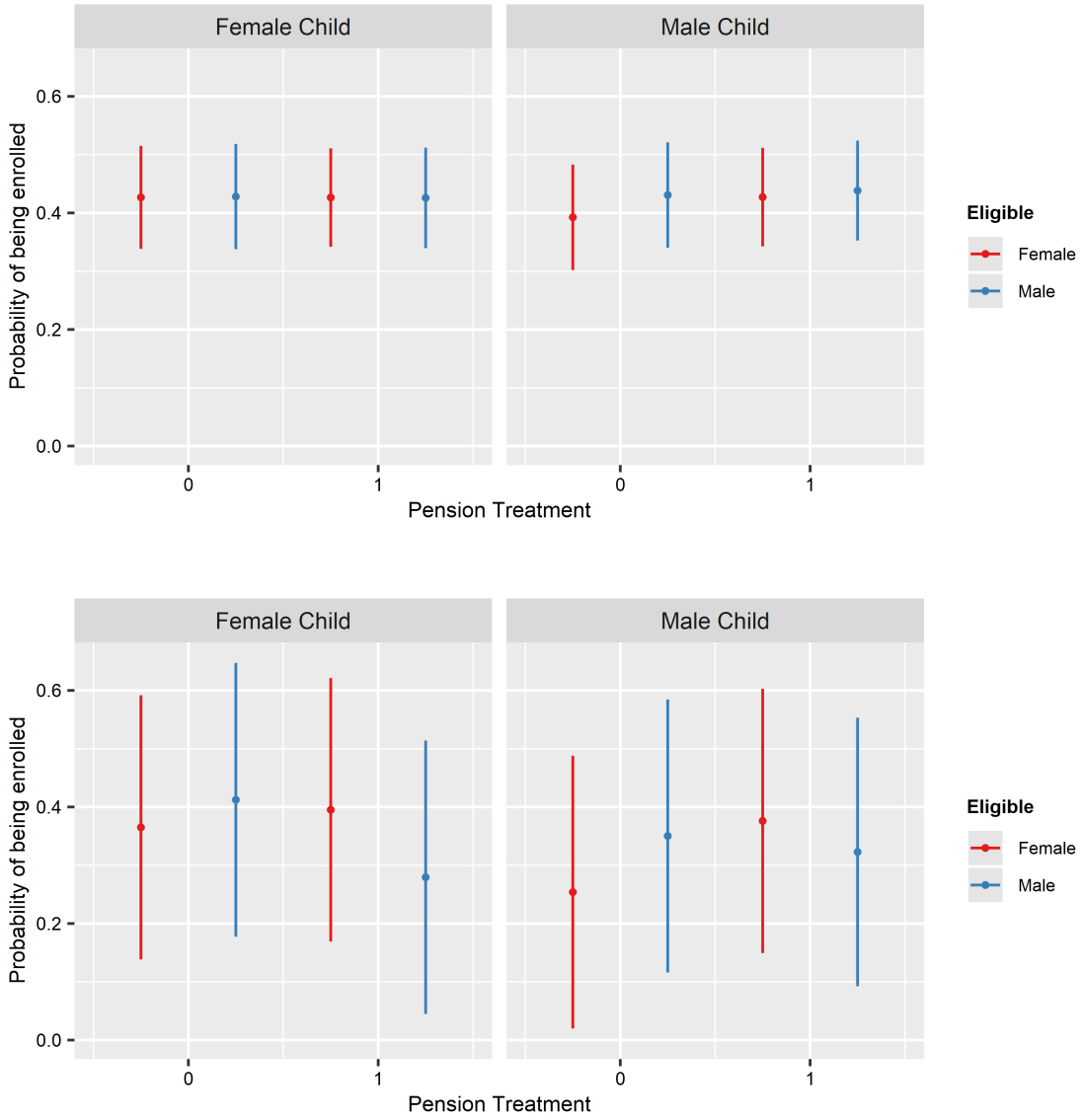
Note: Panel A shows no obvious correlation between estimated OAP claims and age misreporting intensity at county level. Panel B shows a modest positive correlation between the extent that treatment is assigned erroneously. This suggests that treatment is over-assigned to counties with higher instances of age misreporting.

Figure A.4: Propensity score density plot by treatment status



Note: Treated Group 1 indicates children residing with an OAP recipient in 1911 and 0 are all untreated multigenerational households. Vertical lines represent the screening threshold where $0.15 < p(X_i) < 0.85$.

Figure A.5: Point estimates likelihood of enrolment by treatment status, sex of child and sex of eligible person.



Note: Top panel uses full propensity score screened sample and bottom panel uses the richest 20% of households only. 95% confidence intervals shown around point estimates.

Table A2: Heterogeneity by age cohort and sex

	Child Enrolled in School				
	(1)	(2)	(3)	(4)	(5)
Pension	0.152***	0.115**	0.111	0.211**	0.209**
	(0.035)	(0.053)	(0.078)	(0.090)	(0.088)
Pension × Female Child	-0.126***	-0.122**	-0.081	-0.127	-0.206**
	(0.032)	(0.054)	(0.075)	(0.087)	(0.082)
Female Child	0.026***	0.014	0.005	0.048***	0.048***
	(0.007)	(0.012)	(0.016)	(0.018)	(0.016)
Age Cohort	All	13 y/o	14 y/o	15 y/o	16 y/o
Full Specification	✓	✓	✓	✓	✓
R^2	0.358	0.325	0.330	0.398	0.394
Observations	38,400	10,276	10,460	9,035	8,629

Control group mean enrolments for ages 13 - 16 are 86%, 67%, 45% and 26% respectively. Standard errors clustered at household level. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

Table A3: Robustness to alternative matching techniques

	Child Enrolled in School					
	(1)	(2)	(3)	(4)	(5)	(6)
Pension	0.127***	0.149***	0.111***	0.127***	0.139***	0.314**
	(0.028)	(0.036)	(0.038)	(0.035)	(0.034)	(0.152)
Pension × Female Child	-0.129***	-0.126***	-0.137***	-0.137***	-0.144***	-0.148
	(0.025)	(0.032)	(0.033)	(0.030)	(0.030)	(0.131)
Female Child	0.023***	0.026***	0.026***	0.026***	0.028***	0.035
	(0.004)	(0.007)	(0.007)	(0.006)	(0.006)	(0.029)
Matching	None	P-Screen	NN	NN + C	Mah	Exact
Full Specification	✓	✓	✓	✓	✓	✓
R^2	0.331	0.358	0.361	0.355	0.353	0.563
Observations	67,472	38,400	34,772	38,074	38,074	3,887

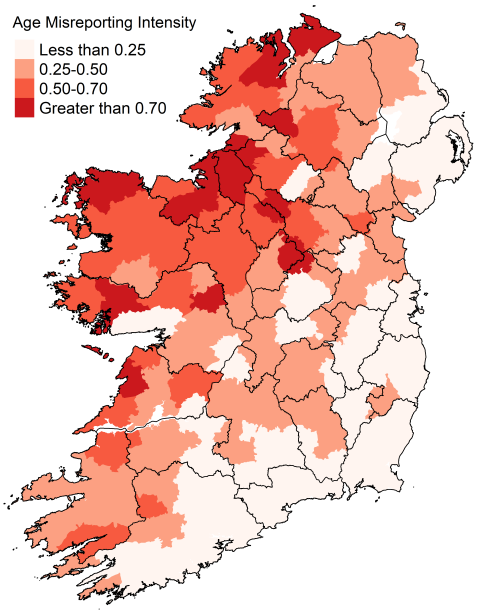
Matching for (2) - (5) conducted using identical observables. (6) differs and uses a dummy to indicate if an over 70 year old in household rather than age of eldest relative. Caliper is 0.01. Standard errors cluster at the household level. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

Table A4: Robustness to age misreporting, cities and Irish speaking areas

	Child Enrolled in School					
	(1)	(2)	(3)	(4)	(5)	(6)
Pension	0.149***	0.119***	0.141***	0.139***	0.129***	0.111**
	(0.036)	(0.040)	(0.037)	(0.039)	(0.040)	(0.043)
Pension × Female Child	-0.126***	-0.119***	-0.126***	-0.100***	-0.100***	-0.104***
	(0.032)	(0.034)	(0.032)	(0.034)	(0.034)	(0.037)
Female Child	0.026***	0.031***	0.027***	0.020***	0.021***	0.025***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Sample	Baseline	AMR	Irish	City	City/Irish	Drop all
Full Spec	✓	✓	✓	✓	✓	✓
R^2	0.358	0.354	0.359	0.360	0.361	0.361
Observations	38,400	33,010	37,893	32,571	32,085	28,533

AMR drops all high age misreporting areas, Irish drops areas with a high number of census returns in Irish, City drops all children residing in urban areas. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

Figure A.6: Map showing age misreporting intensity at Poor Law Union level.



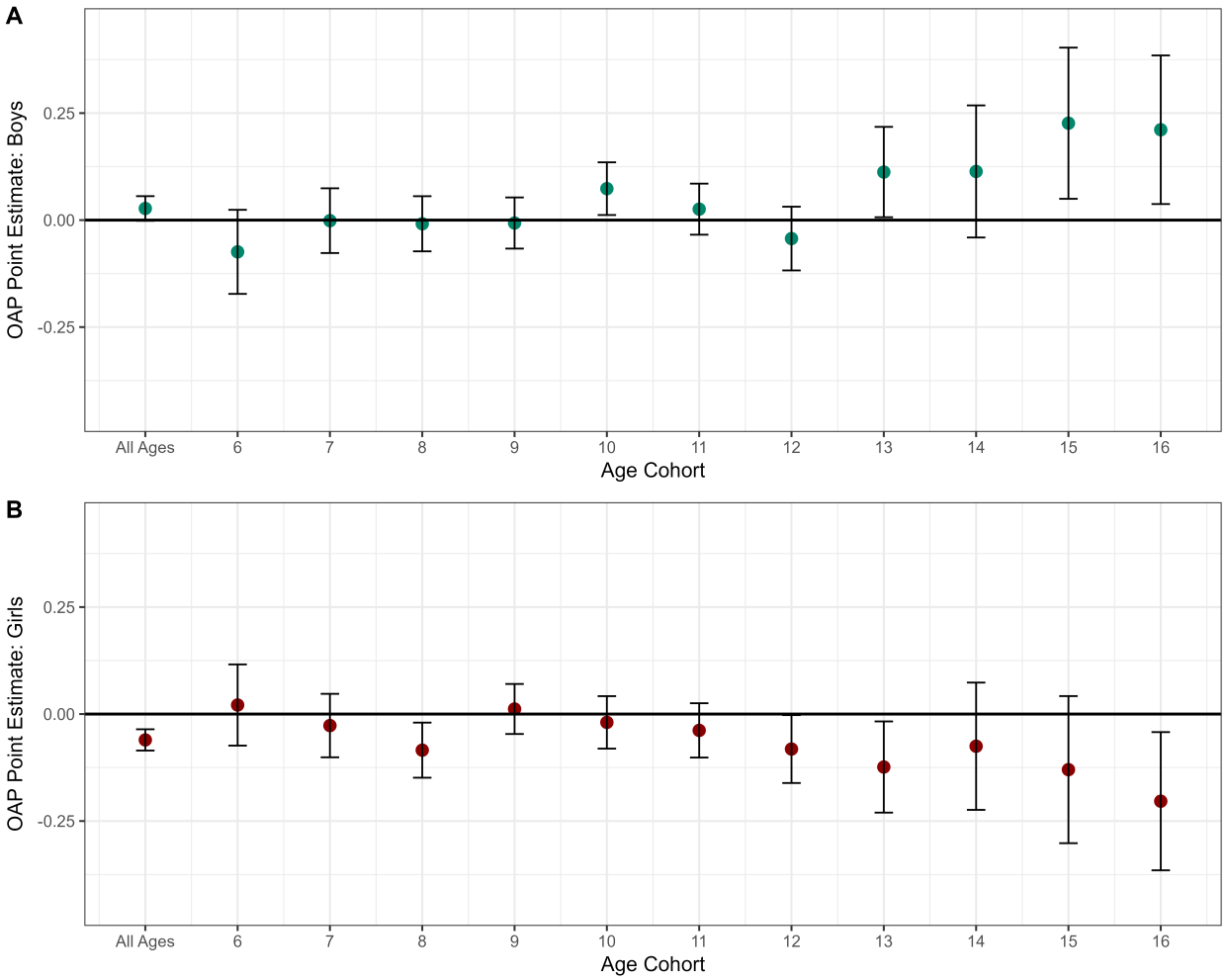
Note: Age misreporting is the difference between the share of the population aged between 55 and 64 in 1901 and the share of the population aged 65 to 74 ten years later in 1911. This measure is normalised between 0 and 1 where 1 indicates the area with the most occurrences of age misreporting. Children residing in the darkest red areas are dropped as part of my robustness exercises.

Table A5: Robustness tests for treatment group contamination

	Child Enrolled in School			
	(1)	(2)	(3)	(4)
Pension	0.149***	0.162***	0.148***	0.162***
	(0.036)	(0.038)	(0.038)	(0.039)
Pension × Female Child	-0.126***	-0.129***	-0.131***	-0.135***
	(0.032)	(0.032)	(0.033)	(0.034)
Female Child	0.026***	0.029***	0.025***	0.028***
	(0.007)	(0.007)	(0.007)	(0.007)
Sample	Baseline	No Down	No Cork	No Down/Cork
Full Specification	✓	✓	✓	✓
R^2	0.358	0.356	0.361	0.359
Observations	38,400	36,674	35,015	33,289

Enrolment rate of control group is 58%. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

Figure A.7: All children of school age 6–16 cohort estimates



Note: Panel A is the effect of the OAP for boys and Panel B is the interaction between the OAP and the presence of a female child showing the treatment difference when the child is a girl. Policy only begins to positively affect older boys. The effect for girls is lower, with this effect increasing with age.

B Appendix B

B.1 Wage Data Sources

Statistic	Description	Source
Crop Yields and Acreage	Crop yields (weight) by Poor Law Union. Limited to crops with available price data: wheat, oats, barley, rye, flax, and potatoes.	British Parliamentary Papers (1911a, 1912b).
Livestock	Animal counts by age and Poor Law Union. Limited to stock with available price data: cattle (bulls, milch, heifers, calves), sheep (rams, ewes, lambs), and pigs (boars, sows, piglets).	As above.
Crop and Livestock Prices	Price data in GBP (£) for all crops and livestock above. Crop prices by weight and animal prices by headcount.	British Parliamentary Papers (1912a).
Milk Prices	Average milk price per gallon in GBP (£), paid by creameries 1897–1918.	McLaughlin et al. (2023).
Farm sizes	Average farm size in acres by Poor Law Union.	British Parliamentary Papers (1913a).
Marketable Share of Product	Estimates of the marketable share of agricultural production.	Barrington (1927) and Turner (1987).
Agricultural Commodity Income	Production and prices of auxiliary agricultural commodities: milk, wool, hides, turf, poultry, horses, and slaughter of spent animals.	Turner (1987) and British Parliamentary Papers (1913e).
Domestic Servants	Average wages in GBP (£) of female domestic servants across 359 households in Dublin, Belfast, Cork and Limerick. Includes cooks, dairymaids, kitchen maids, lady's maids, laundry maids, parlour maids, house maids, nurses, and housekeepers.	Collet (1899).

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Statistic	Description	Source
Spinners, Weavers, and Winders	Weekly starting and maximum salaries for female textile workers in the Belfast Linen industry.	British Parliamentary Papers (1908b).
Railway Workers	Average salaries of railway workers across three major Irish rail networks by age, sex, and occupation. Includes inspectors, drivers, porters, brakemen, engineers, van-men, draymen, watchmen, signalmen, firemen, cleaners and stokers.	British Parliamentary Papers (1893).
Postal Workers	Rubric describing starting wages, annual pay progression, and maximum salaries of postal workers employed by the General Post Office of Ireland (Dublin). Includes postmen, sorters, porters, messengers, labourers, and carriers.	As above.
Tradesmen	Hourly wages, weekly hours worked, and weekly income of tradesmen in 43 Irish towns/cities. Includes carpenters, bricklayers, masons, slaters, plumbers, plasterers, smiths, turners, fitters, pattern-makers, brass moulder/finishers, iron founders, platers, shipwrights, riveters, bookbinders/printers, and compositors.	British Parliamentary Papers (1901b,a).
Urban Labourers	Average weekly wages by sex of general labourers and stone-breakers.	Ó Gráda (1994).
Farm Labourers	County average weekly wages of farm labourers (men and boys) and ploughmen. Minimum and maximum wages also reported by accommodation status: full-board, half-board, and money wages only.	Fitzpatrick (1980) and British Parliamentary Papers (1911b).

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Statistic	Description	Source
National School Teachers	Rubric describing starting wages, triennial pay progression, and maximum salaries by sex and occupation. Includes assistant teachers, teachers (various grades), head teachers, and class monitors.	Dale (1904).
Royal Irish Constabulary (RIC) and Dublin Metropolitan Police (DMP)	Rubric describing starting wages, annual pay progression, and maximum salaries by occupation and police force. RIC includes district inspectors (various grades), head constable major, head constable, sergeant, acting sergeant, and constables. DMP includes chief superintendent, superintendent, inspectors, station sergeant, sergeant, constables, supernumeraries and detective officers.	British Parliamentary Papers (1914) and Constabulary (Ireland) Act (1908).
Medical Doctors	Individual level wage data for 808 medical doctors employed by the Irish Poor Law by dispensary district. Includes starting wages, pay progression, and maximum salaries.	British Parliamentary Papers (1911c).
Public Sector Clerks	Various rubrics describing starting wages, annual pay progression, and maximum salaries of clerks employed in Government departments, Includes clerks (various classes), assistant clerks, and temporary clerks employed by the Irish National Health Insurance Commission, Local Government Board, and Land Commission.	British Parliamentary Papers (1913c, 1897a).

B.2 Wage Data Generation

Statistic	**Description**	**Geography** (n)
Labourer's Wages	Farm labourers assigned average weekly wage at county level. Urban labourers assigned national average by sex.	County (32) and National (1).
Farmer Crop	Annual crop yields adjusted for markable share of produce and converted into income using 1911 prices.	Poor Law Union (158).
Farmer Livestock	Disappearance rate of animals between 1910–1911 proxies livestock slaughter rate. Disappearance adjusted for natural mortality rates of cattle and sheep (5 and 12 per cent respectively) as per Turner (1987). Number of "disappeared" animals converted into income using 1911 prices.	Poor Law Union (158).
Farmer Milk	Working dairy cows is total milch cows minus heifers, and a further 5 per cent for animals that have been allowed to go dry. Each cow is allowed to produce 375 gallons (1704 litres) of milk annually as the mid-point of Turner (1987) and British Parliamentary Papers (1913e). 35 gallons is subtracted from total production for each calf and 5 gallons for each piglet in line with Turner (1987). Milk production converted into income using average price per gallon.	Poor Law Union (158).
Farmer Auxiliary Commodities	As per Turner (1987), adult sheep produce one 11b (5kg) fleece annually and 95 per cent of adult poultry produce 50 eggs annually each. Commodities converted into income as before. To account for missing production (turf, horses, poultry, hides, etc.), a final 5 per cent mark-up is applied to every item of production.	Poor Law Union (158).

Continued on next page

Statistic	**Description**	**Geography** (n)
Farmer's Wages	Income per acre is estimated by dividing the total income from crops, livestock, milk, and auxiliary product by total agricultural acreage. This is multiplied by average farm size to estimate income per agricultural holding. Wages are assigned to the first farmer in each household via areal interpolation using their District Electoral Division of residence.	Poor Law Union (158).
Domestic Servants	Average salaries assigned by occupation. Male domestic servants receive a 16 per cent salary bonus in line with the gender pay gap in teaching.	National (1).
Textile Workers	Average starting salaries assigned by occupation and sex to all weavers, spinners, and winders. Annual pay progression assigned by calculating an arbitrary measure of work experience $(age_i - 18)$, where negative values are coded to zero. As before, male wage progression is 16 per cent larger than females and is assigned until the relevant maximum salary is reached.	National (1).
Railway Workers	The three historical railway networks are matched to their nearest county. Railway worker wages are assigned by network, occupation, age, and sex. In County Dublin, where the networks converged, workers are assigned the average wage by occupation across all three networks.	County (32)

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Statistic	**Description**	**Geography** (n)
Tradesmen	An average tradesman wage is calculated across 43 localities and aggregated to Poor Law Union. Wages are extrapolated to Unions without any wage data using inverse distance weighting and distance to the nearest Union where tradesman wages are known. Wages are assigned to individuals by areal interpolation using District Electoral Division of residence.	Poor Law Union (158).
National School Teachers	Wages are assigned by occupation, sex, and age. As before, triennial pay progression is assigned using approximated work experience until the relevant maximum salary. All teachers are treated as Second Grade Teachers due to missing class/qualification data. Head Teachers are assigned First (higher) Grade wages following Dale (1904).	National (1).
Police Forces	Royal Irish Constabulary and Dublin Metropolitan Police wages assigned by occupation and age. Annual pay progression is assigned using approximated work experience $age_i - 19$ to account for recruitment age until the relevant maximum salary. Any individual that does not specify their rank is assigned a Constable salary. Finally, female police wages are re-coded to zero as these are errors (females banned).	National (1).
Postal Workers	Wages are assigned by age and occupation. Annual pay progression assigned using an arbitrary measure of work experience $age_i - 18$ until the relevant maximum salary.	National (1).

Continued on next page

Statistic	**Description**	**Geography** (n)
Clerks	Clerks referencing a Government Department in their Census return are classified as public sector and assigned 2nd Class Clerk salaries (Local Government Board). Assistant clerks are assigned their respective Local Government Board wages. Work experience and annual pay progression is calculated as before. Other clerks are assigned private-sector wages with a 30 per cent discount to reflect the public–private wage gap.	National (1).
Other Skilled Workers	Remaining Skilled Workers are assigned the average wage of all known skilled workers by age.	National (1).
Other White Collar	Remaining White Collar workers are assigned the average wage of all known White Collar workers by age.	National (1).
Retirees and Unemployed	White Collar retirees are assigned 58 per cent of their equivalent working income in line with retirement schemes of teachers and police officers. All other retirees, as well as those who describe themselves as "out of work" or "unemployed", are assigned zero income	National (1).
Other Female Workers	Wages for female workers who do not have a sex-specific wage are discounted arbitrarily by 16 per cent in line with the institutionalised gender pay gap in teaching.	National (1).
Literacy	Wages for individuals who are "illiterate" are arbitrarily discounted by 20 per cent. Wages for individuals who can "read only" is discounted by 10 per cent.	National (1).