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FROM PENSIONS TO PUPILS? SCHOOLING, RESOURCE CONSTRAINTS AND OLD AGE PENSIONS IN IRELAND 1901-11

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Working Paper 24-09

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

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ABSTRACT

A large literature argues that resource constraints inhibit human capital accumulation. We test this hypothesis using the introduction of the Old Age Pension in Ireland in 1908, evaluating its spillover on school enrolments within multigenerational households. Exploiting the OAP's age-based and means-test criteria, we identify the causal effect of the cash transfer on enrolments for children aged 14 to 16 using data from the 1901 and 1911 Censuses of Ireland. The OAP increased the school enrolments of the poorest children by 8 per cent, while no effect is detected for wealthier households. This suggests that when poverty constrains schooling, unconditional cash transfers amplify a household's demand for education by reducing the opportunity costs of schooling.

KEY WORDS: schooling, poverty, old age pension, cash transfer, human capital, economic history of Ireland.

JEL CODES: D31, H55, I25, J18, J24, N33.

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1. INTRODUCTION

A large literature argues that resource constraints inhibit human capital accumulation if poor households cannot afford the direct or indirect costs of school. Policies which transfer cash to poor households aim to address this problem both by inducing an income effect and a substitution effect, stimulating demand and reducing the opportunity costs of attending school (Hızıroğlu Aygün et al. 2022). These cash transfer programmes have become increasingly popular in the developing world as a cornerstone of public strategies to reduce poverty in the short-run, and break its intergenerational transmission (Premand and Barry 2022; Bastagli et al. 2016; Baird et al. 2014).

Transfers which do not enforce any schooling conditions either as part of eligibility or payment stages are rare as policy makers aim maximise the amount of the cash transfer invested in schooling (Evans and Popova 2014; Baird et al. 2014). Consequently, few of these Unconditional Cash Transfer (UCT) programmes are implemented at national level, while the literature is dominated by evaluations of small experimental or pilot programmes. Many of these programmes target specific households through restrictive eligibility criteria as researchers seek to evaluate the direct effects of UCTs on schooling. As such, the spillovers of larger cash transfers on schooling are infrequently studied, especially ones which do not target children or young people (Haushofer and Shapiro 2016; Baird, McIntosh, and Özler 2019; Benhassine et al. 2015). Moreover, for a UCT to improve schooling, theory implies that a resource constraint must exist which prevents households from actualising their demand from schooling, yet few UCT studies adequately explore the nature of this constraint.

We bridge this literature gap by evaluating the effect of the 1908 Old Age Pension (OAP) on school enrolments in Ireland as an exemplar of a large-scale intervention within a low-income setting, where significant demand constraints on schooling exist. The 1908 OAP was implemented concurrently across the UK and revolutionised social welfare provision in Ireland at a time when no alternative social policy existed to improve conditions for the elderly, other than the Poor Law (Ó Gráda 2002; Budd and Guinnane 1991). Even though the OAP was not motivated or designed to ameliorate Irish poverty, it was transformative for the poorest in Ireland. Since wages and output per capita were lower in Ireland, the relative value of an OAP payment was larger compared to Great Britain (Geary and Stark 2015), while Irish administrative limitations enabled the number of claimants to rapidly increase (Ó Gráda, 2002). Within two years of implementation, OAP payments accounted for 21 per cent of Irish public expenditure (Dunraven 1912), and was financed entirely by the UK Treasury. While the

Government introduced income tax reforms to fund the policy, the tax burden was almost exclusively carried by the UK's top 1 per cent of earners (Giesecke and Jäger 2021) and had little impact in Ireland. As such, the OAP represents an ideal natural experiment and a substantial wealth transfer across the Irish Sea.

We utilise full-count census returns from 1901 and 1911 to measure the spillover impact of the 1908 OAP on schooling within multigenerational households. We use 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. Census data enables us to determine whether a child is enrolled in school, working or is engaged in domestic activities, while we make our own unique data contribution by synthesising census data with new, high-resolution income estimates. Together, census and income data allow us to calculate the average income boost of the OAP and explore its impact on schooling in the poorest, most resource constrained households.

Our identification strategy exploits the OAP's eligibility criteria; its age-based and means-test threshold, to assign treatment to multigenerational households, while we isolate the causal effect of the policy using a linear probability model in a Difference-in-Difference (DiD) framework. The quality of our census data is advantageous and allows us to control for a wide range of probable demographic confounders, as well as including District Electoral Division

¹ (DED) fixed effects. The main threat to identification is OAP driven changes in household composition, either by incentivising the formation of new multigenerational households or by individuals misreporting their age to obtain an OAP. We address these sources of endogeneity in four ways; firstly, we show that our results are fully robust to a range of matching techniques which purge incomparable households from our sample. Secondly, we drop all children residing in areas with a high likelihood of age misreporting and show that our results remain unchanged. Thirdly, we demonstrate that our OAP treatment is a strong predictor of actual OAP claims and is uncorrelated with age misreporting intensity. Finally, using a placebo test we present evidence consistent with our DiD identification assumption and show that placebo treatment did not cause enrolments to evolve differently.

Our headline finding is that the OAP led to a substantial reduction in resource constraints and increased schooling enrolments in treated households. On average, Irish household incomes were boosted by 4.7 per cent, comparable to modern 'Big-Push' anti-

¹ This is a highly granular geography. In 1911, there were 3,372 District Electoral Divisions in Ireland with a median population of 535 people.

poverty initiatives², while incomes were boosted by 27 per cent for the poorest. We estimate that the Average Treatment Effect (ATE) of the OAP increased school enrolments by 4 per cent. This was driven by children in the most resource constrained households where enrolments increased by 8 per cent, while enrolments of children in the richest households were unaffected. Finally, we find evidence that the positive effect of the policy on enrolments was five-times weaker for female children than for male children, consistent with the lower number of economic opportunities available to females at the time.

Our contribution is twofold; firstly, we contribute to the ongoing discussion of the impact of UCTs on schooling in developing economies. Of the OAP specific literature, findings are mixed, context specific and hampered by limited survey data. Furthermore, we are aware of no OAP studies which directly account for any relevant means-test criteria. More generally, other UCT studies are limited by the scale of the programme, especially those where eligibility is determined by a set of geographical and/or household characteristics. We address these deficiencies by evaluating the impact of a country-wide and highly generous intervention using full-count census data and new approximations of income. Secondly, we explore the case where resource constraints have diminished the demand for schooling. This contrasts with most schooling literature which stresses the importance of supply side constraints³.

We proceed as follows. Section 2 reviews the related literature; Section 3 provides the historical context of the OAP and schooling in Ireland; Section 4 describes our data; Section 5 introduces our DiD framework, schooling model and discusses our identification assumption. It continues with a description of our general results, before analysing socio-economic heterogeneity by income quintile and sex. Section 6 discusses our robustness and placebo test, and Section 7 concludes.

2. THEORY AND RELATED LITERATURE

The relationship between income and schooling is a well-established topic of economic interest. Since a household's human capital investment responds rationally to the costs and benefits of schooling (Becker 1992; Becker and Chiswick 1966), then higher income not only incentivises investment in education, but is also the means to do so. Underinvestment in schooling can emerge in poor societies when high schooling costs are compounded by credit

 $^{^{2}}$ See Baird (2014) study of 35 UCT and CCT programmes where the cash transfers represented 5.6 per cent of household income on average.

³ A notable exception to this is Cinnirella and Hornung (2016) who show that serfdom reduced the demand for schooling in 19th century Prussia.

market imperfections, as households are unable to acquire sufficient resources to choose the level of schooling that optimises their future socio-economic outcomes (Galor and Zeira 1993). Provided that supply factors do not constrain the provision of schooling, then cash transfers mitigate the opportunity and direct costs of attending school and allow households to actualise their demand for schooling (Hızıroğlu Aygün et al. 2022; Haushofer and Shapiro 2016; García and Saavedra 2023).

This paper contributes to the debate on the impact of UCTs on schooling. UCTs are cash transfers which do not condition payments on certain household behaviours and include policies such as OAPs and child support grants. UCTs are usually targeted, with eligibility determined by a set of criteria such as age, means-test or geographical location (Baird et al. 2014)). Proponents of UCTs argue that since the lack of money is the main constraint faced by poor people, a UCT can be an effective anti-poverty tool by allowing households to invest according to their own needs (Baird et al. 2014; Haushofer and Shapiro 2016). Yet studies which measure the effect of UCTs on schooling are rare, limited by policy design, and hampered by sample size, programme attrition and selection issues (Baird et al. 2014). Furthermore, few studies measure the spillovers of UTC on schooling since policies target households containing school age children or nudge claimants towards investing in schooling by labelling the transfer as an educational support payment (Benhassine et al. 2015).

The spillover effect of UTCs on schooling is ambiguous since there is no guarantee that households will invest the transfer in schooling (Evans and Popova 2014). Empirical studies confirm this ambiguity, and report mixed and context specific findings. For example Handa et al. (2018) finds that of two Zambian UCTs, only one led to an increase in schooling, while Haushofer and Shapiro (2016) show that in rural Kenya a UCT substantially increased consumption, capital investment and consumer durable expenditure, but did not lead to any increased investment in education. Literature that measures the spillovers of OAPs on schooling also reports mixed findings. In South Africa, Edmonds (2006) explores the impact of an OAP on schooling in poor, black-headed rural households. He finds that the policy increased enrolments to nearly 100 per cent for male children, while average daily child labour fell from 3 to 1 hour a day. Conversely, Filho (2012) measuring the effect of OAP reform in Brazil found that it had no significant impact on rural male enrolments and detects only a

modest positive effect for rural females.⁴ While these inconsistent results lead Baird et al. (2014) to conclude that OAPs have no significant spillovers on schooling, they qualify their findings against the fact that OAP spillovers are also infrequently studied.

The ambiguous effect of OAPs and other UTCs on schooling arise from two sources. The first of these are traditional sources of market failure, where households may lack information on the returns to schooling or discount such returns too heavily, while social inequalities may influence intrahousehold bargaining processes reducing investment in certain children⁵ (Baird et al. 2014; García and Saavedra 2023). Conditional Cash Transfer (CCT) programmes usually address these market failure problems by requiring households to send children to school for a minimum number of days, monitoring compliance and penalising non-compliance. Unsurprisingly, CCT policies to improve schooling are widespread in developing economies while many studies confirm their effectiveness and superiority to UCT programmes. Comparing UCT and CCT arms of a randomised experiment in Malawi, Baird, McIntosh, and Özler (2010) finds that while UCTs do improve schooling, they are less than half as effective as CCTs. This finding has been reiterated by Baird et al. (2014) and García and Saavedra (2023).

Yet there have been cases when UCTs are just as effective at increasing schooling as CCTs (Akresh, de Walque, and Kazianga 2013), while there is a substantial amount of treatment effect variation across CCT studies that cannot be explained by differences in programme design (García and Saavedra 2023). We suggest that a second source of ambiguity may arise from the omission of schooling context, specifically the failure of OAP and some UCT studies to establish the nature of constraints on schooling. Rather than assume the existence of a demand constraint on schooling, we contribute to this literature by measuring the positive spillover of a large-scale UCT within a low-income setting, where there is strong evidence of significant demand constraints on schooling.

Compared to CCT research, there are few rigorous evaluations of large-scale UCTs. Consequently, the mechanism of how UCTs may improve schooling outcomes is poorly understood. Assuming there are no supply constraints on schooling, UCTs are thought to be

⁴ Due to data limitations, the Filho (2012) does not seem to disentangle the OAP reform from the wider social security programmes such as disability allowances and length of service benefits. It is also concerning that the author does not address pre-trends in schooling, which was already trending upwards in Brazil before the reform. We also have a wider concern of the comparability of multigenerational households overtime since neither Filho (2012) or Edmonds (2006) explore whether these highly generous policies affected household formation- although both do show that household composition did not change as a result of the policy.

⁵ For example, social and cultural norms may cause households to discount the value of educating female children compared to males, particularly if females have fewer lifetime socio-economic opportunities.

less effective at improving schooling since they induce a general income effect only, whereas the conditionality of CCTs also compels households to substitute child labour or leisure time for schooling (Baird et al. 2014; Kilburn et al. 2017; Benhassine et al. 2015). The validity of this mechanism assumes that households are already able to optimise their choice of schooling. However, if resource constraints prevent households from actualising their schooling demand, then conditionalities are superfluous as both UCTs and CCTs will induce an income *and* substitution effect by reducing the opportunity and direct costs of attending school.⁶ There is strong evidence this matters, for example Kilburn et al. (2017) show that a UCT in Malawi increased schooling by reducing the direct cost of enrolment. Similarly, H12Iroğlu Aygün et al. (2022) show that following a substantial UCT the share of impoverished refugee children enrolled in school increased to over 86 per cent, a large increase of 22 percentage points.

To conclude this section, we note that OAP spillovers have also been studied in the context of improving health and nutrition outcomes within multigenerational households (Duflo 2003; Case 2001), while the direct effect of *historical* OAPs on elderly living conditions have been studied exploiting the 1935 Old Age Assistance programme in the USA (Galofré-Vilà, McKee, and Stuckler 2022), the 1913 Swedish compulsory public pension(Andersson and Eriksson 2015), and the 1891 German OAP (Grogan and Summerfield 2019). We are aware of only one other paper which explores the UK's 1908 OAP. Giesecke and Jäger (2021) explores the direct effect of the OAP on labour supply in England and Wales using full-count census data. Our paper differs in that we both improve the identification of the OAP in an Irish context by accounting for the means-test, while we focus on the policy's spillover on schooling within multigenerational households, and not its direct effects on the elderly recipient.

3. HISTORICAL CONTEXT

3.1 The Old Age Pension Act (1908)

The OAP Act (1908) introduced a means-tested, non-contributory pension for all over 70-yearolds in the UK. It was a central pillar in 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). The OAP entered the statute books in August 1908, and within a month, the Local Government Board of Ireland (LGB) established 54 Pension Committees and 394 local sub-committees to begin processing applications (LGB 1909). Eligible individuals applied in person by

⁶ If the optimal level of schooling was below some 'socially optimal' level, we note that conditionalities may raise schooling above that which is privately optimal for the household.

completing a generic application form in their local post office. Given the OAP was financed centrally through the Treasury, the application was assessed by a Treasury Pension Officer in line with four eligibility criteria; sufficient age, means-test, residency status and character⁷ (Casson 1908). The Pension officer would then recommend that the relevant Pension Committee accept or reject the claim. A decision could be appealed by the applicant or by the Pension Officer, with the LGB overseeing and enforcing the appeals process. Should a claim be rejected, the same individual could not submit a new claim for four months.

Claims were means-tested and awarded on a sliding scale between 1 and 5 shillings a week, or around £3- £13.50 a year. Annually, an OAP paid at the highest rate corresponds to 54 per cent of the average annual wage in the UK, and around 67 per cent of average wages in Ireland. The highest rate was awarded to the poorest elderly on incomes less than £17 per year, while those on incomes over £31.50 were deemed ineligible. As such, the OAP would have *at least* boosted income for the poorest by 80 per cent. In total, 94.7 per cent of Irish OAP claimants received the full 5 shillings between 1909 and 1912, modestly higher than the share of 5-shilling claims in England (Old Age Pensions Committee 1919; HMT 1913).

The first OAPs were paid on the 1st of January 1909, and policy uptake across the UK was high. In 1903, a Select Committee estimated that total OAP eligibility across the UK would be around 387,000 persons by 1907 (Casson 1908), yet by 1909 there were already 647,500 claims payable. While policy uptake was high across the UK, it was extreme in Ireland. Within three months, the total number of claims and appeals already exceeded the number of over 70s in the 1901 Census⁸ (LGB 1909) and on the eve of the 1911 Census, 201,783 OAP claims were payable in Ireland. We calculate that 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 claims despite accounting for around 14 per cent of the total number of over 70s in the UK (Ó Gráda 2002).

Exorbitant Irish claims can be explained by several factors, including the Great Famine and high levels of out-migration in the latter half of the 19th century leading to the premature aging of the Irish population (Colvin, Henderson, and McLaughlin 2024). The most compelling factor driving high uptake in Ireland was the lack of information available to the LGB and Pension Officers to assess claims against. Under the 1908 Act, an individual must be at least

⁷ Character refers to receipt of Poor Relief, recent work history, criminality, history of addiction (drunkenness) or if the applicant has been or is currently sectioned under the 1890 Lunacy Act (Ireland).

⁸ 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.

70 years of age and earn less than £31.50 per year. While this criterion was more easily applied in England and Wales, it was not an easy task in Ireland where birth registration only became legally enforceable in 1864, nearly 30 years after it was introduced in England and Wales. Consequently, Irish officials could not use birth registration for age verification, and instead relied on household returns from the 1841 and 1851 census. Yet, recognising issues of data quality, Irish officials also accepted baptismal certificates, marriage certificates or even baptismal certificates of children dated before 1860 as proof of age⁹ (LGB 1909). Of the 43,475 appeals addressed by March 1911, 54 per cent regarded the age of the applicant.

A second factor driving a high number of OAP awards in Ireland was the LGB's reluctance to rigorously apply the means test. Applicants self-reported their financial means in their application, including all labour income, remittances, savings, investments (actual and potential) and spousal income (Casson 1908). To disincentivise applicants from disposing of their assets before application, recent asset transfers were also included in the means-test. This created a difficulty for the LGB given the long-standing Irish custom of farmers transferring land to their eldest son or daughter once married, or upon becoming too infirm to work the land. Seeing no legitimate reason to disrupt this practice, the LGB decided not to rigorously apply the means assessment, deciding issues of land transfers on a case-by-case basis (Ó Gráda 2002; LGB 1909). By March 1911, 29 per cent of appeals addressed by the LGB regarded the financial means of the applicant.

Together, these problems led to widespread age misreporting and welfare fraud in Ireland compared to Britain (Ó Gráda 2002). The LGB directed inspectors to use their own discretion when assessing claims and by 1912, acknowledged that they had taken a more lenient approach than provided for in the legislation, rejecting claims only in obvious cases of fraud, and giving 'borderline' applications the benefit of the doubt (LGB 1912). While this OAP quirk in Ireland requires careful consideration in our identification strategy, we note that the generous approval of claims in Ireland improves the likelihood that we accurately predict which individuals received the OAP and reduces issues associated with assigning treatment erroneously.

⁹ While both censuses were destroyed during the Irish Civil War (1922-1923), we know that Ireland was prone to age heaping throughout the 19th Century (Budd and Guinnane 1991) reducing the likelihood of identifying the OAP applicant. 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).

In contrast to Giesecke and Jäger (2021), we note that Poor Law substitution effects may influence our results. While those who had received Poor Relief¹⁰ from 1908 were initially disqualified from an OAP award, this disqualification criteria was abolished in January 1911, three months before the 1911 Census was taken. Since we are interested in measuring the spillover effects of OAPs on schooling within treated households, Poor Law substitution effects may have changed multigenerational household formation. This bias stems from two avenues, firstly the OAP may have replaced Poor Relief for the elderly who otherwise would have had no option but to leave their household and enter a workhouse. Secondly, in the three-month period between the abolition of the Poor Law disqualification criteria and the 1911 Census, elderly individuals in anticipation of receiving an OAP may have left the workhouse and formed new multigenerational households. We will address these sources of endogeneity and show that our results are robust. Finally, we note that there were no substantial changes to the income tax rate in Ireland to fund the policy as most of the Irish population was too poor to qualify (Giesecke and Jäger 2021; Ó Gráda 2002).¹¹ Alternative pensions did exist in Ireland but like in Britain, were concentrated at the upper end of the income spectrum and limited to public sector workers and some private sector white-collar workers. We directly account for these other pension schemes in our income estimates.

3.2 The National School System 1831-1911

Ireland was the first country to receive UK-funded primary education under the National School (NS) system in 1831 (Blum et al. 2017). While its initial expansion was hampered by opposition, it aimed to provide a centralised and secular system of education and address high rates of illiteracy and innumeracy (CONEI 1902; Ó Gráda 2010; Walsh 2016). By the second half of the 19th century, the NS system expanded rapidly, accelerated by several supply side reforms which improved teacher renumeration, provide teaching pensions and reduce the financial burden of establishing new teaching training colleges and schools (CONEI 1901). Consequently, between 1873 and 1901, the number of Teachers increased by over 70 per cent to 11,800 while the number of operational schools increased by 21 per cent to 8,700. Such was

¹⁰ Either by entering a workhouse (indoor relief) or by applying for a small monetary or in-kind payment (outdoor relief). Unlike the OAP, Poor Relief was financed locally through a local land levy in each of Ireland's 159 Poor Law Unions (as of 1911). Consequently, the provision of relief was highly constrained by local economic conditions (Crossman 2006; Laragy 2011).

¹¹ We calculate average Irish Incomes to be around £39 per annum while the income tax threshold was £160. In total, there were only around 1 million income taxpayers in the entire UK during this period, around 2 per cent of the total population.

the supply of schooling that by 1900, there was *at least* one NS operating in every townland or village in Ireland (Walsh 2016).

The rapid expansion of Irish schooling occurred during a period substantial depopulation in Ireland driven by mass emigration and urbanisation, leading to an oversupply of schools. Dale (1904), the Chief Inspector of schools in the UK, observed that there was an adequate supply of schools in Ireland as early as 1867. Yet, between 1881 and 1901 as the population of children under fifteen fell by 26 per cent, the number of operational schools grew by 50 each year on average.¹² By 1895 there were 17,950 more student places available than there were on the actual attendance roles (CONEI 1897).¹³ By 1903, Commissioners began to close or amalgamate small, peripheral schools without any negative effect on school provision (CONEI 1905). Despite closing 915 schools by 1911, the number of unfilled student places grew by a factor of 13 to over a quarter of a million¹⁴ (CONEI 1912)..

The perennial problem for Commissioners was the lack of demand for schooling, expressed through low daily attendance and children leaving school prematurely. Ireland was identified as an outlier for school attendance as early as 1880, as attendance rates fell behind that of Great Britain (Fahey 1992). By our period of interest, over a quarter of students did not attend school regularly. This, alongside excess student places imply that on an average day, classrooms were only half-full. Commissioners attributed low demand to a number of factors, including disinterested parents, poor weather and the seasonality of farm work, particularly in western, poorer areas (Dale 1904; CONEI 1913). While the first two of these factors are interrelated examples of market failure, the fact that many that children could not be spared from work during certain parts of the year implies a resource constraint on the demand for schooling.¹⁵

Children leaving school prematurely is symptomatic of this resource constraint and indicates high opportunity costs of schooling. NS inspectors noted that the general tendency of older children to leave school and join the labour market as soon as an opportunity arises, had led to older children being completely absent from classrooms in some parts of Ireland (CONEI 1913). As of 1900, the NS curriculum grouped children by age into six academic "Standards"

¹² This figure is derived from the Census' of Ireland 1891 and 1901.

¹³ 815,248 students were enrolled on average throughout the year. We emulate CONEI's estimates of school capacity and allow 8 square feet of classroom for each student. We calculate that had the capacity to accommodate 855,889 children in 1895.

¹⁴ The Commissioners also opened 485 new schools over the same period.

¹⁵ Dale (1904) remarked that seasonal attendance in Ireland was exceptional and did not occur in England. He noted that it was particularly bad in poor western areas in summer when children's parents often migrated to England as seasonal workers.

and gives some indication of this problem. Each Standard represented roughly a year of schoolwork and was designed to be completed by the age of fourteen (CONEI 1901). Assuming that children progressed as intended, of the 332,535 children enrolled in the First Standard in 1901, only 14 per cent reached the end of the curriculum by 1906.¹⁶ From 1901-11 only 29 per cent of students were enrolled in the top three Standards on average, while only 6 per cent of students were enrolled in the last Standard (CONEI 1912).

Finally, we note that during our period of interest there were no demand-side schooling reforms introduced in Ireland. The closest demand reform to our period of interest was the 1892 Irish Education Act which abolished all school fees and compelled all children enrolled in school between the ages of six and fourteen to attend. Although the effectiveness of compulsory education in Ireland has been rebuffed by historians (Fahey 1992) we note that in the year following this reform the number of children enrolled in school grew modestly by 2 per cent while attendance grew by 3 per cent. This, alongside poor general attendance, and the oversupply of schools suggests that demand issues were constraining Irish schooling, particularly high opportunity costs for older children.

4. DATA

Our primary data source is the 1901 and 1911 Censuses of Ireland. These censuses were conducted exactly ten years apart and collected similar economic and demographic information about the entire Irish population. A week before the census, Enumerators visited every household in Ireland, recorded the name of the household head and provided a standardised household form which enquired the occupants' address, names, ages, sex, inter-household relationships, religion, birthplace, occupation, literacy, marital status, language (Irish and or English), and disability status.¹⁷ After a week, census forms were collected and forwarded to the Registrar General for tabulation. If households were unable to compete the form, Enumerators enquired the information in person and filled in the census on the household's behalf. Households which refused to participate in the Census and Enumerators who wilfully

¹⁶ Infants may have been counted in the First Standard in NS Commissioner reports. In such a case these infants would not have been eligible to progress to the Second Standard and exaggerate our approximation. Using the Second Standard as a baseline instead, we observe that half of all children enrolled in 1901 did not progress to the final Standard by 1905.

¹⁷ The only difference between the two census schedules was in 1911 where women were asked the number of years married, number of children born alive, and the number of children still alive. We do not consider any of these variables in our analysis. In Ireland, there were more than 4,000 enumerators, all of which were drawn from the ranks of the Royal Irish Constabulary and Dublin Metropolitan Police (The National Archives of Ireland 2013).

shirked their duties could be convicted and fined £5 (Census Ireland Act 1910). This fine was substantial, and we estimate it was worth around 12 per cent of average earnings.¹⁸

We consider household returns only, dropping returns for institutions such as prisons, hospitals and barracks, as well as religious communities and small businesses with workers in residence. Due to data limitations, we drop households residing in tenements¹⁹ as well as households containing transient relationships such as boarders, visitors, and servants. In total, our final census dataset accounts for 6,203,249 individuals and 1,390,280 households. Since we are interested in measuring the spillover effect of the OAP on schooling, we isolate multigenerational households only. We detect these households by exploiting the intrahousehold relationships with respect to the household head and define a multigenerational household as those containing at least one child under the age of 16 and at least one "other" relative over the age of 50. "Other" relatives include the parents, aunts, uncles, cousins and inlaws of the household head, while we choose 50 as the age threshold to limit the impact of age misreporting and ensure our definition of multigenerational household is comparable overtime. Data limitations prevent us from linking multigenerational households between censuses.²⁰

As income is not recorded in the census, we reconstruct this information using a combination of occupational information and other individual characteristics. Equation (1) summarises the weekly wage income of an individual as:

$$w_i = f(occupation, age, literacy, sex)$$
(1)

We standardise occupations via the Historical International Standard of Classification of Occupations (HISCO), and group occupations into five categories: Farm Labourers, Urban Labours, Farmers, Skilled Workers, and White-Collar Workers. Incomes are assigned within these groups with income data drawn from a wide range of contemporary data sources across seventy-three unique occupations. For simplicity, occupations which we cannot directly match are assigned the average income of their respective occupational category. Incomes are calibrated using age, sex and literacy levels to account for variations in income linked to experience, the gender wage gap and human capital levels, before being aggregated into household income. There are several households without occupational data, meaning we cannot

¹⁸ Assuming an enumerator was a Royal Irish Constabulary officer with 4 years of experience, this fine represents around 9 per cent of annual earnings.

¹⁹ We cannot distinguish between households living in tenements. We detect and drop tenements by retaining households with a single household head and less than 20 members. To ensure that the remaining observations are individual households, we generate a measure of surname dispersion and filter out households with more than 10 members where more than 70 per cent of the surnames are different. This drops 2,988 households.

²⁰ Unique names were rare in Ireland at the time, while age heaping in the 1901 census, age misreporting in the 1911 and a highly mobile population reduces our ability to make accurate matches.

approximate their income. Since households must at least have sufficient income to exist, we assign a small subsistence income to all households. We approximate this by exploiting variation in Farm Labourer's incomes to proxy the cost of rent and food at county level, with this measure indirectly accounting for "in-kind" payments.²¹ A full description of our income estimates and methodology is available in the Appendix.

Our objective is to isolate the causal impact of the OAP on school enrolments. While census data provides a comprehensive account of schooling, it does not contain a complete account of OAP claims in 1911. To address this data limitation, we exploit the OAP's age and means based criteria to assign OAP treatment to eligible individuals in 1911. Treatment is assigned in GBP (£) a week, where eligible individuals are endowed with £0.25 (5-shillings) of OAP income a week. Since we assign treatment based on eligibility, we should expect to over-estimate the number of OAP claims since not all who are eligible for such programmes apply, while those recently eligible may not have had their applications approved by the 1911 census date.²² The means-test is a critical consideration as it allows us to disqualify all individuals earning more than £31.50 per annum, calibrating our measure and reducing contamination from age exaggeration in the 1911 census. Our treatment measure assigns an OAP to 218,265 individuals. Comparing this with a Parliamentary Return compiled on the eve of the 1911 census suggests that we overestimate actual claims modestly by 8 per cent.²³ Without the means-test, we would assign treatment to 282,760 individuals, overestimating the number of claims by 40 per cent.

We divide all households into comparably sized quintiles in 1901 and 1911 and calculate the average OAP income boost for each quintile. To prevent high income households from skewing the distribution, we top code household income at £3.50 a week, roughly three times larger than the median household income. Our results are reported in Table 1. We find that the OAP boosted Irish household income by 4.7 per centage points on average, comparable with modern anti-poverty cash transfers.²⁴ The OAP had a transformative impact on the poorest income quintile and led to an average 27-per centage point increase to household income. These poor households are predominantly those living at subsistence with no individuals in employment or are single income households containing a labourer or very poor farmers who

²¹ Money wages formed only a fraction of weekly wages for some labourers. These individuals were chiefly renumerated in foodstuffs and/or had their accommodation provided for.

 $^{^{22}}$ This overestimation will bias our results towards zero since we assign treatment to households which were not treated.

²³ In March 1911 there were 201,783 active claimants in Ireland.

²⁴ Baird et al. (2014) calculates that thirty-five modern cash transfers boosted average household income by 5.66 percentage points.

earn less than a pound a week. The OAP boosted incomes in the second- and third-income quintile by 7.5 and 3.8 per cent respectively, while it had a small effect on the richest households.

The relatively large income boost to most Irish households highlight Ireland's status as an income-poor country. This is confirmed by Figure 1 which maps the spatial distribution of the OAP income boost, aggregated to DED level. The western periphery was the poorest region of Ireland that was 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). At DED level, the OAP boosted incomes by over 2 per centage points on average. In the western periphery, where average household incomes were only two thirds of the average, the mean DED boost was 3 per cent.²⁵

Quintilo	Household Income	Mean OAP Boost
Quintile	(£ per week)	(Log point change)
1	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	1.98 - 3.50	0.013
ALL	ALL	0.047

Table 1: OAP income boost by household income quintile.

²⁵ We categories counties Donegal, Leitrim, Sligo, Mayo, Galway, Clare, and Kerry as the western periphery.

Figure 1: Map of the average OAP boost to household income at District Electoral Division Level in 1911.



Note: County borders are highlighted in black.

Our focus is measuring the effect of the OAP on older children in multigenerational households. Since schooling was compulsory for all children up to the age of 12, we concentrate on households where schooling is optional, containing children aged 13 to 16. As the opportunity costs of school are an increasing function of age, we assume that the opportunity costs of school for these older children are high. Table 2 presents summary statistics of these older children and their respective households. The first column of Table 2 presents summary statistics for all older children in 1901 and 1911. The top panel describes characteristics relating to the child, while the bottom panel describes characteristics of their household. Of the 526,779 children, nearly half are female, and 81 per cent were the eldest child residing in the household. Only 56 per cent of these children were enrolled in school, while a further 18 per cent were employed, implying that a quarter of all children were at home

engaged in domestic activities. Children were highly literate, with nearly all children having some ability to read and write, while only 2 per cent were illiterate. Over three quarters of children were Catholic, a quarter lived in an urban area, and 15 per cent were bilingual (Irish and English).

Just over 13 per cent of older children lived in multigenerational households. While determining the factors behind these low instances of multigenerational households is beyond the scope of this paper, it is likely a symptom of the post-Famine mass migration movement in the latter half of the 19th century (Hatton and Williamson 1994; Ó Gráda 1994). Summary statistics for multigenerational households are reported in Column 2 of Table 2 and are broadly comparable to the general population. That said, children in multigenerational households are 2 per cent less likely to be employed and 7 per centage points more likely to live in a farming household. While multigenerational households also have higher average incomes, this amounts to only 5 pence a week. As a proxy for living standards and resource constraints, we calculate a measure of Household Equivalised Income (HEI) and account for differences in household size and demography. While this measure appears to confirm that multigenerational households are poorer by 5 per cent, once we account for OAP income this gap more than halves.

Columns 3 and 4 of Table 2 explore the differences in multigenerational households between 1901 and 1911. Between the two census years, multigenerational household income fell by around £0.15/week, causing HEI to fall by over 9 per cent. Once we account for OAP labour income, HEI remains unchanged since the fall in labour income is on average equal to the increase in OAP income. This implies that households are substituting labour income for OAP income. Fortunately, most other multigenerational household characteristics remain stable overtime, with few notable differences. In 1901, enrolments for multigenerational households were close to the all-sample average. By 1911, multigenerational enrolments had increased by 4-percentage points, exceeding the all-sample average, and were accompanied by a 5-percentage point reduction in child employment. Figure 2 plots the raw enrolment rates by age cohort for children in multigenerational households.



Figure 2: Rates of enrolment in school in multigenerational households by age cohort and year.

Notibly, the enrolment rate for children under 12 is high at around 94 per cent, changing modestly between 1901 and 1911. After the age of 11, enrolments tend to decline with each additional year in line with our opportunity cost hypothesis. This is especially the case between the ages of 13 and 14, and 14 and 15 where enrolments plumment by over 20 per centage points respectively. By 1911, emrolments improve for virtually every age cohort. The largest change is for 13-year-olds, which is also first age cohort exempt from compulsory education. Enrolments for 13-year-olds increases by 8 percentage points, from 82 to 90 per cent, while enrolments also increase by 6.5 percentage points for 14-year-olds, 3.4 percentage points for 15-year-olds and just over a percentage point for 16-year-olds. This raw data suggests that compared to 1901, children in multigenerational households appear to be staying in school for longer, in line with a reduction in the opportunity costs of school.

Table 2: Pooled 1901 and 1911 Descriptive statistics

	Full Sample		Multigen	erational	19	01	1911	
	N = 52	26,345	N = 7	0,111	N = 3	4,204	N = 3	5,907
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Children aged 13-16								
Age	14.49	1.11	14.47	1.11	14.48	1.11	14.45	1.11
Eldest Child (0-16)	0.71	0.45	0.75	0.43	0.75	0.43	0.74	0.44
Sibling/peer over 16 employed	0.32	0.47	0.33	0.47	0.34	0.47	0.32	0.47
Female	0.49	0.50	0.49	0.50	0.49	0.50	0.49	0.50
Enrolled School	0.56	0.50	0.58	0.49	0.56	0.50	0.60	0.49
Employed	0.18	0.38	0.16	0.37	0.19	0.39	0.14	0.35
Illiterate	0.02	0.15	0.02	0.15	0.03	0.17	0.02	0.13
Catholic	0.76	0.43	0.77	0.42	0.77	0.42	0.77	0.42
Church of Ireland	0.11	0.32	0.10	0.31	0.11	0.31	0.10	0.30
Presbyterian	0.10	0.30	0.10	0.30	0.10	0.30	0.10	0.31
Urban	0.25	0.44	0.20	0.40	0.16	0.37	0.21	0.41
Bilingual	0.15	0.36	0.18	0.39	0.17	0.38	0.22	0.41
Household Statistics								
Household Size	7.06	2.41	7.59	2.67	7.58	2.68	7.59	2.65
Household Income (£/week)	1.33	1.01	1.36	1.10	1.43	1.10	1.29	1.09
Household Income (£/week) + subs	1.55	1.07	1.57	1.10	1.65	1.11	1.50	1.09
Equivalised Income (£/week)	0.44	0.29	0.42	0.28	0.44	0.29	0.40	0.27
OAP Income boost (£/week)	0.01	0.06	0.08	0.13	0.00	0.00	0.15	0.15
Equivalised Income (£/week) + OAP	0.45	0.28	0.44	0.28	0.44	0.29	0.44	0.26
Labourer (Household Head)	0.19	0.39	0.16	0.37	0.16	0.36	0.16	0.37
Farmer (Household Head)	0.46	0.50	0.53	0.50	0.56	0.50	0.50	0.50
Skilled Worker (Household Head)	0.22	0.41	0.17	0.37	0.18	0.38	0.16	0.36
White-Collar (Household Head)	0.07	0.26	0.06	0.23	0.05	0.23	0.05	0.23
Illiterate (Household Head)	0.18	0.39	0.24	0.43	0.25	0.43	0.23	0.42
Relatives over 70 (count)	0.12	0.35	0.61	0.61	0.53	0.59	0.68	0.61
OAP Claimants (count)	0.05	0.24	0.31	0.52	0.00	0.00	0.60	0.59

5. EMPIRICAL ANALYSIS

We choose a Difference-in-Difference (DiD) framework to compare school enrolments between older children in treated and untreated multigenerational households.²⁸ This approach is advantageous since baseline differences are not a key identification assumption, allowing us to calculate an Average Treatment Effect (ATE) for the entire treated population. We restrict our sample to multigenerational households since by definition, children in non-multigenerational households cannot be treated and are an inappropriate control group. Data limitations prevent us from creating a panel of households and following them throughout time. Instead, our identification strategy pools all multigenerational households and measures the difference in schooling outcomes between similar treated and untreated children, explicitly controlling for the post-treatment period. Our baseline DiD approach is surmised by the linear probability model:

Enrolment_{it} = $\beta_1 Pension_{it} + \beta_2 c1911_i + \beta_3 Income_{it} + X_{itk} + a_1 DED + \varepsilon_{it}$ (2) where school enrolment status is a dichotomous dummy variable taking the value of 1 if child *i* is enrolled in school at time *t*, 0 otherwise. *Pension_{it}* is a continuous variable which measures the amount of OAP income allocated to child *i*'s household in GBP (£) per week at time *t*. In this case the GBP value of a 5-shilling OAP is £0.25. *Pension_{it}* is 0 for all children in 1901 and increases by 0.25 for every eligible pensioner in the household in 1911. *c*1911_{*i*} equals 1 in 1911 during the post-treatment period, 0 otherwise and controls for unobserved characteristics correlated with the post-treatment period. $\beta_3 Income_{it}$ controls for the household income of child *i* at *t*, where income contributions from children under 16 and adults over 70 have been subtracted. This allows us to account for the confounding effect of income on schooling, while removing the mechanical link between income and the labour supply decisions of children (staying in school for longer), and the elderly (leaving the workforce to apply for an OAP).²⁹

 X_{itk} is a vector of covariates across a range of child and household characteristics which affect the probability of school enrolment. These include child age, to capture the negative correlation between age and primary schooling, literacy level, and a range of dummies which

²⁸ Other comparable OAP studies opt for a regression discontinuity research design to identify the effect of the OAP on their outcomes of interest. This approach is problematic in our case given the prevalence of age exaggeration to obtain an OAP in 1911.

²⁹ Without this adjustment, household income becomes a "collider" since it both affects enrolments through an income effect and is mechanically affected by the OAP.

indicate if the child is the eldest residing in the household, is female, is catholic and is bilingual (Irish and English).³⁰ We account for differences in household preferences for schooling by including two dummies which indicate if the child resides in a farming household, and if they reside in an urban area, as well as a controls for household size and preferences for schooling by including an illiteracy dummy which indicates if the household head is illiterate or not. To prevent age trends from biasing our estimates, we also include the age of the eldest person in the household under fifty and its square.³¹ Finally, *DED* is a set of District Electoral Division (DED) fixed effects to account for unobserved heterogeneity which might affect individuals similarly in each DED, ε_{it} is the remaining error term. Standard errors are clustered at the DED level.

In our fully specified model, β_1 can be interpreted as the spillover causal effect of the OAP on schooling, if in the absence of treatment school enrolments in multigenerational households would not have evolved differently. To build a stylistic argument that our model complies with the parallel trends assumption, we isolate multigenerational households in 1901, assign a placebo OAP and explore if this placebo led to any difference in enrolments. Our placebo model is specified:

$$Enrollment_{1901i} = \beta_1(Placebo_{1901i}) + X_{1901ik} + a_1DED + \varepsilon_{it}$$
(3)

where all variables are defined as before except $Placebo_{1901i}$ which is a continuous variable increasing by 0.25 GBP for every placebo treated pensioner in the household in 1901. We do not control for the post treatment period since all observations are taken from the 1901 census. If our placebo is uncorrelated with the error term and has no significant impact on schooling, then we have evidence consistent with the assumption of parallel trends as the placebo did not cause enrolments to evolve differently in the pre-treatment period.

6. **RESULTS**

6.1 **Baseline Schooling Results**

We now test if the OAP increased in school enrolments of 13- to 16-year-olds in multigenerational households. We estimate several variations of Equation (2) to test the sensitivity of our results to our choice of covariates and our results are reported in Table 3.

³⁰ We include bilingual status to account for cultural confounders correlated with the Gaelic Revival movement.

³¹ We should not include the ages of individuals above the age of 50 not only due to the likelihood of age exaggeration to receive the OAP, but because its interpretation is unclear since it is the criteria we have used to identify multigenerational households.

Model (1) includes our variable of interest, child characteristics and household incomes. Model (2) adds DED fixed effects, Model (3) adds the rest of household characteristics and Model (4) accounts for age trends using the age of the eldest person in the household under 50 and its square. We find that the OAP had a highly significant impact on school enrolments of older children across all four models, with little change in the size or significance of the OAP effect. This suggests that changes in schooling as shown in the raw data cannot be explained by differences in observable child characteristics, household characteristics or age trends.

Our fully specified Model (4) indicates that for every £1 of OAP income a week, children in treated households are 9.3 percentage points more likely to be enrolled in school than children in untreated households. Given that the average OAP payment corresponds to £0.25 a week, we calculate the Average Treatment Effect (ATE) of the OAP on enrolments by dividing the Pension coefficient by four. As such, the OAP had an ATE on schooling by 2.3 percentage points and increased actual enrolments by just over 4 per cent. In line with expectations, our coefficient for household income is positive and highly significant across all specifications. Even though average household incomes are low at only £1.40 per week, the effect of an additional £0.25 of labour income is modest, increasing the likelihood of enrolment by a percentage point. Given that the OAP boosted the incomes of the poorest the most, the larger effect of OAP income on schooling than an equivalent labour income boost indicates the presences of heterogeneity across the income spectrum.

		Dependent variable:					
		In Sc	chool				
	(1)	(2)	(3)	(4)			
Pension	0.113***	0.092***	0.099***	0.093***			
	(0.018)	(0.017)	(0.017)	(0.017)			
1911 Census	0.025***	0.039***	0.037***	0.038***			
	(0.006)	(0.005)	(0.005)	(0.005)			
Household Income	0.021***	0.035***	0.034***	0.034***			
	(0.007)	(0.004)	(0.004)	(0.004)			
Age 14	-0.194***	-0.192***	-0.192***	-0.192***			
	(0.012)	(0.013)	(0.013)	(0.013)			
Age: 15	-0.419***	-0.420***	-0.420***	-0.419***			
	(0.013)	(0.013)	(0.013)	(0.013)			
Age:16	-0.608***	-0.607***	-0.606***	-0.605***			
	(0.008)	(0.008)	(0.008)	(0.008)			
Sex: Female	0.011**	0.013***	0.013***	0.013***			
	(0.005)	(0.005)	(0.005)	(0.005)			
Illiterate	-0.349***	-0.312***	-0.303***	-0.302***			
	(0.015)	(0.015)	(0.016)	(0.016)			
Bilingual	0.049^{***}	0.062^{***}	0.061***	0.061^{***}			
	(0.007)	(0.007)	(0.007)	(0.007)			
Catholic	0.109^{***}	-0.035***	-0.029***	-0.029***			
	(0.020)	(0.008)	(0.008)	(0.008)			
Eldest Child	-0.010**	-0.005	-0.006	-0.007			
	(0.004)	(0.004)	(0.004)	(0.004)			
DED Fixed Effects	Х	\checkmark	\checkmark	\checkmark			
Household Characteristics	Х	Х	\checkmark	\checkmark			
Age Trends	Х	Х	Х	\checkmark			
1901 Enrolments (%)	57.3	57.3	57.3	57.3			
Observations	67,472	67,472	67,472	67,472			
\mathbb{R}^2	0.243	0.329	0.330	0.330			
Adjusted R ²	0.243	0.294	0.295	0.295			

Table 3: Baseline regression results- the effect of the OAP on schooling.

Note: Robust standard errors clustered at DED. Significance levels: ***p<0.01, **p<0.05, *p<0.1. This is a linear probability model with "In School" taking the value of 1 if a child is enrolled in school and 0 otherwise. The Pension is calculated in GBP (£) per week, with an average weekly pension claim of £0.25. We take the natural log of household income to account for its skewed distribution. Household income is adjusted for child and over 70 labour income to prevent reverse causality problems. Literacy rank is a categorical variable where 1 indicates illiteracy, 2 indicates ability to read only, and 3 indicates the ability to read and write.

6.2 **Resource Constraints**

We hypothesise household resource constraints reduced the demand for schooling in Ireland amongst the poorest. So far, we have shown so far that the OAP led to a substantial 27-percentage point boost to household incomes amongst the most resource constrained households while on average, the OAP increased school enrolments by 4 per cent. If resource constraints reduced demand for schooling, we would expect the OAP to positively affect enrolments at the lower end of the income spectrum. Given that household income tends to increase with household size, we compute a measure of Household Equivalised Income (HEI) to explore the question of resource constraints. While numerous weighting strategies exist, we choose the OECD Modified Scale for its simplicity, allowing our income measure to account for differences in household size and demography.³²

Our results are presented in Table 4, and we find strong evidence that enrolments increased the most in the poorest treated households. Model (1) reports our baseline model where we additional amend household income to account for labour income from siblings over the age of sixteen.³³ Models (2) to (6) estimate identical specifications, subsampled by HEI quintile. For the poorest, the OAP increased the likelihood of an older child being enrolled in school by over 4-percentage points, corresponding to an actual increase in enrolments of nearly 8 per cent by 1911. This result is in line with our hypothesis that prior to the OAP, resource constrained households diminished the demand for schooling. For the poorest, the coefficient of household income is positive but statistically insignificant, likely due to a lack of variation in the income for the poorest.³⁴ Models (4) and (5) describes the results for households in the third HEI quantile and indicates a negative correlation between the OAP and enrolments.

 $^{^{32}}$ We assign the first adult in the household a weight of 1, each subsequent adult receives a weight of 0.5, and children under the age of 14 are assigned a weight of 0.3.

³³ This prevents bias in our household income coefficient since it will capture the indirect effect of having an employed older sibling who is not a child on the schooling rates of children in the household.

 $^{^{34}}$ The poorest have an average income of just £0.51 a week, while the minimum is £0.17, and maximum is £1.25 a week.

			Depende	nt variable:		
			Enrolled	l In School		
	(1)	(2)	(3)	(4)	(5)	(6)
Pension	0.090^{***}	0.163***	0.048	-0.026	-0.048	0.067
	(0.017)	(0.042)	(0.040)	(0.039)	(0.043)	(0.045)
1911 Census	0.039***	0.079^{***}	0.045^{***}	0.022^{**}	0.015	0.029^{***}
	(0.005)	(0.014)	(0.012)	(0.011)	(0.009)	(0.010)
Household Income	0.033***	0.012	0.045^{***}	0.073^{***}	0.105^{***}	0.091***
	(0.004)	(0.016)	(0.013)	(0.010)	(0.009)	(0.006)
Age 14	-0.191***	-0.148***	-0.144***	-0.184***	-0.220****	-0.300***
	(0.013)	(0.009)	(0.011)	(0.015)	(0.019)	(0.020)
Age 15	-0.417***	-0.367***	-0.352***	-0.391***	-0.470***	-0.514***
	(0.013)	(0.013)	(0.013)	(0.020)	(0.017)	(0.013)
Age 16	-0.603***	-0.554***	-0.552***	-0.598***	-0.624***	-0.650***
	(0.008)	(0.014)	(0.013)	(0.015)	(0.012)	(0.014)
HHEI Quantile	All	1- Poorest	2	3	4	5- Richest
1901 Enrolments (%)	55.5	60.7	60.4	53.9	53.9	42.5
Household/Child Characteristics	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
Age Trends	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
DED FE	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
Observations	67,472	14,431	14,063	13,386	13,119	12,473
R2	0.330	0.394	0.428	0.439	0.463	0.454
Adjusted R2	0.295	0.296	0.289	0.291	0.336	0.344

Table 4: Regression results by HEI Quintile

Note: Robust standard errors clustered at the household. Significance levels: ***p<0.01, **p<0.05, *p<0.1. All variables specified as before except household income where we also subtract labour income from siblings over the age of 16 to account for peer effects. Model (1) is our baseline model, while Model (2) – (6) estimates an identical specification by equivalised income quintile. Sample sizes vary slightly due to data attrition, especially when we account for the literacy status of the household head. It may seem peculiar that 1901 enrolments fall further up the HEI spectrum, yet household income effect, where children who have siblings over the age of 16 working. If we do not discount, then the size of the positive coefficient increases by around two thirds. The remaining income effect that we have captured above may relate to rate cases where children in wealthier households enrol in secondary school.

6.3 Did the OAP reduce the opportunity costs of school?

If resource constraints diminished the demand for schooling, then it follows that the opportunity costs of attending school are too high for the poor to actualise their demand. We can indirectly explore if the OAP reduced the opportunity costs of attending school by evaluating how the policy affected schooling by age and sex. If the OAP affects children on the margins of attending school i.e. "marginal children", then the policy likely reduced the opportunity costs of school increase with age

and available economic opportunities, we assume that these marginal children are older males. We sub-sample our dataset by child age cohort and generate a new interaction term to explore if the OAP affected female children differently than male children. Our results are reported in Table 5, where Model (1) describes our baseline results, Models (2) - (5) explores the effect of the OAP on each age cohort, while Model (6) includes our interaction term in our full specification.

We find that the OAP improves the likelihood of enrolment for these marginal children. We detect no significant effect of the OAP on enrolments for 13-year-olds as the cohort which is most likely to be enrolled in school. Comparatively, the OAP increases the likelihood of enrolment for treated 14, 15 and 16-year-olds by a similar magnitude of around 2.5 to 3 percentage points. Since enrolments are a decreasing function of age, this corresponds to an actual increase in enrolments of 4.5, 5.5 and 11.2 per cent respectively, in line with our expectations. As a validation check, we sample both censuses for all children aged 5 to 16 to explore if the OAP increased the likelihood of younger children being enrolled in school. Consistent with our opportunity cost hypothesis, we find no significant effect of OAP on enrolments for this younger child sample, nor could we detect any systematic effect by age cohort.³⁵ These results are available in our appendix.

Finally, Model (6) explores if there the OAP affected enrolments for female children differently to male ones. The estimated coefficient on our interaction term is negative and highly significant, indicating that the OAP affected female enrolments 3 percentage points less than male enrolments. The Pension coefficient increases substantially from baseline, implying that the OAP increased the likelihood of male children being enrolled by 3.9 per centage points. Taken together, we can conclude that the OAP increased female enrolments by a modestly 0.9 percentage points, around a fifth the size of the male effect.³⁶

³⁵ We detect a small positive effect for 10-year-olds, where the likelihood of enrolment in treated households increases by less than a percentage point. However the estimate is less precise (p<0.05) and corresponds to a 0.01 per cent increase in enrolments.

 $^{^{36}}$ We also explore if gender differences extend to the sex of the treated pensioner. We find no significant difference in our results if the treated pensioner is male or female.

	Dependent variable:						
			Enrolled	In School			
	(1)	(2)	(3)	(4)	(5)	(6)	
Pension	0.093***	0.035	0.121***	0.099**	0.115***	0.156***	
	(0.017)	(0.024)	(0.033)	(0.039)	(0.038)	(0.021)	
Household Income	0.034***	0.026***	0.042^{***}	0.039***	0.030***	0.034***	
	(0.004)	(0.004)	(0.007)	(0.007)	(0.007)	(0.004)	
1911 Census	0.013***	0.003	-0.004	0.028^{***}	0.033***	0.023***	
	(0.005)	(0.005)	(0.011)	(0.010)	(0.008)	(0.005)	
Sex: Female	0.038***	0.054^{***}	0.061***	0.021**	0.018^{**}	0.038^{***}	
	(0.005)	(0.007)	(0.009)	(0.010)	(0.009)	(0.005)	
Female*Pension						-0.127***	
						(0.025)	
Age Cohort	All	13	14	15	16	All	
DED Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	
Household/Child Characteristics	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	
1901 Enrolments (%)	57.3	87.5	67.9	44.8	25.7	57.3	
1901 Female Enrolments (%)	57.7	87.8	67.8	46.2	27.2	57.7	
Observations	67,472	17,036	18,164	16,160	16,112	67,472	
\mathbb{R}^2	0.330	0.232	0.254	0.293	0.290	0.330	
Adjusted R ²	0.295	0.063	0.102	0.130	0.127	0.295	

Table 5: Regression results by age cohort and sex.

Note: Robust standard errors clustered at DED. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

6.4 Identification Assumption

Our identifying assumption is that schooling enrolments would not have evolved differently in treated households than untreated ones in the absence of the OAP policy. In short, we assume that prior to the OAP intervention we have parallel trends in schooling between treated and untreated children. An ideal approach to explore the validity of our parallel trend assumption would be to assign a placebo to individuals who were eligible for the OAP in 1911 but did not actually apply. Since our experimental design and data is not conducive to this test, we instead use the 1901 census to assign a placebo OAP to individuals who meet the eligibility criteria. These individuals living in multigenerational households are the most comparable to our 1911 treatment group, and given that we measure these households in 1901, it is impossible that they were treated.

Of the 32,528 children aged 13 to 16 living in multigenerational households in 1901, 42 per cent of them live with a placebo treated individual. Our placebo model is described in

our Empirical Framework section by Equation (2). Table 6 reports our results where Model (1) controls for child characteristics and applies DED fixed effects, Model (2) adds controls for household characteristics, and (3) accounts for age trends. We find no evidence that the placebo OAP increased school enrolments. While the placebo coefficient is positive, it is statistically insignificant across all specifications. We conclude that this evidence is consistent with our assumption of parallel trends, indicating that our empirical approach is valid.

	Dependent variable: In School				
	(1)	(2)	(3)		
Placebo OAP	0.034	0.033	0.020		
	(0.022)	(0.022)	(0.022)		
Household Income	0.041^{***}	0.048^{***}	0.048^{***}		
	(0.006)	(0.007)	(0.007)		
DED Fixed Effects	\checkmark	\checkmark	\checkmark		
Child Characteristics	\checkmark	\checkmark	\checkmark		
Household Characteristics	Х	\checkmark	\checkmark		
Age Trends	Х	Х	\checkmark		
N Placebo	14,358	14,358	14,358		
Observations	32,811	31,483	31,483		
R2	0.358	0.364	0.365		
Adjusted R2	0.289	0.292	0.293		

Table 6: Placebo test regression results

Note: Robust standard errors clustered at DED. Significance levels: ***p<0.01, **p<0.05, *p<0.1. The sample is multigenerational households in 1901 only. Placebo OAP is a categorical variable taking the value of £0.25 if the child's household contains a placebo treated pensioner in 1901. All controls other than household income are omitted from the table.

6.5 Robustness

For identification to be valid, changes in child characteristics and multigenerational household composition should not be correlated with OAP eligibility. Yet, we know that OAP treatment is non-random, with individuals attempting to select themselves into the policy by misrepresenting their age. Furthermore, substitution effects between the OAP and the Poor Law may have enabled the formation of new, poorer, multigenerational households by 1911 which are systematically different to those in 1901. Figure 3 visualises the change in the number of multigenerational households between 1901 and 1911 and, as expected, shows a large increase in the share of multigenerational households in the poorest income quintile. Regressing the presence of a multigenerational household as a function of the post-treatment period and accounting for age trends suggests that by 1911 there was a 7-percentage point increase in the

probability of multigenerational households amongst the poorest, most resource constrained households.³⁷

The OAP driven increase in the likelihood of multigenerational households is only problematic if it also changed household composition. Yet, as discussed in our Data section, the average differences in multigenerational household composition between 1901 and 1911 are small. We check if these differences are correlated with OAP treatment by regressing treatment as a function of our variables of interest. Table 7 reports our results and confirms that some modest differences are correlated with treatment. By 1911, children in treated multigenerational households are more likely to be literate, live in a larger household, less likely to be a farming household or contain a literate household head. While this increase in literacy could be driven indirectly by the OAP through increased schooling, multigenerational households in 1911 are clearly poorer and contain more younger children than in 1901. Since treatment is non-random and is correlated with these changes in household composition, then systematic differences may lead to spurious results.





³⁷ Results are available in Appendix B.

To explore the impact of systemic differences, we use a range of matching methods to detect and purge incomparable households from our sample. We first use a P-Score screening technique as a precursor to regression similar to those demonstrated by Angrist and Pischke (2008) and Crump et al. (2009), to address the issue of non-random treatment groups to reduce the likelihood that unobserved differences associated with the pension are confounding our estimates. As a rule of thumb, we use a logit to predict the probability of treatment, conditional on a multivariate covariate vector of characteristics which are not directly affected by OAP treatment.³⁸ We drop observations at the tails of the distribution where $0.1 < p(X_i) < 0.71$. In practice, this ensures that our regressions are estimated between comparable treatment and control groups, requiring no extrapolation for observations where there is no overlap in the distribution between treatment and control.³⁹

Encouragingly, the distribution of P-scores for treated and untreated households mostly overlap, and we screen out only 131 households. Our results are fully robust to this new sample, including when we use P-Scores weights to minimise the absolute differences between households. We also test our results using exact, nearest neighbour (with and without calliper) and mahalanobis distance matching. Our baseline results are fully robust to all matching techniques, reporting either an identical or modestly increased OAP effect. Our results for the most resource constrained households are also fully robust except for exact matching. There are only 4,896 households in the matched sample, and we lose a substantial amount of statistical power when we split the sample by HEI. However, we note that exact matched households are also much poorer than the census average. We conclude that that minor changes in household composition between 1901 and 1911 are not driving our main results.

³⁸ We drop age trend controls and the 1911 census dummy.

³⁹ This differs from Crump et al. (2009) who recommend trimming the distribution where $0.1 < p(X_i) < 0.9$. Trimming at these thresholds make no difference to our results given that both our treatment and control groups have a similar likelihood of being treated.

	Dependent variable:						
			OAP Eligible	:			
	(1)	(2)	(3)	(4)	(5)		
Income	-0.020***	-0.019***	-0.020***	-0.020***	-0.020***		
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)		
Sex: Female	-0.0001	-0.0001	-0.0001	-0.0001	-0.0001		
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)		
Illiterate	-0.007**	-0.007**	-0.007**	-0.007**	-0.007**		
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)		
Bilingual	-0.002	-0.002	-0.002	-0.002	-0.002		
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)		
Farming Household	-0.007***	-0.007***	-0.007***	-0.007^{***}	-0.007***		
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)		
Catholic	0.0005	0.0005	0.0005	0.0005	0.0005		
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)		
Illiterate Head	0.015***	0.015***	0.015***	0.015***	0.015***		
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)		
Urban	-0.007^{*}	-0.007^{*}	-0.007^{*}	-0.007*	-0.007^{*}		
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)		
HH Size	0.001**	0.001**	0.001**	0.001**	0.001**		
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)		
13-Year-Old Present		0.004^{***}					
		(0.001)					
14-Year-Old Present			-0.0003				
			(0.001)				
15-Year-Old Present				-0.001			
				(0.001)			
16-Year-Old Present					-0.003***		
					(0.001)		
DED Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark		
1911 Census	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark		
Age Trends	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark		
Observations	67,488	67,488	67,488	67,488	67,488		
R2	0.423	0.423	0.423	0.423	0.423		
Adjusted R2	0.392	0.393	0.392	0.392	0.392		

Table 7: Regression results- correlation between the OAP and variables of interest.

Note: Robust standard errors clustered at DED level: ***p<0.01, **p<0.05, *p<0.1. OAP eligible indicates treatment status in 1911 (£0.25 or 0). Child and over 70 income is subtracted from household income and is in log form. Illiterate is a dummy variable which indicates if the child is illiterate, while Illiterate Head refers to the household head. HH Size refers to household size.

Age misreporting in the 1911 census may be an additional source of endogeneity if individuals who are more likely to age misreport are systematically different from those who are not likely to age misreport. This problem will not be addressed by our matching techniques if these age misreporting individuals introduce an unobserved confounder to our model. To address this, we generate a measure of age misreporting intensity at Poor Law Union (PLU) level (n = 159) by taking the difference between the share of the full census population who were aged between 55 and 64 in 1901, and the share of the population aged 65 to 74 in 1911.⁴⁰ We normalise this measure between 0 and 1 for ease of interpretation, where 0 indicates a low propensity to age misreport. A description of this measure is available in the appendix, and shows that PLUs in the west, where poverty was much higher, have a higher propensity to age misreport. This could be problematic for our results since we also identify this area where the benefit of obtaining an OAP was greatest.

In our appendix, we show that there is no correlation between treatment and age misreporting intensity. Nevertheless, we test the robustness of our results to age misreporting by dropping all children who live in a PLU of misreporting intensity greater than 0.5. Our results are robust to age misreporting, albeit with the size of our estimated Pension coefficient falling modestly. Given we have indiscriminately purged many of the most resource constrained household from our sample, this fall is expected. We also drop several other unique geographies from our multigenerational household sample, including children residing in urban DEDs, cities and areas with a high share of census returns written in Irish. We find that children in treated households in cities did not respond to the OAP any differently than rural ones, while our baseline results are fully robust to alternative subsamples.⁴¹

Finally, we briefly address the issue of treatment group contamination since, as discussed in our data section, we overestimate the number of OAP claims by 8 per cent. However, we note that the presence of children in our treatment group who were not actually treated biases our ATE downwards towards zero, implying that our treatment effects underestimate the actual OAP effect on school enrolments. Figure addresses the validity of our treatment measure by regressing it against actual OAP claims in 1911 at county level (n = 32) and show in that it is a strong predictor of actual OAP claims ($R^2 = 0.94$). In our appendix we

⁴⁰ We select the younger cohort to limit the impact of age heaping in the 1901 census. We also choose to generate the measure at PLU level rather than DED level to prevent low population DEDs skewing the measure. We do not account for changes in demography driven by migration.

⁴¹ We also drop all observations in counties Cork and Down, since Figure 2 indicates that they are outlier counties when it comes to treatment assignment. Our results are robust to this subsample; indeed, the size of our estimated coefficient increases.

show that there is a weak positive association between age misreporting and the differences between our OAP treatment and actual OAP claimants at county level, indicating that we modestly overestimate the number of individuals treated in areas most likely to age misreport. As shown in our age misreporting robustness test, these individuals are not biasing our results. Our final check explores whether data attrition is driving our results since we drop around 3.8 per cent of older children who reside in multigenerational households due to missing data. Missing data occurs when households fail to fully complete their census return, which in our case, is principally linked to literacy and occupational status of the household head. In Appendix B, we re-estimate three variations of Equation (2) where first we drop the literacy status of the child, the literacy status of the household head, and finally if the household head is a farmer. We show that data attrition makes no difference to our results.

Figure 4: Scatter plot of actual OAP claims in 1911 against our estimate.



Note: Most observations lie within or close to the 95% confidence interval (shaded area). Outlier counties are Cork and Down. Source: Old Age Pensions (Ireland) 1913 Parliamentary Return.

7. CONCLUSION

We have shown that following a substantial UCT, school enrolments can increase when there exists a demand constraint on schooling. Unfortunately, since literacy information collected by the censuses is categorical, we cannot fully explore if these enrolments translated into increased human capital accumulation, or into regular attendance at school. Even so, the OAP undoubtedly created more opportunities for older male children to gain elementary literacy skills. Future work could improve our analysis by integrating school attendance figures, as well as improving our income estimates to better account for wage variation linked to work experience and sex, as well as non-labour income such as capital income.

Schooling context clearly matters. Using the example of the 1908 OAP in Ireland, we show that the OAP cash transfer only improved school enrolments for the most resource constrained households. The fact that we detect no effect of the transfer further up the income spectrum implies that poverty and resource constraints prevented the poorest households from actualising their demand for schooling in 20th century Ireland. For these poor households, enrolments only improved for the most marginal children for whom the opportunity costs of schooling were higher i.e. older males. This may be the case for several reasons. Firstly, OAP income is not risk free, treatment is temporary until the elderly recipient dies or is disqualified after investigation by a Pension Officer. A household might seek to diffuse this risk by prioritising children who can more readily find work. Secondly, the OAP transfer was sufficient to replace a full-time labour wage of at least one child. For example, a 5-shilling OAP was worth 1.6 times the weekly wage of a newspaper boy working full-time.⁴² To prevent a fall in living standards, households would need to substitute *at least* equal child labour income for OAP income. Therefore, it would not be conducive for households removing multiple children from the workforce.

While we do not find that the OAP discouraged female enrolments, societal gender norms and the lack of socio-economic opportunities for women offers one rationale why the policy improved male enrolments five-times more than females.⁴³ Alternatively, the policy may also have helped males to catch-up with their female peers. Older female children were moderately more likely to be enrolled in school, while qualitative information from the CNE notes a perennial concern of poor school attendance, particularly the extent that the agricultural

⁴² Figure taken from 1902 Inter-Departmental Committee on the employment of children during school age, especially in street trading in the large centres of population in Ireland. The report is a survey of several hundred children employed in street trading. Figure indicates a typical newsvendor boy in Chancery Lane Station, Dublin. ⁴³ The National School curriculum was also different for males and females, where for the latter subjects relating to house keeping and needlework were emphasised.

calendar disrupts the education of boys. A potential avenue for future work could explore if the OAP offered rural boys a second chance at school to obtain rudimentary literacy skills.

To conclude, OAP cash transfers are not regarded as a key pillar in poverty reduction strategies in developing countries. We have shown that OAP cash transfers can have significant positive effects on the developmental trajectories of economies by mitigating the short-run opportunity cost constraints of schooling for older, marginal, male children within multigenerational households. Our research also highlights that for UCTs, context matters, whether that be the existence of a diminished demand for schooling or the more limited economic opportunities for females. We argue that a deeper understanding of UCTs spillovers is essential to fully evaluate their ability to erase absolute poverty.

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APPENDIX A – INCOME NOTE

We standardise occupations in the 1901 and 1911 census' using the Historical International Standard of Classification of Occupations (HISCO) and group occupations into one of five categories: Farm Labourers, Urban Labours, Farmers, Skilled Workers, and White-Collar Workers. We group occupations uniformly by cross-referencing each HISCO with its relevant Historical International Social Class (Hisclass). Table A1 sets out summary statistics for our occupation groups and the relevant Hisclass which are assigned to them.

Occupation Group	Hisclass	Count	Share of Total (per cent)
Farm Labourer	12	420,402	11.4
Urban Labourer	11	353,158	9.6
Farmer	8	1,215,961	33.1
Skilled Worker	6 - 7, 9 - 10	1,290,013	35.1
White-Collar	1 - 5	398,309	10.8
Unassigned		30	0.0
Total		3,677,873	100.0

Table A1: Occupations, hisclass and summary statistics.

Farmers and Skilled Workers are the two largest occupation groups, accounting for over 68 per cent of occupations combined, followed by Farm Labourers and White-Collar workers. This is unsurprising given Ireland's status as a largely agriculture-based economy throughout most of its history. While there appears to be slightly more Skilled-Workers than Farmers, we note that we find that HISCO standardisation struggles with female employment, particularly for women who describe themselves as 'farmer's wife' or 'farmer's daughter'. If these women, like 'farmer's son', were counted as Farmers then the number of individuals employed in farming would be much higher. As such, this appendix note will proceed with Section 1 which describes the allocation of Farmer's incomes, before Section 2 which describes the same for Skilled-Workers, Section 3 which describes assignment for both Farm and Urban Labourers, and Section 4 which describes assignment for White-Collar workers. Section 5 will discuss our final income calibration and conclude with a description of the distribution of Irish incomes.

FARMER INCOME

We assume that farmer's incomes are fully derived from the land, accounting for production, the marketable share of produce, prices and farm size. We vary farmer income by Poor Law Union (PLU) as the most consistent and granular level of geography that we have data on. We take crop yields, livestock figures and market prices from the annual reports of the Department of Agriculture and Technical Instruction (DATI), calculate the marketable share of produce

using approximations from Barrington (1927) and Turner (1987), and calculate average farm sizes using grouped agricultural land data from the General Report of the 1911 Census of Ireland. Finally, we supplement this data with approximations of other commodity income such as milk, wool, eggs, turf and animal hides using Turner (1987) and figures from the Vice-Regal Commission on the supply of Irish Milk (1913).

For crop yields, the DATI annual reports allow us to collect yields (in weight) of all major crops produced in Ireland at the time including wheat, oats, barely, rye, flax, and potatoes, as well as crops used for animal feed (turnips, beets, cabbage) and grass/hay production. We assume that crops typically used for animal feed were not sent to market, and approximate that the average farmer sent 48 per cent of wheat, 24.3 per cent of oats, 70 per cent of barley, 5.9 per cent of hay, 18.5 of potatoes and 100 per cent of flax to market. We then convert this marketable share of produce for each PLU to GBP (£) using DATI's agricultural market prices for 1911.

Consistent with other explorations of Irish agricultural production, we approximate livestock slaughter rates by calculating the rate of livestock disappearance for various animal classes between 1910 and 1911. This includes cattle – bulls, milch, heifers and calves (under one year old, 1 - 2 years old, and over 2 years old), sheep – rams, ewes and lambs (under one year old and older than 1 year), and pigs – boars, sows and piglets (under 6 months old and over 6 months old). Due to limited price data, we cannot directly account for income from horses, mules, goats or various species of poultry. These will be indirectly accounted for in our final income adjustments. Furthermore, the disappearance rate of cattle and sheep accounts for natural mortality rates of 5 and 12 per cent respectively, as suggested by Turner (1987). As livestock prices vary depending on unobservable animal characteristics such as size, weight and quality, some generalisation is necessary. We convert disappeared animals into GBP (£) for each PLU using figures contained in Table A2 which has been taken from the 1911 DATI market prices.

Animal	Price (£) per head
Lambs	1.22
Sheep*	1.64
Piglet	1.14
Pig	1.64
Calf	2.27
Cow*	10.84

Table A2: Livestock prices 1911.

*The store price of adult animals has been used.

Other than raising animals for market, they also produce other valuable agricultural commodities. We estimate wool income as per Turner (1988) and assume that each sheep over 1 year old which has not been raised for market produces one 11b (5kg) fleece annually, which we convert into GBP (£) using the 1911 market price of wool. We allow 95 per cent of the mature poultry population to produce 50 eggs annually to account for age and mortality, which are converted into GBP (£) for each PLU by the dozen. While we cannot fully account for the intricacies of the Irish dairy industry, we exploit milk production estimates from the Vice-Regal Commission on the supply of Irish Milk (1913) and Turner (1987).

We calculate that the number of working milch cows in 1911 are total milch cows minus heifers in calf, minus a further 5 per cent to account for cows which have been allowed to go dry during some part of the year. We assume that each working milch cow produces 375 gallons (roughly 1704 litres or 3000 pints) of milk per year, as the midpoint between the Commission's estimate of 400 gallons and Turner's estimate of 350 gallons. We then calculate the marketable share of milk production by accounting for livestock rearing costs and subtracting 35 gallons per calf and 5 gallons per piglet in each PLU. We take the price of milk from McLaughlin et al. (2023) at 4.66 pence (£0.02) per gallon as the average paid by Irish creameries between 1897 and 1918. Finally, we apply a 5 per cent markup to our income figures for each commodity to account for missing production such as turf, horses, mules, hides, poultry and the slaughter of spent animals.

We approximate that farmers made a total of £36,217,038 from market in 1911, with the average PLU generating an income of £229,222. To assign these incomes to individual farmers, we account for PLU differences in average farm sizes and agriculturally productive land and assume that a household containing *at least* one farmer earns income from one average sized farm. This is an important assumption since it avoids issues created by the under-counting of female farmers. We calculate average farm size at PLU level in acres from the 1911 census General Report. This report groups the number of farming households and holdings into 12 different acreage classes (less than 4 acres to more than 300). We calculate that the average PLU farm size in 1911 was quite small at 20.7 acres ($0.08km^2$). To convert this farm-size measure to income, we calculate the income generate per acre of productive land in each PLU by subtracting land that is classified as barren, towns/buildings, bodies of water, marsh, woods and bogland. We then calculate the amount of income generated per acre of productive land and multiply this with average farm size. We estimate that the average PLU Farm income was £0.85 per week (17 shillings) or around £45 per year.

6.6 SKILLED-WORKER INCOME

Skilled workers are a diverse demographic in early 20th century Ireland. Our HISCO standardisation groups obvious occupations into this group such as tradesmen such as carpenters, masons, blacksmiths and shipwrights, as well as bootmakers, weavers, seamstresses and domestic servants. Using the Parliamentary Papers archive, we gather an extensive wage series for a variety of domestic servants, textile workers, railway workers, communications workers and tradesmen.

Domestic servants are the largest sub-group of Skilled-Workers, accounting for over 32 per cent of the total. These workers were overwhelmingly females employed as housekeepers, with only 8 per cent of observations being male. Given their few numbers, no wage series exists for male domestic servants and must be estimated indirectly from female wages. We exploit the Collet (1899) Board of Trade Report on Money Wages of Indoor Domestic servants which surveyed households across the UK. While we are unable to scrutinise to the representativeness of this data, the report disaggregates UK statistics by country and presents Irish wages for cooks, dairymaids, kitchen maids, lady's maids, laundrymaids, nurses, parlourmaids, housekeepers and housemaids. Table A3 sets out our assigned baseline wages for each class of domestic servant.

Occupation	Baseline Weekly (shilling)	Baseline Annual (£)	
Cook	5.4	14.1	
Dairymaid	4.6	12.0	
Kitchen Maid	3.8	10.0	
Lady's Maid	6.9	18.0	
Laundry Maid	5.4	14.0	
Nurse	4.5	11.7	
Parlour Maid	6.1	16.0	
Housekeeper*	5.1	13.5	
Housemaid	5.0	13.0	

Table A3: Female Domestic Servant wages by occupation.

*We note that housekeeper wages appear low. While Irish domestic servants are poorly represented in the Collet data, housekeepers in Ireland may also not have had the same managerial function as their counterparts in Great Britain. Housekeeper was the largest group of domestic servants in both census years, so it is unlikely that Irish housekeepers were running large servant households.

While the report surveyed 359 households in Dublin, Belfast, Cork, Limerick, data limitations prevent us from accounting for geographical wage variation, or even variation by age. The best that we can do is to account for variation across occupations in the number of domestic servants living in the household by assigning Collet's reported baseline average wage for each occupation, with wages increasing incrementally by 15.8 per cent for every additional domestic servant in the household in line with Collet's averages. We cap these increments at 7

other domestic servants in the household as the maximum household size indicated by the Collet report. We arbitrarily account for the gender wage gap by increasing male domestic servant wages by 16 per cent. This figure has been obtained from the institutionalised wage gap in teaching at the time and should be interpreted as a conservative approximation of the wage gap. As such, we calculate an average domestic servant wage of £13.3 a year, or around 5.11 shillings a week.

We obtain average baseline wages of textile workers such as weavers, spinners and winders from Board of Trade annual reports on the Standard Time Rates of Wages 1901-1907. These reports indicate the highest and lowest salaries for these occupations by sex, enabling us to account for this variation. We assume that the most productive and experienced textile workers earn the highest salaries, and generate a generic work experience variable for each of the 65,000 non-retired textile workers as their age minus 18. All negative work experience values are recoded to 0. Using the difference between the maximum and minimum wage for each occupation and sex, we assume that females earn an extra £1 for every year of work experience they have up to their respective maximum, while men earn an extra £1.16 per year (16 percent wage gap). We estimate an average skilled textile worker wage of around £31.26 per annum or around 12 shillings a week. Table A4 describes a breakdown of base and maximum wages and increments for textile workers.

Occupation	Sex	Base Wage (£)	Maximum (£)
Spinner	Male	23.5	33.9
Weaver	Male	23.5	40.9
Winder	Male	23.5	35.6
Spinner	Female	23.5	32.5
Weaver	Female	23.5	38.5
Winder	Female	23.5	33.9

Table A4: Base and maximum wages for textile workers by sex.

The 1891 BoT General Report on the Wages of the Manual Labour Classes in the United Kingdom provide wage data for rail workers across three of Ireland's major rail companies: the Great Northern, the Midland and Great Western and the Great Southern and Western. The only major rail network missing is Dublin, which for simplicity we allocate the average of all three networks. Wage data is grouped by rail department, occupation, age (child and adult) and gender, allowing us to account for all these dimensions. The report includes wage returns from occupations from all aspects of freight and commercial rail travel from ticket inspectors, drivers and porters, to brakemen, engineers, van-men, draymen, watchmen, signalmen, firemen, cleaners and stokers. After assigning wages to individuals based on their location, gender and

age there are only 900 observations we are unable to account for. For simplicity we assign these individual their respective coach department average wage. We calculate an average rail worker wage of £43.89 per year or around 17 shillings a week.

The 1893 Report on the Wages of the Manual Labour Classes in the UK and Reports of Standard Time Rates of Wages 1901-1907 contains a rubric for calculating communication workers, allowing us to approximate wages for postmen, sorters, porters, messengers, labourers and mail carriers working in the General Post Office in Dublin. Table A5 details a reproduction of this rubric.

Occupation	Base Wage (£)	Annual Increment (£)	Until Max (£)
Postman	44.3	2.6	84.2
Sorter	39.1	5.2	153.8
Porter	46.9 (13.0) *	2.6	65.2
Messenger	46.9 (13.0) *	2.6	65.2
Labourer	46.9 (13.0) *	2.6	65.2
Carrier	46.9 (13.0) *	2.6	65.2

Table A5: Postal Workers wage rubric

Note: * figure in parenthesis is base wage for children. No increment for children is recorded. Source: 1893 General Report on the wages of the manual labour classes.

Like the case of textile workers, we generate a work experience variable and distribute annual increments until the max salary is reached as before. We calculate that the average salary of communications worker in the Irish postal service earned around £45 a year or around 18 shillings a week.

Finally, we assign wages to an extensive list of 180,146 skilled craftsmen such as carpenters, bricklayers, masons, slaters, plumbers, plasters and turners. A full list of these occupations can be found at the end this income note. These wages are included in Board of Trade reports and record hourly wage rates, weekly hours worked and weekly income of tradesmen across 43 Irish towns. To maximise the spatial distribution of these wages and minimise the impact of incomplete wage series, we aggregate all wage series to Poor Law Union level and calculate an average skilled craftsman wage. Using this method, we can estimate an average skilled worker wage of around £78 per annum or 30 shillings a week for 34 Poor Law Unions. To assign an average skilled craftsman wage to the remaining 125 Poor Law Unions, we use a method of inverse distance weighting to calculate an average maximum wage for each poor law union based on its distance from the nearest PLU with a known average tradesman wage, and the distance between PLUs with known average tradesmen wages. Figure A1 maps the geographical variation in skilled tradesmen in the census by Poor Law Union, we

calculate an average wage of 34.11 shillings a week or around £88.93 a year. Our average estimate is practically identical to that reported by Ó Gráda (1994).



Figure A1: Tradesmen average wages at PLU level.

Note: black boundaries are county borders. The areas with the highest tradesmen wages are the larger cities-Belfast, Dublin and Cork.

LABOURER INCOME

Labourers are divided into two groups by Urban Labourers which include general labourers, and stonebreakers, while Farm Labourers include all agricultural labourers such as ploughmen and farm servants. The 353,158 Urban labourers are concentrated in cities like Dublin and Belfast and are assigned Ó Gráda's (1994) national estimate of urban labourer wages. Male Urban Labourers are assigned a weekly income of 16.75 shillings a week or around £44 a year, while females are assigned 9.4 shillings a week or around £25 a year. After assigning these wages, we calculate that the average Urban Labourer was paid around 15 shillings a week or £39 a year. Future work will concentrate on introducing greater variation to these Urban Labourer wages.

Farm Labourer wages are assigned at county levels taken from Fitzpatrick's (1980) agricultural labourer series. Table A6 describes this income series and shows that the weekly rate of pay of Farm Labourers was similar across most counties, with notable exceptions being Westmeath and Wexford. For the 420,402 Farm Labourers we calculate an average wage of around 10 shilling a week or around £26.6 a year.

County	Weekly Wage shilling	Annual Wage (£)
Antrim	12.25	31.85
Armagh	10.75	27.95
Carlow	11.00	28.6
Cavan	10.00	26.00
Clare	10.92	28.38
Cork	10.75	27.95
Donegal	10.25	26.65
Down	12.00	31.20
Dublin	12.00	31.20
Fermanagh	10.25	26.65
Galway	10.50	27.30
Kerry	12.17	31.63
Kildare	11.75	30.55
Kilkenny	10.50	27.30
Offaly	10.00	26.00
Leitrim	10.50	27.30
Limerick	11.42	29.68
Derry	10.00	26.00
Longford	10.92	28.38
Louth	10.00	26.00
Mayo	10.17	26.43
Meath	11.25	29.25
Monaghan	11.50	29.90
Laois	10.33	26.87
Roscommon	11.00	28.60
Sligo	10.17	26.43
Tipperary	10.75	27.95
Tyrone	11.00	28.60
Waterford	10.58	27.52
Westmeath	9.50	24.70
Wexford	8.58	22.32
Wicklow	11.33	29.47

Table A6 County level estimates of Farm Labourer wages.

WHITE-COLLAR INCOME

Despite Ireland's lower level of development than in Great Britain, White-Collar workers make up a sizable minority within the Irish workforce in 1901 and 1911. These workers are quite diverse, spanning professional classes such as police, teachers, clerks, doctors and solicitors, the merchant classes such as shopkeepers, publicans, and dealers, as well as the elite classes such as wealthy landowners, members of parliament and senior government ministers. Unsurprisingly, many of these white-collar occupations are concentrated within cities and towns, particularly centres with commercial or administrative significance.

We identify 398,309 white collar workers across our period of interest, the vast majority of which work as clerks for small private companies. While data limitations prevent us from assigning wages to this group with a high level of precision, fortuitously around a fifth of all While-Collar occupations work in the public sector. Wages for these National School teachers, civil servant clerks, police and Poor Law doctors were determined algorithmically, and like our example of Postal Workers (classified by HISCO as a Skilled Worker), these wage rubrics were published in legislation and parliamentary returns for the period. For each of these occupations, we gather data regarding base salaries, pay increments and maximum salaries across multiple dimensions such as rank, age and sex.

The 1904 Dale Report on the Irish National School System includes a pay rubric for all teachers as set out by legislation in 1900- Table A7 reproduces this rubric. For ease of interpretation, we note that in our research context Principal Teachers refer to full-time Teachers, not to school headmasters or headmistresses. A male Assistant Teacher with three years of experience would have received an income of £63 a year (£56 base salary £7 increment), while a female Principal Teacher with 6 years of experience would have received her maximum income of £89 (£73 base salary and £16 increment). In general, most teachers qualified for their max salary for their respective grade after 6 years.

Sex	Grade	Occupation	Grade Salary (£)	Rising by Triennial Increments of	To Maximum Income
	III	Assistant Teacher	56	7	77
MALE	II	Principal Teacher	87	10	107
MALL	Ib	Principal Teacher Lower	117	10	127
	Ia	Principal Teacher Higher	139	12	175
MALE		Class Monitor*	9.8	None	None
	III	Assistant Teacher	44	7	65
FEMALE	II	Principal Teacher	73	8	89
TEMALE	Ib	Principal Teacher Lower	97	8	105
	Ia	Principal Teacher Higher	114	9	141
FEMALE		Class Monitor*	9	None	None

Table A7 Determination of teaching wages from 1st April 1900.

* Class Monitors have been added to this rubric by the authors. These individuals were usually older children who helped Principal Teachers manage large class sizes. Source: Excerpts from Dale Report (1904) on Irish Primary Education.

While this rubric has the potential to account for most variation in teaching wages, teachers tended not to indicate their specific grade on the census so some generalisation is necessary. Firstly we identify all Assistant Teachers, Headmasters/Headmistresses and Monitors and assign wages in accordance with the rubric, where years of work experience are calculated as age minus 18, while any negative work experience values are recoded to 0⁴⁴. Head Teachers are assigned salaries as First (higher) Grade Principal Teacher, although in reality Head Teachers' salaries were partially determined by pupil numbers. Dale (1904, 23) suggests that head teacher posts tend to be filled by First Grade Teachers, so we follow this example.

For the remaining unassigned Principal Teachers, we conservatively assume that they are all Second Class Principal Teachers. We opt for the lowest pay scale here because not all schoolteachers were fully qualified in line with the teaching regulations or employed by the national school system. Despite improvements in teacher training, by 1911 31 per cent of teachers still had no official teaching certification and may not have been covered by this wage rubric. We calculate, the average Teacher salary was around £94.11 a year, or around 36.01 shillings a week. This rate is quite encouraging, being only marginally higher than the average skilled worker during the period. Indeed, the Dale Report (1904) estimates that the average male teacher wage in 1904 was £99.50. Our estimates for 1911 put the average male teacher similarly around £105.84.

We estimate police wages using rubrics contained within the Royal Irish Constabulary (Ireland) Reform Act 1908, and the 1914 Committee of Inquiry on the Royal Irish Constabulary and Dublin Metropolitan Police. Given that our census methodology only retains individuals residing in households on the night of the census, we do not calculate incomes for police officers who were garrisoned in a barracks. Tables A8 and A9 describe a reproduction of these rubrics for members of the Royal Irish Constabulary and Dublin Metropolitan Police.

⁴⁴ We use this work experience variable for all occupations for comparability. We also note that according to Rule 177 (a) of the Rules and Regulations of the Commissioners of National Education in Ireland (1903) requires a newly appointed teacher to be at least 18 years old, and no older than 35 years old.

Rank Royal Irish Constabulary	Base Salary (£)	Rising after (years)	Increase (£)	Thereafter rising (years)	Increase (£)	Wage Cap
District Inspector 1st Class	225	3	25	None	None	300
District Inspector 2nd Class	165	5	15	None	None	180
District Inspector 3rd Class	135	None	None	None	None	None
Head Constable Major	130	None	None	None	None	None
Head Constable	96	5	8	None	None	104
Sergeant	78	4	5	None	None	83
Acting Sergeant	76	None	None	None	None	None
Constable	39	0.5*	15.6	3	1	73

Table A8: Royal Irish Constabulary (RIC) wages 1908 - 1914.

*0.5 refers to a 6-month probationary period for all new recruits, after which base salaries were increased by $\pounds 15$ and 12 shillings ($\pounds 15.6$).

Rank Dublin Metropolitan Police	Base Salary (£)	Rising after (years)	Increas e (£)	Thereafter rising (years)	Increase (£)	Wage Cap
Chief Superintendent	400	1	15	None	None	500
Superintendent	250	1	10	None	None	320
Inspector	120	1	6	None	None	160
Station Sergeant	104	None	None	None	None	104
Sergeant	89	2	5	3	5	99
Constable	60	1	5	2	5	78
Supernumerary	40	None	None	None	None	40
DETECTIVE DIVISION						
Detective Officer*	78	None	None	None	None	78

Table A9: Dublin Metropolitan Police (DMP) wages 1908 - 1914.

*We only include Detective officers since we can identify these individuals in the census. All other ranks within the detective division cannot be disentangled from the rest of DMP ranks.

Our method for assigning police wages using these rubrics is identical to that of teachers with only three adjustments; firstly, as women were banned from joining the police, we drop any individual who may have inaccurately filled in their occupation. Secondly, as recruits were required to be at least 19 years of age, we calculate work experience as age minus 19 rather than 18. Finally, there are around 1,100 members of the police which we cannot easily assign a rank to. 75 per cent of these observations include some misspelling or acronym of "constable", while the remaining 25 percent identify their occupation merely as RIC, or some other unique string such as "harbour police", "messenger", "bandsman" or "DMP". For simplicity, we assign all these individuals constable wages, differentiating by RIC or DMP membership. We calculate that the average police officer earned around 27 shillings per week or around £72.3 per year.

Finally, we assign wages to clerks, who account for nearly 10 per cent of all White-Collar occupations. We detect over 2000 unique descriptions of clerks across both census and as such generalisation is necessary to assign wages. We first disentangle those clerks who are employed in the public sector, searching for terms linked to any Government Department operating in Ireland at the time⁴⁵. Table A10 reproduces several clerk rubrics by government department.

Grade	Base Salary	Annual Increment	Maximum Salary
The National Health Ins	urance Commission o	of Ireland	
1 st Class Clerk	350	15	500
2 nd Class Clerk	100	10	350
Assistant Clerk	55		150
The Local Government	Board of Ireland		
		15	300
1 st Class Junior Clerk	150	Thereafter	Thereafter
		20	500
		7.5	130
2 nd Class Clerk	70	Thereafter	Thereafter
		10	300
A agrictorit Claula		5	85
Assistant Clerk	45	Thereafter	Thereafter
		7.5	150
The Land Commission o	f Ireland		
1 st Class Clerk	350	15	450
2 nd Class Clerk	100	10	300
		5	105
3 rd Class Clerk	80	Thereafter	Thereafter
		7.5	150
Temporary Clerk	10d /hour	N/A	N/A

Sources: National Health Insurance Commission and Local Government Board from New Hire Returns (1912-1913); Land Commission from 1897 Land Commission Return for the HoC.

While we are unable to fully exploit the inter-departmental wage variation of clerks since, like teachers, clerks tended not to record while rank they were employed, we opt for a conservative estimate and assign fully time public sector clerk wages using 2nd Class Local Government Board wages. Assistant clerks are also assigned their equivalent LGB wages. Increments are estimated using an identical work experience variable as teachers. For non-

⁴⁵ Any clerk which mentions the following in their occupation: Local Government Board, Agriculture, Court, County, Land Commission, Poor Law, Post Office, Army, Registrar, Dublin Castle, Secretary, Police, Customs, Education, Health, Congested Districts, Prison, Public Records, Valuation, Corporation, Ordnance or Stationary Office. Variations of these terms cover all Government Departments operating in Ireland at the time.

Government clerks, we assume that there exists a public sector wage premium, where public sector clerks due to the wider nature of their responsibilities and government employer, are paid a higher rate than their private sector peers. We assign these clerks an annual base salary of £49, 70% of the base salary of 2nd Class LGB Clerks, while assistant clerks are assigned similarly at £31.50. While this is an arbitrary value intended to maximise variation, future efforts to refine this wage series can attempt to replace this arbitrary variation with geographical data related to clerks employed by much smaller local district councils and poor law unions, as well as wage reports in major newspapers for the period. We estimate that the average clerk earned around 43.93 shillings a week or around £114.53 a year and is the highest paid occupation that we estimate.

Although data limitations weaken our assignment of clerk wages, we maintain that they are at least a reasonable approximation of income. We can compare them to Poor Law Union Medical Officer wages (doctors) for 1911, which have been obtained at individual level for each of the 808 Poor Law Doctors. These are all highly skilled individuals, having obtained a university level education. Intuitively, it should be the case that these doctors have an average salary higher than the average clerk. In 1911, the average doctor salary was £128.71, or around 49.37 shillings a week. This exceeds our average clerk salary by around 11 per cent and suggests that they are a reasonable estimate.

FINE TUNING

To ensure that we account for as much variation in income as possible, we fine-tune our income approximations in several ways. Firstly, we recode all income estimates to zero for anyone who includes in their occupation description "out of work" or "unemployed". For all occupations where we have not accounted for the gender wage gap, we discount female incomes conservatively by 16 per cent, simulating the institutional wage gap in teaching. Since income is a function of an individual's human capital, we arbitrary discount the incomes of those who are illiterate or who can read only by 20 and 10 per cent respectively⁴⁶. Finally, retired White-Collar and Skilled Workers are assigned 58 per cent of their equivalent labour income to account for private pension schemes or other remittances, in line with the average retirement income of teachers and police officers.

⁴⁶ Discounts are compounding, so an illiterate female would expect to earn 64 per cent of her literate male counterpart.

Figure A2: Boxplot showing the distribution of wages in shillings a week by occupation group.



Figure A3: Map showing the distribution of average weekly per capita income by District Electoral Division in 1911.



Note: Where $\pounds 1 = 20$ shillings. Higher income DEDs in the west tend to be small towns. Black borders indicate counties.

Labourers	Farmers	Skilled V	Vorkers	White-Collar
(N = 7)	(N = 4)	(N =	= 48)	(N = 16)
Farm Labour (labourers & ploughmen) Urban Labourer	Farmer	Carpenters & Joiners	Cabinet Maker	Monitor
(general & builders)	Farmer's Son	Bricklayers	French Polisher	Teacher
Postal Labourer	Farmer's Daughter	Masons	Upholsterer	Head Teacher
Cleaner/ Charwoman	Farmer's Wife	Slaters	Painter	Constable
Porter (GPO)		Plumbers	Rivetter	Head Constable
Messenger Boy		Plasterers	Cook	Sergeant
Messenger (adult)		Turners	Dairymaid	District Inspector
		Smiths	Kitchen maid	Detective
		Fitters	Lady's maid	Superintendent
		Pattern Maker	Launderer	Chief Superintendent
		Brass Moulder/Finisher	Nurse (domestic)	Numerary
		Iron Founder	Parlourmaid	Assistant Clerk
		Plater	Housekeeper	Clerk
		Shipwright	Housemaid	Register (rail)
		Bookbinder/print		
		er/	Carman	Ticket inspector
		compositor	X 7	
		Spinner	Vanman	Postman
		Weaver	Dray	
		Winder Engingen (mail)	Checker (rail)	
		Signalman	Keeper (rail)	
		Carter	Brakemen	
		Goods Porter	Foreman (rail)	
		Goods Shunter	Driver (rail)	
		Fireman	Stoker	
		1 ii eiliali	Store	

Table A11 List of Occupations we have assigned wages by Occupational Category

LIST OF WAGE SOURCES

Our wage data is drawn from a multitude of archival material contained on Parliamentary Papers. This information is either contained in isolated parliamentary returns, Board of Trade (BoT) reports, or appendices to Committees of Inquiry or Commissions. Our data sources include a return on the wages of the manual labour classes 1886-1891, the 1897 return of the Irish Land Commission of name and salaries of civil servants, the 1899 report on Money Wages of Indoor Domestic Servants (BoT), annual reports on Standard Time Rates of Wages 1901-1907 (BoT), 1901 return of appointments to Local Government Board, the 1902 Report of the Street-Trading Children Committee, the 1904 Dale Commission report into Irish National

Schools, a 1913 return of civil service appointments and the 1914 Committee of Inquiry on the Royal Irish Constabulary and Dublin Metropolitan Police.

APPENDIX B

rabie D1. Regiession results, n			in meor			
		Ì	Dependen	t variable:		
		1911	Pension B	oost to Ind	come	
	(1)	(2)	(3)	(4)	(5)	(6)
Std. Relatives 70+	0.138***	0.141***	0.134***	0.140***	0.134***	0.131***
	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0003)	(0.0003)
Std. Household Income	-0.001***	0.005***	0.028***	-0.006***	-0.001***	-0.0001
	(0.00005)	(0.001)	(0.004)	(0.001)	(0.0004)	(0.0001)
Household Income Quintile	All	1	2	3	4	5
Household Size Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	703,323	141,878	157,480	133,582	139,617	130,766
\mathbb{R}^2	0.911	0.885	0.932	0.900	0.933	0.951
Adjusted R ²	0.911	0.882	0.930	0.897	0.931	0.950

Table B1: Regression results, main contributor to OAP income boost.

Note: Variables are standardised as per Gelman (2009). Since we have a binary dependent variable 0 has been recoded to -1. The number of relatives and household income are continuous and are divided by their standard deviation. Robust standard errors clustered at DED. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

8			
Age Cohort	1901-1911	1891-1901	Difference
Age Collott	Survival Rate	Survival rate	Difference
45-49	0.81	0.56	0.25
50-54	0.59	0.50	0.09
55-59	1.02	0.41	0.62
60-64	0.85	0.35	0.50
65-69	0.84	0.28	0.56
70-74	0.36	0.20	0.16
45-54	0.69	0.77	0.08
55-64	0.92	0.57	0.35
65-74	0.61	0.41	0.20
45-74	0.76	0.60	0.16
50-74	0.75		
55-74	0.81		
A Calcart	1911 change in		
Age Conort	population share		
>70	0.71		
>65	0.66		
>60	0.22		
>55	0.17		
>50	0.08		
>45	0.08		

Table B2: Age cohort survival rates and changes in population share.

Note: Age cohort survival rates are interpreted as the share of 45–54-year-olds in 1901 who survive as the population of 55–64-year-olds in 1911. Cohort boundaries selected to minimise the impact of Age Heaping. Cohort Survival rates 1891-1901 are approximations derived from Budd & Guinnane (1991) and Ó Gráda (2002). 1901-1911 survival rates calculated from the census by authors.

									Depe	endent vari	able:		
										In School			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Pension	0.003	-0.040	-0.059**	-0.017	-0.004	0.003	0.040^{***}	0.023	-0.004	0.035	0.121***	0.099^{**}	0.115***
	(0.008)	(0.029)	(0.027)	(0.019)	(0.017)	(0.016)	(0.015)	(0.016)	(0.019)	(0.024)	(0.033)	(0.039)	(0.038)
Age Cohort	All	5	6	7	8	9	10	11	12	13	14	15	16
DED Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark						
Household + Individual Demographics	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark						
Enrolled Share 1901 (%)	78.5	64.7	82.8	91.0	93.9	95.2	95.3	94.6	91.2	84.6	64.4	43.0	24.6
Observations	238,159	24,092	23,154	23,128	21,560	20,654	20,616	18,168	19,315	17,036	18,164	16,160	16,112
\mathbb{R}^2	0.356	0.377	0.362	0.300	0.254	0.233	0.226	0.237	0.229	0.232	0.254	0.293	0.290
Adjusted R ²	0.347	0.282	0.260	0.188	0.125	0.095	0.086	0.081	0.081	0.063	0.102	0.130	0.127

 Table B3 Regression Results, schooling spillovers to younger demographics.

Note: Standard errors clustered at DED. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

				Dependen	t variable:	
				In So	chool	
	(1)	(2)	(3)	(4)	(5)	(6)
Pension	0.095***	0.106^{***}	0.095***	0.110^{***}	0.096***	0.163**
	(0.017)	(0.018)	(0.018)	(0.022)	(0.022)	(0.068)
Household Income	0.031***	0.023***	0.025^{***}	0.023***	0.024^{***}	-0.045*
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.024)
1911 Census	0.036***	0.044^{***}	0.044^{***}	0.044^{***}	0.042^{***}	0.033
	(0.005)	(0.005)	(0.006)	(0.007)	(0.008)	(0.027)
Sample	Baseline	P-Screened	NN	NN+C	Mah	Exact
DED Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
Household + Individual Demographics	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
Weights	Х	\checkmark	\checkmark	\checkmark	\checkmark	\checkmark
Observations	67,418	67,287	59,895	37,448	37,448	4,896
\mathbb{R}^2	0.331	0.329	0.334	0.364	0.358	0.539
Adjusted R ²	0.296	0.294	0.294	0.304	0.297	0.348

Table B4: Reducing impact of systematic differences in household composition. Alternative Matching Techniques

Note: Robust standard errors clustered at DED. Significance levels: ***p<0.01, **p<0.05, *p<0.1. P-Screened refers to our P-Score screened sample where $0.1 \le pscore \le 0.71$, NN refers to Nearest Neighbour matching, NN + C refers to Nearest Neighbour with a Calliper (0.01), Mah refers to Mahalanobis distance matching and Exact refers to Exact matching.

			H	IEI QUINTIL	E	
Sample	(ALL)	(1- POOR)	(2)	(3)	(4)	(5- RICH)
Pension Baseline	0.095***	0.169***	0.003	-0.094**	-0.036	0.019
	(0.017)	(0.042)	(0.043)	(0.041)	(0.040)	(0.042)
Pension P-Score	0 096***	0 152***	0.007	-0.080**	0.028	-0.006
r clision r -score	0.090	0.132	0.007	-0.080	0.028	-0.000
	(0.017)	(0.041)	(0.043)	(0.038)	(0.044)	(0.044)
Pension NN	0.110***	0.167^{***}	0.058	-0.071	0.009	-0.006
	(0.022)	(0.046)	(0.056)	(0.053)	(0.060)	(0.063)
Pension NN + C	0.095***	0.162***	0.008	-0.086**	0.016	0.022
	(0.018)	(0.043)	(0.044)	(0.040)	(0.045)	(0.047)
Pension Mah	0.096***	0.160^{***}	0.035	-0.050	-0.042	0.032
	(0.022)	(0.045)	(0.054)	(0.054)	(0.056)	(0.058)

 Table B5: OAP effect across the income spectrum with alternative matching techniques.

 Exact matching omitted due to lack of income variation.

Note: Regression results and SEs reported ONLY. For each model, robust standard errors clustered at the DED. Significance levels: ***p<0.01, **p<0.05, *p<0.1. Dependent Variable is Enrolled in School and full-specification controls and age trends are included. Fixed Effects applied at DED level.

			Dependen	t variable:		
			Enrolled	in School		
	(1)	(2)	(3)	(4)	(5)	(6)
Pension	0.089^{***}	0.075^{***}	0.091***	0.087^{***}	0.082^{***}	0.055^{**}
	(0.019)	(0.020)	(0.017)	(0.020)	(0.020)	(0.024)
Urban DED	-0.033**	-0.033**	-0.032**			
	(0.015)	(0.015)	(0.014)			
Urban * Pension	0.032					
	(0.033)					
		No Ago			N.	
Sample	Baseline	Misreport	No Irish	No Cities	Irish/Cities	Drop all
Sample DED Fixed Effects	Baseline √	Misreport √	No Irish √	No Cities √	Irish/Cities	Drop all √
Sample DED Fixed Effects Household + Individual Demographics	Baseline √ √	No Age Misreport √	No Irish √ √	No Cities √ √	Irish/Cities ✓	Drop all ✓ ✓
Sample DED Fixed Effects Household + Individual Demographics Observations	Baseline ✓ ✓ 67,418	No Age Misreport ✓ ✓ 48,213	No Irish ✓ ✓ 66,608	No Cities √ √ 54,912	Irish/Cities √ 54,137	Drop all ✓ ✓ 35,605
Sample DED Fixed Effects Household + Individual Demographics Observations R ²	Baseline √ √ 67,418 0.331	No Age Misreport √ 48,213 0.338	No Irish ✓ ✓ 66,608 0.331	No Cities √ √ 54,912 0.327	Irish/Cities √ 54,137 0.327	Drop all √ 35,605 0.335

Table B6: Regression results: showing that OAP effect is robust to various geographies including high age misreporting, Irish language areas and cities.

Note: Standard errors clustered at DED. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

Table I	B7: Regressi	on results.	Showing	that our	results	are robust	to potential	erroneous
treatm	ent in outlier	[,] counties	Cork and	Down.				

	Dependent variable:					
	Enrolled in Sch	pool				
	(1)	(2)				
Pension	0.095***	0.109***				
	(0.017)	(0.018)				
HH Income	0.031***	0.031***				
	(0.004)	(0.005)				
1911 Census	0.036***	0.038***				
	(0.005)	(0.005)				
Sample	Baseline	Drop Cork/Down				
DED Fixed Effects	\checkmark	\checkmark				
Household + Individual Demographics	\checkmark	\checkmark				
Observations	67,418	58,176				
R ²	0.331	0.331				
Adjusted R ²	0.296	0.295				

Standard errors clustered at DED. Significance levels: ***p<0.01, **p<0.05, *p<0.1.

	Dependent variable:							
	In School							
	(1)	(2)	(3)	(4)				
Pension	0.093***	0.085^{***}	0.081^{***}	0.081***				
	(0.017)	(0.017)	(0.017)	(0.017)				
Literacy (Child)	-0.303***	-0.311***	-0.314***					
	(0.016)	(0.015)	(0.015)					
Farming Household	-0.006	-0.002						
	(0.005)	(0.005)						
Literacy (Head)	-0.035***							
	(0.005)							
DED Fixed Effects	\checkmark	\checkmark	\checkmark	\checkmark				
Household Characteristics	\checkmark	\checkmark	\checkmark	\checkmark				
Age Trends	\checkmark	\checkmark	\checkmark	\checkmark				
Observations	67,488	67,836	69,743	70,076				
\mathbb{R}^2	0.330	0.329	0.328	0.319				
Adjusted R ²	0.295	0.294	0.293	0.285				

Table B8: Regression results showing no data attrition bias.

Note: Robust standard errors clustered at DED. Significance levels: ***p<0.01, **p<0.05, *p<0.1. Around 3.8 per cent of total sample of children attrit due to missing data. Practically all attrition is driven by missing literacy and occupational (household head) data.

Figure B1: Age Misreporting intensity by Poor Law Union in 1911.



Note: Age misreporting is the difference between population aged 55-64 in 1901 and aged 65-74 in 1911. The measure is then normalised between 0 and 1 for ease of interpretation where 0 indicates a low propensity to age misreport.

Figure B2: Scatter plots showing that we modestly overestimate the number of OAPs in areas of high age misreporting.



Figure B3: P-Score Density plot



Note: This plot shows that treated and control group have very similar probabilities of being treated. Vertical red lines indicate our screening range between P-score of 0.1 and 0.71.