Childcare, social skills, and the labor market

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Abstract

This paper provides some of the first large-scale evidence linking the effects of childcare programs on long-term outcomes to social skills measured in adulthood. First, we evaluate the effects of universal childcare in Finland following the sudden passing of the *Childcare Law of 1973* and find that small average effects of public childcare access conceal considerable heterogeneity; public childcare levels the playing field, reducing the family-son rank-rank income correlation by 0.05. Second, we attempt to understand how and why public childcare access shapes long-term outcomes. We find that treatment effects on income are most correlated with treatment effects on skills related to social competence ($r = 0.38$), somewhat correlated with treatment effects on academic skills ($r = 0.22$), and uncorrelated with treatment effects on a measure of fluid intelligence ($r = -0.04$). These treatment effect correlations support the hypothesis that the long-term economic effects of childcare programs are likely to operate through lasting effects on social skills. Consistent with the view that childcare affects long-term outcomes by changing early childhood socialization, we show that children likely to be exposed to lower quality socialization in their homes are likely to benefit from public childcare access.

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Although there is a consensus regarding the importance of early childhood in shaping long-term outcomes (Heckman, 2006; Yoshikawa et al., 2013; Almond et al., 2017; Black et al., 2017), we still know little about how these effects operate (Duncan et al., 2022). A number of studies have found that childcare programs affect short term outcomes across various domains, that these effects disappear in the medium term, but then re-emerge in measures of adult outcomes such as earnings, health, and crime (Deming, 2009; Chetty et al., 2011; Heckman et al., 2013; Bailey et al., 2020; Li et al., 2020). A common explanation for this pattern is that the persistent effects of childcare operate through socio-emotional rather than cognitive skills (see, for example, Heckman et al., 2013). However, since linking the effects of childcare to measures of these skills from adulthood has been challenging, this hypothesis lacks strong empirical support.

We provide some of the first large-scale evidence on how the long-term effects of public childcare programs operate, focusing explicitly on the role of skills measured in adulthood. We study the consequences of the expansion of public childcare in Finland following the *Childcare Law of 1973*. National administrative registries allow us to track the full population from their birth through education and the labor market. Moreover, exceptionally detailed data from the Finnish Defense Forces covering 80 percent of male cohorts gives us several measures of skills across academic and social domains measured in adulthood.

In the first part of this paper, we evaluate the effects of universal public childcare on average and across the family income distribution. After establishing that universal childcare in Finland levels the playing field – reducing the family-son income rank correlation by 0.05 – the second part of the paper studies how and why childcare shapes long-term outcomes.[1]

We evaluate the effects of changes in public childcare access resulting from the *Childcare Law of 1973* using municipality by cohort variation and a binary two-by-two differencs-in-differences strategy. Since there was no access to public childcare outside cities before 1973, we focus on rural municipalities where home-care was the dominant form of childcare before the policy. Without sufficient resources for the government to provide public childcare in all municipalities right away, only some municipalities received funding for public childcare in the initial expansion of childcare access following the *Childcare Law of 1973*. We compare cohorts born in the first set of rural municipalities to receive public childcare (treatment) to the same cohorts born in similar municipalities that only received public childcare in later years (comparison). Just a few years after the introduction of the policy, about 35 percent of eligible cohorts in the treatment municipalities attended public childcare; the comparison municipalities remained unaffected by the policy. Before the introduction of public childcare, the outcomes of children in treatment and comparison munic-

[1] In a companion paper we examine the role of public childcare access on maternal labor market outcomes over the life-cycle, and find economically significant increases in maternal labor market participation that persist to retirement, well past their childrens' childcare eligibility (Mäkinen and Silliman, 2022).

ipalities progressed in a parallel manner, both on average, and among individuals born to similar types of families. Moreover, the introduction of public childcare does not coincide with changes in the composition of families in treated versus comparison municipalities.

After the introduction of public childcare, cohorts in treatment municipalities eligible for public childcare experience no average improvements in educational attainment, measures of adult skills, or labor market outcomes. However, these average estimates conceal considerable heterogeneity for children growing up in different types of families. Children from the poorest fifth of families end up more than two percentage points higher in the adult income ranking (ages 35-40), while children from the richest fifth of families experience negative effects of almost a comparable magnitude. Together, these imply a reduction in the rank-rank correlation between family income and sons' income by 0.05.[2] For outcomes we observe in administrative data effects for females are similar to results for males. For males, we see that the effects on adult labor market outcomes are accompanied by effects on education and adult skills (across both academic and socio-emotional domains).

How does public childcare access shape long-term economic outcomes? Bailey et al. (2020) argue that for long-term effects of childcare to be driven by some skill, that skill must be: i) malleable as a result of childcare access; ii) relevant for long-term outcomes; and iii) would develop differently in the absence of childcare. Meeting these conditions, a commonly held view in economics is that long-term effects are driven by social skills (Deming, 2009; Heckman et al., 2013). Relatedly, developmental psychologists argue that *social competence* – the ability to recruit personal and interpersonal resources for goal achievement in social contexts – is the organizing construct in child development (Ogbu, 1981; Waters and Sroufe, 1983; Denham et al., 2003; Santos et al., 2014).[3]

To empirically interrogate the hypothesis that the effects of childcare on long-term economic outcomes are explained by lasting effects on social skills, we study the covariance between treatment effects on skills (social competence – and, for comparison, both academic and visual-spatial skills) and treatment effects on long-term outcomes like income. While such exercises are common in the context of teacher value-added – for example, studying the relationship between teacher effects on test scores and teacher effects on long-term outcomes (Chetty et al., 2011; Jackson, 2018), such covariances between treatment effects cannot be estimated in typical reduced form contexts where only one estimate per outcome is produced. To overcome this challenge, we follow the insight

[2]Since our estimates are intention-to-treat estimates (like other studies, we do not observe individual-level enrollment in childcare), this heterogeneity should be interpeted as suggestive of how children from different types of families in Finland actually responded to increased childcare access rather than how children from different types of families might respond to public childcare access if they chose to enroll.

[3]Recent empirical work from psychology attempts to shed a light on how social competence developes, suggesting, for example, that early childhood interactions with adults can be linked to later social motivation (Gunderson et al., 2013). See also Bost et al. (1998), who study social competence in the context of Head Start, Ladd (2005) for an overview related to child development and social competence, or Vaughn et al. (2009), for an empirical article on the measurement of social competence. For broader work on social competence in children, see Dodge et al. (1986) or Rose-Krasnor (1997).

from Angrist et al. (2022) and study the relationships between shorter and longer-term treatment effects across subgroups. To both tie our hands – reducing researcher degrees of freedom – in forming a large number of granular subgroups and to maximize treatment effect variation across subgroups, we base these groups on predicted treatment effect heterogeneity using the machine learning framework from Chernozhukov et al. (2021). We then estimate split-sample correlations between treatment effects on long-term outcomes and treatment effects on mediating outcomes.

We find that treatment effects on income are most highly correlated with treatment effects on social competence ($r = 0.38$), less correlated with treatment effects on academic skills ($r = 0.22$), and uncorrelated with treatment effects on visual-spatial skills ($r = -0.04$) – the closest measure to fluid intelligence or IQ.[4] These results are robust to several potential biases, and the correlations between skill treatment effects and the treatment effects on years of education exhibit a similar pattern. The lack of a treatment effect correlation between long-term outcomes and visual-spatial skills rules out these skills as the mediating mechanism, while the relatively strong treatment effect correlation between long-term outcomes and social competence provides evidence consistent with the idea that social skills may explain the long-term effects of childcare programs. Interestingly, this pattern of results is strikingly different from the raw correlations, where these three skills exhibit roughly similar correlations with adult income rank.

How does public childcare, then, shift social competence – and why might children from different types of families experience distinct effects from public childcare access? Prior literature suggests a succint hypothesis:[5] When children are shifted from their home environment to public childcare, children exposed to high-quality parent-child interactions may experience a decrease in the quality of care they receive, while children exposed to lower quality parent-child interactions may experience an improvement in the quality of the socialization they receive. We build on existing literature and provide a simple empirical test of this theory. Price (2008) show that first-born children receive more time with parents, and Black et al. (2018) show that first-born children experience improvements in a broad range of skills, likely as a result of receiving higher quality parent-child

[4] We take several precautions to avoid bias in these estimates. First, to avoid mechanical correlations between treatment effects estimated in the same sample, we use a split-sample approach where we estimate skill treatment effects in one sample and labor-market treatment effects using another sample. Second, we avoid small sample bias in estimates of correlations by using a large number of subgroups. Larger numbers of subgroups also avoids upward bias in the correlation that could occur when the same general groups of people might benefit in terms of both skills and long-term outcomes even when the actual individuals who benefit in these two domains are different. Third, we mitigate potential attenuation bias from estimation error by adjusting the skill treatment effects using empirical Bayes. Fourth, we show that this pattern of results is not explained by attenuation bias related to differential reliabilities in how skills are measured; sibling correlations in skills suggest that, if anything, academic skills are measured more reliably than social competence.

[5] A large literature in child development and economics finds that socialization in the home – parenting – is enormously important in shaping childrens' development of behavior and personality (Baumrind, 1967; Grolnick and Ryan, 1989; Darling and Steinberg, 1993; Kochanska, 1993; Coleman and Karraker, 1998; Aunola et al., 2000; Aunola and Nurmi, 2005; Pomerantz et al., 2005; Black et al., 2018).

interactions than their younger siblings. We replicate the finding from Black et al. (2018) in our data, and then show that access to public childcare lowers the advantage in social competence of first borns by about one quarter, but has little effect on visual-spatial or academic skills. These results support the theory that the some of the heterogeneity in the effects of public childcare may result from substitution of parent-child interactions with public childcare. Further, our results by family income suggest that public childcare benefits children from disadvantaged families more than those from more affluent families (see also Duncan et al., 2010). And, results from our machine learning predictions of treatment effect heterogeneity – highlighting the potential importance of public childcare for the children of mothers likely to otherwise work – also corroborate the argument that factors beyond family income but linked to early life socialization predict treatment effect heterogeneity. Additionally, while we lack data on the counterfactual mode of care, in our context, access to public childcare increases maternal labor market participation substantially, consistent with the idea that public childcare may substitute for home-care (Mäkinen and Silliman, 2022).

The results from this study extend the existing literature on the effects of childcare in several important ways.

First, our study provides some of the first large-scale evidence that public childcare can shift long-term measures of social skills. The existing literatures in economics and psychology provide a handful of estimates of the effects of childcare on detailed measures of socio-emotional skills measured in childhood (Weiland and Yoshikawa, 2013; Drange and Havnes, 2019; Ichino et al., 2019; Cappelen et al., 2020). In adulthood, treatment effects on socio-emotional skills have been proxied by behavioral outcomes such as dropout, educational choice, teenage pregnancy, and crime (Deming, 2009; Heckman et al., 2013; Sorrenti et al., 2020). Our study examines the effects of childcare on detailed measures of adult social and emotional skills measured at age nineteen for nearly full Finnish male cohorts.[6] Our results suggest that access to public childcare affects skills across a range of domains, particularly those linked to social competence, and that these skills persist through adulthood.

Perhaps most importantly, we provide an empirical test for the relationship between effects on these measures of adult skills and effects on long-term outcomes such as income and years of education. Our main results suggest that the effects of public childcare on long-term outcomes are most strongly correlated with adult measures of skills linked to social competence, compared to those linked to school readiness (academic skills) or raw intelligence (visual-spatial skills). These results lend empirical support for the hypothesis in the economics literature that behavioral or socio-emotional skills drive the effects of childhood programs and long-term outcomes (Deming, 2009; Heckman et al., 2013; Bailey et al., 2017). These results also contribute to the literature in psy-

[6] While these measures were not developed for academic study, they were carefully constructed by psychologists to help the military make decisions on issues like promotions (Jokela et al., 2017).

chology, providing evidence that shifts in social competence – a central concept in early childhood development (Waters and Sroufe, 1983; Santos et al., 2014) – can lead to shifts in adult outcomes in various domains, including education and employment. For practitioners, these results underline the importance of activities relating to the development of social skills as well as an environment with affirmation and warmth (Burchinal and Farran, 2020).

Further, our results help shed light on why childcare might affect children from different types of families in different ways. Prior work documents treatment effect heterogeneity by family income (Fitzpatrick, 2008; Cascio and Schanzenbach, 2013; Havnes and Mogstad, 2015) and ability (Bitler et al., 2014; Drange and Havnes, 2019), though results from Cornelissen et al. (2018) suggest that other characteristics (unobserved in their context) may be important in shaping treatment effect heterogeneity.[7] Our pattern of results by family income mirror those from Havnes and Mogstad (2015). Further, we extend this work and show that by shifting childhood socialization, public childcare access may mitigate some of the inequalities in child development linked to parental attention and birth order (Black et al., 2018). Additionally, the results from our predicted treatment effect heterogeneity excercise – highlighting potential benefits of childcare for children of working mothers and those without older siblings – corroborate the idea that children without high-quality socialization in their home environments may benefit from public childcare access most. These empirical results complement theory from psychology highlighting the socializing role of the early childhood environment (Black et al., 2017) and the importance of high quality interactions between young children and adults (Clarke-Stewart et al., 1994; Csibra and Gergely, 2009).

Together, the results in this paper provide new evidence on the effects of national public childcare programs. As inequality in educational and labor market outcomes grows, policy-makers are increasingly looking to public childcare as a promising tool to help improve well-being, to boost economic outcomes, and to reduce inequalities (European Union, 2019; Biden, 2021). Nonetheless, the potential for negative effects of childcare was noted already decades ago by psychologists (Belsky and Steinberg, 1978; Belsky, 2001; Maccoby and Lewis, 2003), and empirical evidence on the effects of national childcare programs remains mixed. While evaluations of early Head Start cohorts and some other state-level programs in the United States find considerable benefits (Gormley Jr et al., 2005; Ludwig and Miller, 2007; Deming, 2009; Gibbs et al., 2011; Carneiro and Ginja, 2014), later cohorts appear to experience zero or even negative effects of attending Head Start (Pages et al., 2019). Evidence from Denmark, Canada, Germany, Italy, and Norway also suggest mixed or negative effects of public childcare, often with children from some families benefiting while those from other families lose out (Gupta and Simonsen, 2010; Havnes and Mogstad, 2015; Kottelenberg and Lehrer, 2017; Ichino et al., 2019; Cornelissen et al., 2018).[8] Our results suggest

[7]Outside the context of public childcare, see also Nicoletti et al. (2020), who find that mothers' hours worked in pre-school years have negative effects on learning.

[8]At the state level, see Durkin et al. (2022) find substantial negative effects on academic achievement and behavior

that although Finland's 1970's public childcare program generated small average effects, it leveled the playing field significantly, reducing the family-son rank-rank income correlation by 5 points.[9] While childcare access benefited the children of poorer families, some other children even experienced negative effects of the policy – likely because of the high-quality care they may have received in their home environments absent the policy. These results suggest that in order for public childcare to not only level the playing field but benefit all children, policies should focus on improving the quality of public childcare so that it is better than the counterfactual modes of care.

After dicussing the history and institutions of public childcare in Finland (Section 1), we outline the data and measures we use (Section 2), detail the empirical approach we take and report the reduced form results (Section 3), and delve into the potential mechanisms behind these results (Section 4). We conclude with a short overview of our findings, pointing to areas for future work.

1 Institutional context

The seeds of the first public childcare services in Finland took root in 1919 under the auspices of social services, and by the 1920's and 1930's the first laws formalizing the government role came into place. In 1922, the *Poverty-care Law* (Law 145/1922) provided a legal basis for national support for childcare–but primarily for those with special needs or disabilities (Alila et al., 2014).[10] Still focused on children with special needs and disabilities, the *National Childcare Funding Law* of 1927 (Law 296/1927) provided government funding to individual childcare centers through application on the basis of demonstrated need. And, in 1936, the *Child-protection Law* (Law 80/1936) stipulated that municipalities must make efforts to supply childcare or support private childcare provision for children growing up in poverty or in unsafe home environments. Despite much deliberation and attempts to expand childcare to a universal right, due to political gridlock there were no significant changes to childcare in Finland for nearly forty years. In this period, some urban municipalities had developed public childcare infrastructure, but families outside urban areas had little access to childcare.

through grade six. It is also important to note that in sharp contrast to results from national studies, evaluations of both small-scale high-intensity programs such as Perry Preschool (e.g. Heckman et al., 2010b) as well as broader city-level programs (Gray-Lobe et al., 2021) document large improvements in a range of outcomes such as educational attainment, crime, and earnings. In fact, these studies have found childcare interventions to be one of the most effective ways to promote economic outcomes in adulthood (Hendren and Sprung-Keyser, 2020). See also Baker (2011), Elango et al. (2016), and Duncan et al. (2022) for overviews of prior work on public childcare.

[9] This is comprable in magnitude to the effects of the Finnish comprehensive school reform on intergenerational mobility (Pekkarinen et al., 2009).

[10] Alila et al. (2014) describe that this law provided support primarily for children that were mentally disabled, blind, deaf, or physically disabled.

The foundation for the public childcare system in place in Finland today was laid by the *Childcare Law* of 1973 (Law 36/1973). Finally, after being in the works for nearly a quarter of a century (Alila et al., 2014), in 1972 a proposal for a law concerning childcare was presented in parliament.[11] These legal proceedings emphasize the urgency of public support for childcare highlighting the "variability in quality and uneven geographic distribution of childcare" (Valtiopaivat 1972).[12] Commentary from the time period suggests the increasing labor market participation of women was also an important factor behind the newfound support for public childcare (Hulkko, 1971).

The law was implemented quickly and on April 1st, 1973 the *Childcare Law* (Law 36/1973) made the provision of childcare a universal right, unified concepts surrounding childcare, and provided a transparent and simple mechanism for the government funding of childcare. In order to incentivize municipalities to provide high quality care, government funding was designed to cover up to 80% of the costs, depending on the municipality's ability to pay. To supply the estimated 100,000-120,000 spots demanded, given the time required to train qualified staff, the government intended to grow childcare coverage by 5,000 annually until the year 1990 (Valtiopaivat 1972). To facilitate this process, the government agreed to help with the annual funding of childcare, and the fixed costs associated with establishing a daycare center.

As shown in Figure 1a, below, the number of government funded daycare spots grows at roughly a pace of the planned rate of 5,000 a year after 1973. Prior to 1973 childcare was provided primarily by a patchwork of informal and private services, and was almost entirely unavailable in rural municipalities. Given that this is where the expansion of childcare grew most, rural municipalities are the focus of this paper (Figure 1b).

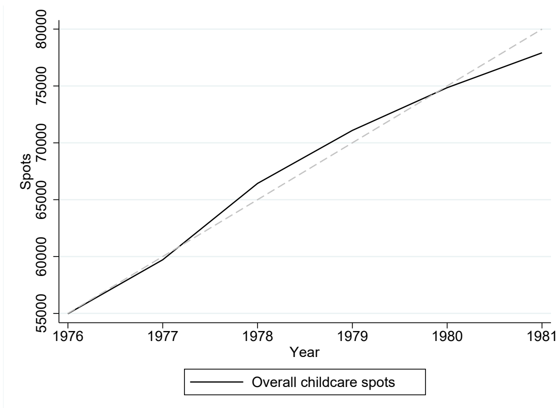
Some of these municipalities had both half and full day care available, and some childcare centers also provided free lunch. Despite its roots in social services, the potential importance of childcare for child development was acknowledged already in this period: a publication from the Finnish Population and Family Welfare League argues that "the work implies participation in productive activity, since it constitutes the production of coming labor power" (Hulkko, 1971). Although the concepts surrounding quality in early childhood education and care directly after 1973 were still developing, maximum group-size limits were in place to ensure that childcare centers were sufficiently staffed. However, the services these childcare centers provided in these early years was likely of considerably inferior quality compared to modern childcare. And only in 1983, after the next wave of childcare reform in Finland, did the role of childcare began to more formally shift

[11] Advocates of the new law cited demographic and cultural changes that resulted in the demand for childcare had far outstripped the supply: "*Increasing industrialization and broad economic development have offered employment opportunities which have lead to rapid urbanisation, particularly amongst young families and their children. In tandem, the employment rates of the mothers of young children have increased. The economic and demographic changes, as well as the increased time spent in education, have increased the demand for childcare*" (Valtiopaivat 1972).

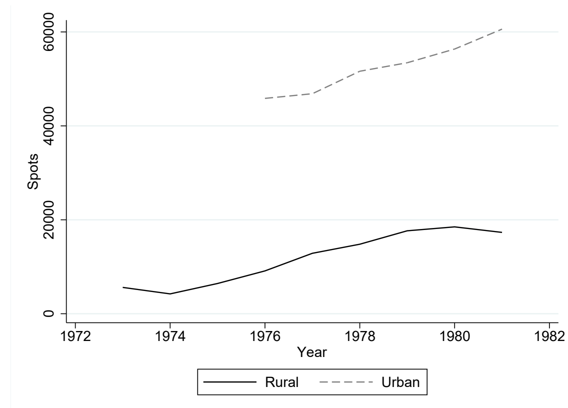
[12] The shift from needs-based provision of childcare to universal access fits in with a broader shift towards universalizing policies that have come to define the Finnish welfare state (Esping-Andersen, 1990).

Figure 1: The Expansion of Public childcare, 1976-1981

(a) New childcare spots, 1976-1981



(b) Childcare spots 1973-1981, urban and rural municipalities



Notes: These figures show data on the growth in public childcare spots following the *Childcare Law* of 1973. Figure (a) shows that the annual increase in childcare spots in the data corresponds to almost exactly 5,000 spots annually (scenario in gray) – the target number in the parliamentary proceedings from 1972. These years (1976-1981) are the only years that public childcare data is available for all municpalities (urban and rural). Figure (b) shows the annual number of public childcare spots by urban and rural status. Prior to 1973 there was almost no public childcare available in rural areas. This is set of municipalities is the focus of our paper.

from social care to child development and education (Alila et al., 2014).[13]

Today, the effects of public childcare access in Finland remains hotly debated by academics and policy-makers (Erola, 2018; Erola et al., 2020). This debate emerged after a pair of papers found participation in public childcare to be associated with positive outcomes (Karhula et al., 2017; Hiilamo et al., 2018) while another paper found there to be no association between learning outcomes and childcare participation (Saarinen et al., 2019). However, as one of the authors of these studies themselves notes, a potential reason for the discrepancies in these results is that these studies lack an experimental or quasi-experimental setup (Erola, 2018). While this paper may help to inform the debate on public childcare, it is important to remember that our context is the 1970's. Still, by studying how the effects of public childcare operate, this paper may prove helpful in ensuring quality in today's childcare provision.

2 Data, concepts, and descriptive statistics

2.1 Data sources and outcomes

We link together various sources of data for cohorts born between 1962 and 1976. This allows us to include several cohorts aged three to six – the initial ages for childcare eligibility – before and after the *Childcare Law of 1973* was passed.

Childcare data. We begin with municipal-level data on public childcare. After the passing of the *Childcare Law of 1973*, data on childcare provision was collected annually by the research and planning division of the Association for Finnish Municipalities and reported in their annual reports on social spending and services for the years 1973-1981 (Association for Finnish Municipalities, 1974; 1975; 1976; 1977; 1978; 1979; 1980; 1981; 1982).[14] We located these reports at the archives of Statistics Finland, and transcribed data from them manually over a period of several weeks. These reports include statistics on the number of spots for children three to six years old in municipal childcare centers. Since the administrative unit in the early seventies was different for municipalities classified as urban and rural, these data do not include urban municipalities for the years 1973-1975 (See Figure 1b).

Background characteristics. We link this municipality data to individual data from Statistics Finland's FOLK database (from Statistics Finland, 2021c) detailing each individual's gender as well as their year and municipality of birth. We then merge this data to a register containing parent-child links to identify the fathers and mothers of all individuals, and create measures of family composition (Statistics Finland, 2021c). Population-wide censuses from 1970-1985 contain data on parental education and income (Statistics Finland, 2021b). We form measures of family income rank based on cohorts from their childrens' birth year based on the full (not estimation) sample.

Measures of adult skills. The Finnish Defense Forces collect measures on various dimensions of skills for everyone entering the military (Finnish Defence Forces, 2021). Due to national conscription for all male citizens, these measures—collected at age nineteen—are available for upwards of eighty percent of males from the cohorts we study. Our binary measure – "Military service" – measures whether such skill data exists for each individual. These data were collected upon con-

[13] Alila et al. (2014) explain that after its birth, the next major period of childcare reform took place between the years 1984-1996. During these years, childcare became a subjective right, first for children under the age of three (1990), and then for all children not yet in school (1996) (Alila et al., 2014, pg. 13). Further securing its position as a universal right integral to the operation of the Finnish welfare state, the legal basis for both home-care and private-care became linked to the *Childcare Law* of 1990.

[14] After the year 1981 the statistics are no longer reported in a consistent format that would allow for year to year comparisons.

scription using testing instruments designed by pscyhologists that remained the same for all cohorts we study. This test includes three dimensions of cognitive skills—arithmetic, verbal reasoning, and visual-spatial skills—as well as several dimensions of socio-emotional skills including activity energy, achievement striving, deliberation, dutifulness, leadership motivation, self-confidence, and sociability. We standardize all test scores to have a mean of zero and a standard deviation of one for the cohort born in 1967, anchoring all other cohorts to this year. See Appendix Section 3 for more details Nyman et al. (2007) or Jokela et al. (2017) for an extensive overview of this data. We report results for each of these outcomes in the appendix of the paper, but focus on a set of constructs motivated by the literature on child development and economics.

Educational and labor market outcomes. National degree registries (Statistics Finland, 2021a) provide us information on educational attainment for everyone in our sample. We construct simple binary measures of secondary school dropout, upper-secondary general track graduation, upper-secondary vocational certification, and tertiary completion. We also aggregate these measures to measure years of education. We use the FOLK databases to then generate annual measures of cohort income rank and employment. To measure income, we form a measure of mean cohort income rank (ranging from 0-1) of incomes between the ages of 35 and 40 – typically a good proxy for lifetime income (Bhuller et al., 2017). To measure employment, we take the mean years of employment between thirty and forty (0-10). We create a measure for whether each person in our data is observed married at any point by the time they reach forty.

Occupational task shares. We follow Silliman and Virtanen (2022), linking occupational task share measures from Acemoglu and Autor (2011) to four-digit ISCO occupational codes measured between the ages of 35-40. These allow us to measure the extent to which individuals in our study end up in jobs using cognitive and social skills.

2.2 Concepts and measurement

We organize our paper to test hypotheses from the literature on child development in economics and psychology.

Economists have argued that early childhood programs shape long term outcomes primary through social – as opposed to cognitive – skills (Deming, 2009; Chetty et al., 2011; Heckman et al., 2013). Empirical studies report a pattern of results where childcare programs have positive effects on early measures of both learning outcomes and behavioral skills, no effects on later measures of achievement, but improve long-term economic outcomes (Heckman and Rubinstein, 2001; Gibbs et al., 2011).

Understanding how early childhood programs shape people's later behavior has also been a central goal of research in psychology. Psychologists understand a child's socialization both at home and in childcare, to play an important role in this process (Clausen, 1966; Baumrind, 1967). Waters and Sroufe (1983) argue that *social competence* – the ability to recruit personal and interpersonal resources in the context of goal achievement – is the central organizing construct of early childhood. Since then, social competence has played an important organizing role in early childhood research (Campbell et al., 2000; Denham et al., 2003; Vaughn et al., 2009). Vaughn et al. (2009) describe that social competence consists of three parts: i) behavioral and cognitive skills for successful goal achievement in social contexts; ii) the ability to discover the goals of interactive peers; iii) the understanding of a child's relative value as a preferred playmate. Gunderson et al. (2013) describe one nice example of how such skills might develop, focusing on how parental praise can lead to persistent improvents in the self-confidence and motivation of young children.

Social competence. The measures from the Finnish Defence Forces that map most closely to the concept of social competence are achievement striving, leadership motivation, and self-confidence.[15] We take the average of each child's standardized score across these measures to define their social competence. For interpretability, we standardize this measure to have a mean of zero and a standard deviation of one. This measure is intended to gauge the hypothesis from developmental psychology and economics that the social competences developed in early childhood may explain the effects of public childcare on long-term outcomes (Waters and Sroufe, 1983; Deming, 2009).

Academic skills. Similarly, we create a blanket measure of academic skills by taking the mean of each child's arithmetic and verbal scores also standardized to have a mean of zero and a standard deviation of one. These skills map closely to the concept of school readiness often discussed in the literature on early childhood educcation (Duncan et al., 2022). This skill is included to test the hypothesis that, through dynamic complementarity, public childcare may shape long-term outcomes by facilitating academic learning (Heckman et al., 2013; García et al., 2021).

Visual-spatial skills. To test the hypothesis that childcare might not affect intelligence unrelated to academic learning, we include the measure of visual-spatial skills from the Finnish Defence Forces, again standardized to have a mean of zero and standard deviation of one. This measures fluid intelligence similar to Raven's matrices. We use this measure to see if the fadeout of cognitive skills affected by childcare may be explained by the fact that the effects on fluid intelligence remain small and potentially unrelated to long term outcomes (Deming, 2009; Chetty et al., 2011; Heckman et al., 2013).

The conceptual framework for our paper is laid out more thoroughly in Appendix Section 4. We also report results for all the raw outcomes available in the Appendix.

[15]Sociability, another measure collected by the Finnish Defence Forces, measures a person's gregariousness and preference for socialization. This measure has little information on how well a person navigates social situations in the context of goal achievement. As such, it is not included in our measure of social competence. Still, we report estimates for all individual concepts separately in the Appendix.

2.3 Descriptive statistics

Merging these data together provides us with a data set covering full cohorts born in the years 1962-1976, and including information on family background, birth, educational attainment, and labor market outcomes through age forty. Additionally, the data-set includes measures of adult skills for eighty percent of the male population. Altogether, this data set spans 463 municipalities, and covers 928,500 individuals.

The analysis in this paper, described in more detail in Section 3, will be based on comparing the first set of municipalities that receive public childcare spots following the *Childcare Law of 1973* (treatment) to municipalities that only come to receive public childcare spots in later years (comparison). Since we can only estimate our full set of results for males (few women are included in the Finnish Defence Forces data), the primary sample used in our estimates only includes males in the above-mentioned set of municipalities. However, female siblings are included in our counts of siblings, and we report labor market estimates for females separately in the Appendix.

Family background. Table 1 presents the mean background characteristics of full Finnish cohorts (Column 1) and our estimation sample (Column 2). Given that military data on skill outcomes is only available for males, we also present mean characteristics separately for males in both the full and estimation sample (Columns 3 and 4). Since the expansion of public childcare took place outside of major urban areas, the estimation sample differs markedly from the full sample. Compared to the full sample, families in our estimation sample tend to be larger, poorer, and have less educated parents.

Outcomes and family backgroud. Table 2 shows how family income is related to long-term outcomes in our estimation sample. The first column presents mean outcomes for children from the poorest fifth of families in our sample, while the second column presents mean outcomes for children from the richest fifth of families in our sample. These data suggest large family-income based gaps in long-term outcomes for all outcomes in our data: children from the poorest fifth of families score 0.4 SD lower on academic achievement, and end up with incomes that rank 12 percentiles lower in the adult income rank between the ages of 35 and 40. Appendix Table 1 presents the mean outcomes for the full and estimation samples. Likely due to the exclusion of large cities from our estimation sample, individuals in our sample earn less and are slightly less educated than individuals in the full sample.

Skills and long-term outcomes. Both cognitive and socio-emotional skills measured in adulthood are strongly correlated with long-term outcomes such as income and educational attainment. Correlations between skills and income for our estimation sample are shown in Table 3.

Table 1: Estimation sample versus full sample: Family background

			Males	
	Full sample	Estimation	Full	Estimation
	(1)	(2)	(3)	(4)
Mother's education	10.50	10.23	10.52	10.24
	(2.34)	(2.12)	(2.35)	(2.12)
Father's education	10.73	10.25	10.74	10.26
	(2.57)	(2.22)	(2.58)	(2.22)
Mother's age at first birth	23.71	23.64	23.72	23.66
	(4.28)	(4.35)	(4.28)	(4.35)
Family size	2.00	2.11	1.99	2.10
	(1.04)	(1.12)	(1.04)	(1.11)
Family income percentile	49.94	43.12	50.02	43.10
	(28.95)	(27.86)	(28.98)	(27.88)
Lowest income decile	0.10	0.13	0.10	0.13
	(0.30)	(0.34)	(0.30)	(0.34)
Highest income decile	0.10	0.06	0.10	0.06
	(0.30)	(0.24)	(0.30)	(0.24)
Grandparent present	0.48	0.61	0.48	0.61
	(0.50)	(0.49)	(0.50)	(0.49)
Municipalities	463	229	463	229
Individuals	928,500	177,808	472,591	90,434

Notes: This table reports the means and standard deviations of the background characteristics for the full and estimation samples in this paper (Columns 1 and 2) and males (Columns 3 and 4).

Table 2: Gaps in outcomes between children from rich and poor families

	Poorest fifth of families (1)	Richest fifth of families (2)
	Panel A: Education, marriage, and the labor market	
Dropout	0.22	0.13
	(0.42)	(0.34)
HS graduate	0.20	0.44
	(0.40)	(0.50)
Tertiary education	0.22	0.41
	(0.42)	(0.49)
Years of education	12.11	13.17
	(2.22)	(2.52)
Income rank at age 35-40	0.47	0.59
	(0.31)	(0.31)
Years employed in 30s	7.88	8.63
	(3.19)	(2.60)
Ever married	0.54	0.63
	(0.50)	(0.48)
Military service	0.81	0.81
	(0.39)	(0.39)
	Panel B: Adult skills	
Visual-spatial	-0.21	0.20
	(1.03)	(0.96)
Academic	-0.31	0.15
	(1.02)	(1.00)
Social competence	-0.31	0.10
	(0.98)	(1.00)
Individuals with skill data	15,024	15,114
Individuals	18,133	18,156

Notes: This table presents the mean outcomes for males born to the poorest and richest fifth of families in our estimation sample.

Table 3: Correlations between skills (age 19) and adult income rank (ages 35-40)

	Income rank	Visual-spatial	Academic	Social competence
Income rank	1.000			
Visual-spatial	0.283	1.000		
Academic	0.300	0.693	1.000	
Social competence	0.256	0.365	0.422	1.000

Notes: This table is based on the estimation sample, and reports the correlations of our three primary skill outcomes with adult income rank. N= 90,434.

3 Evaluating the effects of access to public childcare

3.1 Empirical approach

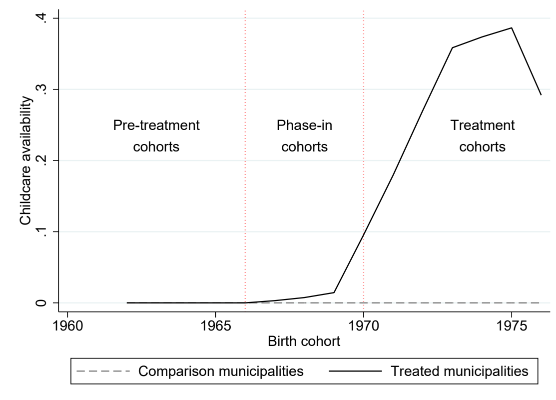
The primary challenge in estimating the effects of access to childcare on later life outcomes is that municipalities that offer access to childcare may be different from municipalities that do not (childcare investments are *endogenously determined*). For example, while urban areas tend to have much greater access to childcare than rural areas, families living in urban areas differ from rural families in numerous ways, and growing up in a densely populated city might affect a child's trajectories through life through more channels than simply access to childcare.

In this paper we focus on changes in the geography of public childcare availability in the years immediately after the *Childcare Law of 1973*, a law that passed suddenly after decades of political gridlock. As a result of the *Childcare Law of 1973,* the government provided resources to fund 5,000 new childcare spots a year, so that by the 1990's, all children in Finland would have access to public childcare—irrespective of the municipality in which they were lived in. However, for the initial years after 1973, while children growing up in some areas could access public childcare, children from similar families in other municipalities had no access to public childcare. This sudden change in the geographic availability of public childcare based on the sudden passage of the *Childcare Law* provides us with the basis for our empirical strategy. To estimate the effects of access to public childcare, we compare the adult outcomes of cohorts who are differentially exposed to public childcare access based on their cohort and municipality of birth. We consider the first municipalities to receive access to public childcare after the policy to be our treatment group and compare their outcomes to the set of municipalities that remains untreated for the entire duration we study.

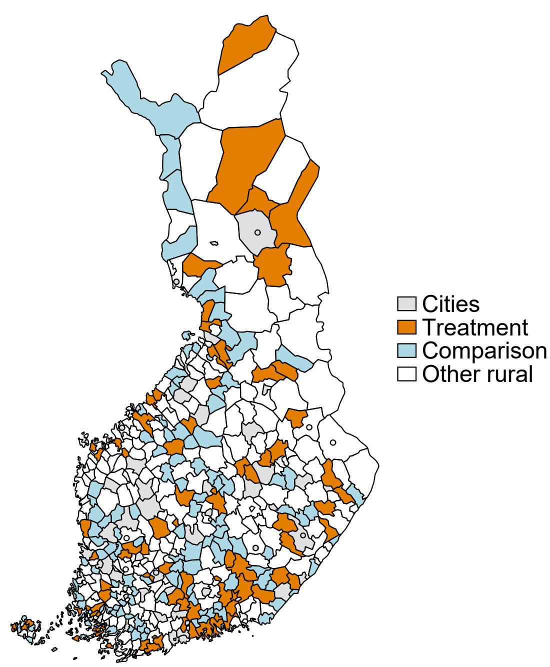
Regardless of rural municipality they were born in, members of the 1962 cohort had no access to public childcare. As a result of the 1973 *Childcare Law*, children born in the set of treatment municipalities in 1970 or later could access public childcare for the full period between the ages of

Figure 2: Childcare availability in treatment and comparison municipalities

(a) Childcare places per cohort



(b) Geography of childcare



Notes: Figure (a) reports the availability of childcare spots compared to the number of three to six year olds in birth-cohorts in treated (N=89) and comparison (N=223) municipalities. Figure (b) shows the geographic distribution of treatment (orange) and comparison (blue) municipalities. In addition to the municipalities in our estimation sample, rural municipalities that were in the process of expanding childcare amidst our period of study are shown in white and urban municipalities are shown in gray.

3-6. Those born between the years 1967-1970 might have been able to attend public childcare for at most a portion of this period (phase-in period). Figure 2a shows childcare availability by birth cohort in treatment and comparison municipalities. As shown in Figure 2b, the rollout of childcare spots does not follow simple regional geography – both treated and comparison municipalities are distributed across the country. Just a few years after the introduction of the policy, upwards of thirty-five percent of children aged 3-6 were attending public childcare in treated municipalities, while no-one was attending public childcare in comparison municipalities.

Average treatment effects. In the most simple empirical operationalization of this approach, we estimate the effects of access to public childcare using the following specification:

$$Y_{imc} = \beta(FIRST_m \times POST_c) + \delta(FIRST_m \times PHASEIN_c) + \pi_m + \gamma_c + e_i \qquad (1)$$

In the above equation, we regress individual (i) outcomes (Y) in municipality m and cohort c on an indicator variable for whether or not the municipality belonged to the first set of municipalities covered by the 1973 policy ($FIRST$), and whether the child was aged 3 years old in the period after the policy was implemented ($POST$) (cohorts are born between 1970 and 1976).[16] Since some children are already four, five or six when the policy was implemented (cohorts born between 1967 and 1969) and may have also enrolled in childcare, we remove any effect on these cohorts from our primary coefficient of interest by adding an interaction between $FIRST$ and $PHASEIN$.[17] We account for consistent differences between children born in different municipalities (π_m) and cohorts (γ_c). Following Bertrand et al. (2004), we cluster all standard errors by municipality in all our analysis.

The coefficient of interest, β, is our difference-in-differences estimate of the effects of access to public childcare on outcome Y. The first difference measures the extent to which the outcomes of post-period cohorts vary from prior cohorts within their own municipalities. The second difference measures the extent that this within municipality variation differs between treated and comparison municipalities.[18]

We also adapt the above specification to produce annual estimates of any differences in outcomes

[16] By collapsing all variation in childcare spots to a simple binary measure with a comparison group of municipalities that never gets treated in the period studied, we avoid potential bias from staggered differences-in-differenecs designs (Goodman-Bacon, 2018) and complications arising from continuous treatment measures (Callaway et al., 2021).

[17] Including these cohorts is likely to bias our TE downward, since they would have been exposed to childcare for a shorter period of time. This approach is similar to that taken by prior work on the gradual implementation of policies (Havnes and Mogstad, 2011, 2015).

[18] Relating our empirical approach to the conceptual framework from the Appendix Section 4, we can imagine the policy (the term, $FIRST_m \times POST_c$) to result in a shock to public investment in childcare (D). The parameter, β, relating outcomes to shocks to D, may reflect endogeneous changes in household provision of childcare that result from increases in public provision.

between the first set of municipalities to receive access to public childcare and our set of comparison municipalities. This "event-study" specification is estimated by the following equation:

$$Y_{imc} = \sum_{c=1962}^{1976} \beta_c \big(\mathbf{1}[c_i = c] \times FIRST_m\big) + \pi_m + \gamma_c + \epsilon_i \tag{2}$$

The term β_c measures the extent to which the outcomes between the treatment and comparison sets of municipalities differ in outcomes in each year before and after the policy, taking into account initial differences in outcomes as well as annual variation in outcomes affecting both treatment and comparison municipalities.

Internal validity. The interpretation of the coefficient of interest – β – as reflecting the causal effects of public childcare access rests on the assumption that, in the absence of public childcare, the outcomes of treated and comparison municipalities would have developed in a parallel manner. While we cannot observe what would have occurred in the absence of the policy – the identifying assumption does provide testable implications.

To probe the validity of our approach empirically, we study whether the outcomes of treatment and comparison municpalities are parallel prior to treatment (Figure 3) and examine whether changes in observable characteristics of families in treatment and comparison municipalities coincide with the introduction of public childcare (Table 4). Figure 3 suggests that there no are changes in the difference in mean income rank between treated and comparison municipalities before treatment municipalities receive access to public childcare. Appendix Figures 1-3 suggest a similar story holds when we examine educational, labor market, and skill outcomes. Additionally, Figure 3 and Appendix Figures 5-6 suggest that prior to the introduction of public childcare, trends in the outcomes in treated and untreated municipalities developed in a parallel manner – even when zooming into more granular subgroups. Further, Table 4 suggests that there is little evidence of changes in the observable characteristics of families coinciding with the introduction of public childcare access.

Table 4: Descriptive data and covariate balance

	Pre-period mean		
	Treatment	Comparison	DiD
	(1)	(2)	(3)
Mother's education	10.01	9.92	0.08
	(2.01)	(1.92)	(0.07)
Father's education	10.11	9.96	0.13
	(2.19)	(2.07)	(0.09)
Mother's age at first birth	23.92	24.02	0.03
	(4.46)	(4.60)	(0.10)
Family size	2.05	2.12	-0.02
	(1.06)	(1.13)	(0.04)
Family income percentile	44.64	38.63	-1.19 *
	(27.81)	(26.92)	(0.71)
Lowest income decile	0.09	0.12	-0.00
	(0.29)	(0.32)	(0.01)
Highest income decile	0.11	0.08	-0.01 **
	(0.31)	(0.27)	(0.01)
Grandparent present	0.61	0.70	-0.03
	(0.49)	(0.46)	(0.02)
Municipalities	89	140	223
Observations	21,581	14,752	90,434

Notes: This table reports the pre-period means and standard deviations of background characteristics for the first group of municipalities that receive public childcare after the 1973 *Childcare Law* (Column 1, Treatment) and the group of municipalities that only receive public childcare in later years (Column 2, Comparison). The difference-in-differences estimate of the difference before and after the 1973 *Childcare Law* for treated and comparison municipalities is shown in Column 3.

Treatment effect heterogeneity. Building from prior research suggesting that the effects of public childcare may vary significantly by the type of family that a child is from – for example by family income (Havnes and Mogstad, 2015), we modify our main specification to allow us to study heterogeneity in any effects of public childcare. For our main estimates of heterogeneous treatment effects we focus exclusively on heterogeneity by family income percentile and assume a linear relationship between family income percentile and the magnitude of the treatment effect. In the second half of the paper, we use this same equation to study heterogeneity predicted by our full set of background characteristics.

$$Y_{imc} = \beta_1(FIRST_m \times POST_c) + \beta_2(FIRST_m \times POST_c \times HET_i) + \quad (3)$$

$$\lambda HET_i + \delta(FIRST_m \times PHASEIN_c) + \pi_m + \gamma_c + e_i$$

The above equation is identical to Equation 1, but includes an additional term (HET_i) for a measure of heterogeneity both alone and interacted with treatment status.

To test for whether parallel trends hold for children of different types of families, we also modify our event-study model to produce cohort-specific estimates for groups of children from different types of families (g) within municipalities.

$$Y_{imc} = \sum_{g=1}^{G} \sum_{c=1962}^{1976} \beta_c \big(\mathbf{1}[g_i = g] \times \mathbf{1}[c_i = c] \times FIRST_m\big) + \pi_m + \gamma_c + \epsilon_i \tag{4}$$

Equation 3 provides us information about how access to public childcare shifted the relationship between family characteristics (family income) and childrens' outcomes. An assumption underlying this model is that there was a linear relationship between family characteristics and childrens' outcomes, and that any effects of public childcare on childrens' outcomes shifted the slope of that relationship. However, it is entirely possible that the relationship, and particularly the change in the relationship may be non-linear. Both to test for whether treatment effects are indeed linear to the measure of heterogeneity as well as to form granular heterogeneity-group (g) specific estimates of treatment effects, we also estimate our model non-parametrically:

$$Y_{imcg} = \sum_{g=1}^{n} \beta_g (FIRST_m \times POST_c \times \mathbf{1}[g_i = g]) + \sum_{g=1}^{n} (\mathbf{1}[g_i = g]) + \tag{5}$$

$$\delta(FIRST_m \times PHASEIN_c) + \pi_m + \gamma_c + e_i$$

In this model, we relax the assumption of linear treatment effect heterogeneity, and allow for unique treatment effects for each heterogeneity group (g). These granular estimates of subgroup treatment effects will also provide a key component for our analysis of the associations between treatment effects across various outcomes (ex. skills and income).

3.2 The effects of public childcare, on average and by family income

We begin by reporting our estimates of average treatment effects. Figure 3a plots the average difference in outcomes between treated and comparison municipalities by cohort (Equation 2). These event-study plots do not show any signs of a discernible change in outcomes of cohorts eligible for public childcare in treated versus comparison municipalities. This suggests that public childcare access did not shift the mean outcomes of men in cohorts eligible for childcare in the first set of municipalities exposed to the *Childcare Law of 1973*. Difference-in-differences estimates of the average treatment effects of access to public childcare suggest a similar story (Table 5, Column 3).[19] Across almost all outcome measures, these results suggest that public childcare access did little to shift the average long-term outcomes of our estimation sample. By and large, our estimates of average treatment effects are economically and statistically indistinguishable from zero. The results are similar for females for measures available in administrative data (see Appendix Table 3).

As several studies find that estimates of average effects of public childcare access mask important heterogeneity – notably by family income (Havnes and Mogstad, 2015; Kottelenberg and Lehrer, 2017; Cornelissen et al., 2018; Ichino et al., 2019), we examine any potential effects across the family income distribution (Figure 3b and Table 5). The results from parametric estimates following equation 3 suggest that access to public childcare levels the playing field, reducing the association between family income and childrens' later outcomes (Table 5, Panel A). Public childcare access improves the long-term outcomes of children born to the poorest fifth of families (Column 2), access to public childcare improves their long-term outcomes meaningfully, while children of the most affluent families (Column 4) experience slightly worse outcomes from access to public childcare. Children from poor families are 3.6 percentage points less likely to drop out of secondary school, 3.3 percentage points more likely to gain a higher educational degree – and, as adults (ages 35-40) their incomes place them 2.3 percentage points higher in their cohort income rank. Children from rich families experience negative effects of almost comparable magnitudes.

Given the differential effects of public childcare access by family income, the association between family and son income rank falls by 5.1 points – improving intergenerational mobility considerably. These results, similar to those reported by Havnes and Mogstad (2015) who study a similar reform in Norway, are robust to the inclusion of controls (Appendix Table 6, Columns 1-3). By adding municipality by year fixed effects to our estimating equation, we use a triple-differences strategy that compares outcomes by family income within municipalities to show that this pattern of heterogeneity is not driven by differences in characteristics between treated municipalities (Column 4).

Next, we study the effects of public childcare access on skills, measured in adulthood (age 19), upon conscription to the Finnish Defence Forces (Table 5, Panel B). As in Panel A, we see that public childcare access levels the playing field not just in terms of adult economic outcomes, but also skills – with the largest effects on social competence, and the smallest effects on visual-spatial skills.[20]

However, since skills are only measured for 80 percent of males, we need to pay particular attention to the sensitivity of these results to various tests of robustness. Reassuringly, we see that

[19] Since we do not have skill data for women, our focus is on males throughout this paper. To complement these estimates, we show estimates of administrative data outcomes for females in Appendix Table 3.

[20] The estimate for social competence is greater than the estimate for visual-spatial skills by more than twice the standard errors for either estimate.

there are small and insignificant effects on military service, suggesting that we may not expect the skill estimates to be particularly affected by any missingness in that data. Further, we show that the effects on outcomes from administrative data do not budge when we restrict the sample to males with skill data available (Appendix Table 6, Column 5). Garlick and Hyman (2021) suggest that the inclusion of covariates tends to provide efficient and valid estimates in the presence of missingness. In contrast to the effects for the administrative data outcomes, we do see the estimates of the skill effects with additional covariates shrink beyond the bounds expressed by their confidence intervals from the initial specification without covariates (Appendix Table 6, Column 3). These estimates with covariates further suggest that the magnitudes of the effects of public childcare access on social competence may be larger than the effects on visual-spatial skills. Further, we bound our estimates using Horowitz and Manski (2000) bounds (Columns 6 and 8). Although they are typically very conservative, given the negative selection to missing skill data, the bound generated when missing skill data is replaced with extremely low measures may be particularly informative in our case – since people for whom skills are missing tend to come from the bottom of the academic distribution. The estimates from this bound are similar to the skill estimates using controls (Column 6), and again suggest larger effects on social competence than on visual-spatial skills.

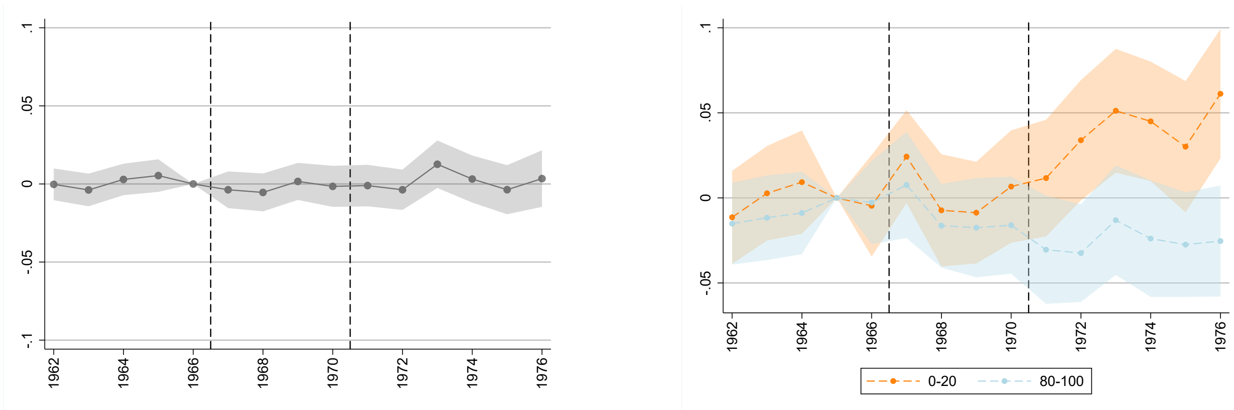
The pattern of effects on skills – emphasizing social competence rather than visual-spatial skills – provides preliminary evidence that the effects on social skills may underlie the long-term effects of public childcare on adult outcomes. If skills play a role in explaining the effects of childcare on economic outcomes, we should expect childcare to not only shift people's incomes, but also the type of work people do. To look at this, we study the effects of public childcare access on occupational tasks (Appendix Table 5). In line with our hypothesis, we see that children from poor families with access to public childcare are more likely to work in occupations with tasks requiring more non-routine cognitive analytic skills as well as cognitive personal skills.

We delve more deeply into the relationship between skills and economic outcomes in the following section.

Figure 3: Event-study comparisons of adult income rank

(a) Average treatment effects

(b) Treatment effects by family income



Notes: These figures plot the estimates of treatment effects, following Equation 2 and Equation 4. The x-axis in this and all subsequent event-study figures is the birth cohort, rather than year. Corresponding graphs for other outcomes are shown in Appendix Figures 1-3.

Table 5: Descriptive data and average treatment effects

	Pre-period mean		Average treatment
	Treatment	Comparison	effect (ATE)
	(1)	(2)	(3)
	Panel A: Education, marriage, and the labor market		
Dropout	0.19	0.18	-0.016
	(0.39)	(0.39)	(0.010)
HS graduate	0.25	0.23	0.006
	(0.43)	(0.42)	(0.010)
Tertiary education	0.27	0.25	-0.009
	(0.44)	(0.43)	(0.010)
Years of education	12.40	12.33	0.008
	(2.30)	(2.20)	(0.060)
Income rank	0.46	0.44	0.003
	(0.28)	(0.28)	(0.008)
Years employed in 30's	8.08	8.16	0.013
	(2.99)	(2.91)	(0.074)
Ever married	0.59	0.59	-0.001
	(0.49)	(0.49)	(0.009)
Military service	0.81	0.82	0.011
	(0.39)	(0.38)	(0.033)
Municipalities	89	134	223
Individuals	55,730	34,704	90,434
		Panel B: Adult skills	
Visual-spatial	-0.20	-0.19	0.011
	(1.02)	(1.01)	(0.018)
Academic	-0.12	-0.08	0.015
	(1.02)	(1.01)	(0.019)
Social competence	-0.18	-0.19	-0.007
	(1.01)	(1.00)	(0.019)
Municipalities	89	134	223
Individuals	45,747	28,365	74,112

Notes: Column 1 and 2 report the means and standard deviations of all key outcome variables for cohorts who were childcare age prior to the introduction of the *Childcare Law* of 1973. Column 3 reports the average treatment effect estimates along with their standard errors following Equation 1. *= p<0.05, **=p<0.01,***<p<0.001.

Table 6: Treatment effects by family income

	Treat X family inc. percentile	Effect at 10th percentile	Effect at 50th percentile	Effect at 90th percentile
	(1)	(2)	(3)	(4)
	Panel A: Effects on education, marriage, and the labor market			
Dropout	0.051***	-0.036***	-0.016	0.004
	(0.011)	(0.010)	(0.010)	(0.013)
HS graduate	-0.102***	0.048***	0.007	-0.034**
	(0.016)	(0.010)	(0.009)	(0.013)
Tertiary education	-0.106***	0.033***	-0.009	-0.052***
	(0.015)	(0.009)	(0.009)	(0.013)
Years of education	-0.564***	0.234***	0.008	-0.217**
	(0.076)	(0.053)	(0.058)	(0.076)
Income rank	-0.051***	0.023**	0.002	-0.018
	(0.010)	(0.008)	(0.008)	(0.010)
Years employed in 30's	-0.378***	0.161	0.009	-0.142
	(0.089)	(0.082)	(0.073)	(0.080)
Ever married	-0.054***	0.020	-0.002	-0.024*
	(0.015)	(0.010)	(0.009)	(0.010)
Military service	0.022	0.003	0.012	0.020
	(0.013)	(0.034)	(0.033)	(0.033)
Municipalities	223			
Individuals	90,434			
		Panel B: Effects on skills		
Visual-spatial	-0.190***	0.088***	0.012	-0.064**
	(0.032)	(0.021)	(0.018)	(0.022)
Academic	-0.258***	0.118***	0.015	-0.088***
	(0.031)	(0.022)	(0.019)	(0.023)
Social competence	-0.269***	0.100***	-0.008	-0.116***
	(0.035)	(0.021)	(0.019)	(0.025)
Municipalities	222			
Individuals	75,996			

Notes: Column 1 of this table reports the coeffient β_2 from Equation 3. This coefficient measures the difference in effect of public childcare access between a child at the very bottom of the family income distribution compared to a child at the very top of the family income distribution. Column 2(/4) evaluates this expected treatment effect for the fifth of children from the poorest(/richest) families. Column 3 evaluates the treatment effect for families at the middle of the family income distribution. *= p<0.05, **=p<0.01,***<p<0.001.

4 How does public childcare shape long-term outcomes?

4.1 Social skills as a potential driver of long-term outcomes

Prior research has documented that early childhood programs affect short term outcomes across both academic and socio-emotional domains, that these effects disappear in the medium term, but then re-emerge in adult outcomes ranging from earnings to criminal activity (Deming, 2009; Heckman et al., 2010a; Chetty et al., 2011; Bailey et al., 2017).

A prominent explanation for this pattern of results is that these programs may have lasting effects on social skills, and that the effects on these – typically unmeasured – skills explain the long-term effects on economic outcomes (Deming, 2009; Heckman et al., 2013). A testable implication of this hypothesis is that individuals who experience effects on economic outcomes should also experience effects on the relevant socio-emotional skills.

To test this condition empirically, we study the correlations between the treatment effects of childcare on long-term outcomes and the treatment effects of childcare on skills (see Figures 4 and 5).[21] This excercise has a parallel in the literature on teacher value-added. For example, Chetty et al. (2011) study the covariance between teacher effects on test scores and teacher effects on long-term outcomes. However, this type of a treatment effect correlation cannot be readily estimated in typical reduced form contexts like our own, where only one estimate (or a handful of estimates) per outcome is produced.

To overcome this challenge, we follow an insight from Angrist et al. (2022) and study the relationships between shorter and longer-term treatment effects across subgroups (see Appendix Figure 4). We take several precautions to avoid bias as we estimate treatment effect correlations. To avoid small sample bias in estimates of correlations, we generate a large number of granular subgroups by predicted treatment effect heterogeneity from machine learning based on Chernozhukov et al.

[21] If treatment effects on social competence rather than visual-spatial skills drive the long-term effects of public childcare, we should expect treatment effects on long-term outcomes to be correlated with treatment effects on social competence, but not with those on visual-spatial skills. This strategy tests a basic assumption of mediation hypotheses – that people who experience effects on the main outcome should also experience effects on the mediator. Still, the empirical results it generates remain insufficient to show that effects on the mediator drive the effects on the outcome: effects on the outcome could be driven by any combination of both observed and unobserved mediators. Importantly, however, comparing the magnitudes of the treatment effect correlations across potential mediating hypotheses can be particularly informative if there are large differences in these coefficients, or if one goes to zero. The same approach to mediation can also be used to study the relation between moderators and treatment effects – for recent examples, see Gendron-Carrier et al. (2018) or Terrier et al. (2020). The benefits and costs of the approach contrast to parametric approaches to mediation that are used for stronger mediation arguments, but rely on stronger assumptions (see, for example, Imai et al. (2010); Heckman et al. (2013)). Although these approaches can be used to argue that effects on a mediator explain a certain proportion of treatment effects on the outcome, these approaches rely on the much stronger set of assumptions related to sequential ignorability. At its heart, this assumption requires that conditional on treatment, the mediator is independent from potential long-term outcomes. Moreover, empirical estimates of such mediation often rely on strong parametric assumptions.

(2021).[22] This process is useful because it reduces researcher degrees of freedom by tying our hands in the construction of subgroups while maximizing the variation in predicted treatment effects between subgroups. Then, to avoid upward bias caused by a mechanical correlation between treatment effects on different outcomes estimated using the same sample, we use a split-sample approach where we estimate the treatment effects on skills in one sample and the treatment effects on long-term outcomes in another. We randomize these samples several times and report the median correlation across these randomizations as our main result, and show the full distribution of split sample correlations in Appendix Figure 15. Finally, we avoid attenuation bias stemming from imprecision in the estimation of a large number of treatment effects by shrinking our skill treatment effect estimates using empirical Bayes (shrinking both skill and long-term outcome treatment effects using empirical Bayes would result in upward bias). A more formal discussion of our approach and its assumptions is found in Appendix Section 5.1.

Treatment effects on social competence are most highly correlated with those on income (r = 0.38), treatment effects on academic skills are next most correlated with those on income (r = 0.22), and effects on visual-spatial skills are least correlated (r = -0.04). The relationship between treatment effects on adult skills and years of education displays a similar pattern of results (Figure 5b). The lack of a correlation in treatment effects on visual-spatial skills and long-term outcomes rules out the possibility that these skills explain the long-term effects of childcare, while the relatively strong correlation between treatment effects on long-term outcomes and treatment effects on social skills provides evidence consistent with the idea that social skills may explain some of the long-term effects of childcare.

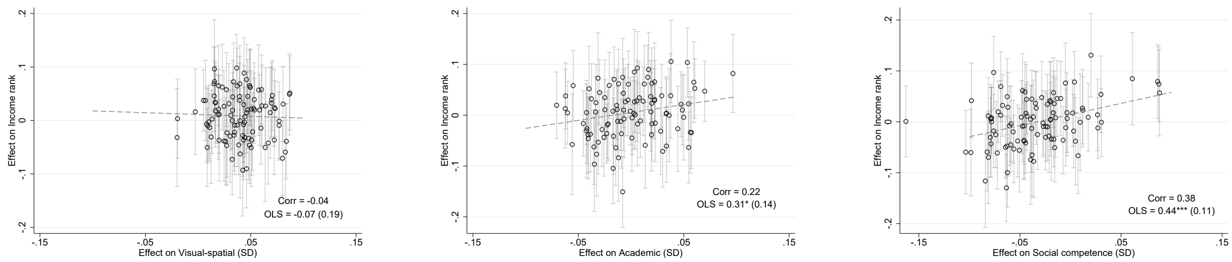
Similar treatment effect correlations for raw skills from the Finnish Defence Force data are shown in Appendix Figure 14. These corroborate the results from our composite measures, and suggest that treatment effects on income are most strongly linked to treatment effects on skills related to goal achievement in social contexts, and least related to treatment effects on cognitive skills or those related to self-regulation (deliberation) are much less so.[23]

[22]For a full discussion of how we implement this, see Appendix Section 5.2.

[23]See Duncan et al. (2022), who highlight the possibility that self-regulation skills are a candidate mechanism driving the long-term effects of childcare.

Figure 4: Treatment effects on income rank and treatment effects on measures of adult skills

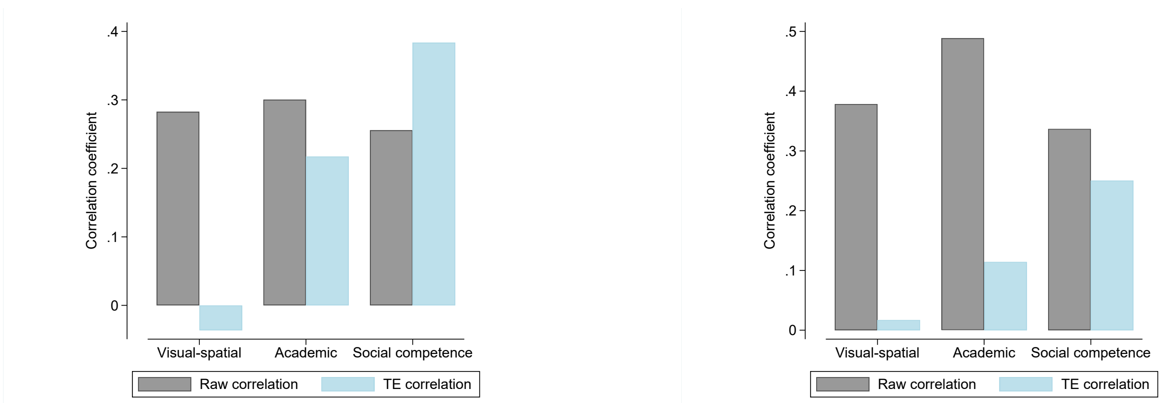
(a) (b) (c)



Notes: This figure plots the split-sample relationships between treatment effects on adult income rank (y-axis) and the treatment effects on measures of adult skills, shrunken using empirical Bayes (x-axis), for a hundred subgroups based on the predicted treatment effect heterogeneity index. The correlation and regression coefficients (along with their statistical significance) are reported in the bottom right of each figure.

Figure 5: Correlations between long-term outcome treatment effects and skill treatment effects

(a) Skills and adult income rank (b) Skills and years of education



Notes: This figure presents the correlation between treatment effects on long-term outcomes and treatment effects on measures of adult skills alongside the raw correlation between skills and long-term outcomes. This treatment effect correlation is the median split-sample estimate of the correlation between treatment effects across a hundred subgroups, with estimates of treatment effects on skill outcomes adjusted using empirical Bayes to avoid attenuation bias.

These correlations must be interpreted carefully. The existence of a correlation between skill treatment effects and long-term treatment effects is in itself an insufficient condition for causal mediation. Even if we were to imagine that all people who experience effects on income also experience effects on social skills, this would not mean that the effect on that particular skill drives the effect on the labor market outcomes. For example, long-term effects may be driven by another (unmeasured) skill or other mediating factor distinct but highly correlated with our measure of social competence. Still, the relative magnitudes of treatment effect correlations between social competence compared to other skills does suggest that, among these candidate mechanisms, social competence is the most plausable.

To study the sensitivity of these correlations to exactly how we split the data into subgroups for the estimation of treatment effects, we re-run our estimates with a range of ten to a hundred subgroups (see Appendix Figure 13). The results from this exercise suggest that while the actual correlation coefficient does appear to attenuate up through around 70 splits, the rank of the correlations across skills appears relatively stable. Further, the results from this excercise suggest that it is important to gauge the sensitivity of correlations in treatment effects across outcomes for different subgroups.[24] Since these correlations might be attenuated by measurement error in our estimates, in our main estimates of the covariance, we correct our estimates for (some of) such error using emiprical Bayes. To ensure that such shrinkage is not contributing to the qualitative nature of our findings, we report our estimates with and without empirical Bayes in Figure 13. As expected, the empirical Bayes bias-correction produces slightly higher correlations between income and skill effects, but does not change the results in a substantive way.

Even if these treatment effect correlations are estimated without bias, differential measurement error in skills may make it difficult to compare treatment effects across outcomes. Unfortunately, without access to the particular items underlying the measures from the Finnish Defence Forces, we cannot estimate the reliabilities of these measures directly. However, the Finnish Defence Forces report that the Cronbach alphas for their measures of cognitive skills range between 0.76 and 0.88, while the Cronbach alphas for their measures of socio-emotional skills range from 0.6 and 0.9 (Nyman et al., 2007) – suggesting that, if anything, the measures of socio-emotional skills may be less reliable than the measures of cognitive skills. In our data, we see that the raw correlations between these skills and long-term outcomes are in a similar range to each other, though long-term outcomes are slightly more correlated with academic skills than with social competence (left bars in Figure 5). Further, as another way to interrogate the reliability of our skill measures, we study sibling correlations in the different skill outcomes (Appendix Table 7). In the case that each skill is equally shaped by genetic and environmental factors shared by siblings, we should expect the magnitudes of the sibling correlations of skills to reflect the reliability with which each skill is measured (a form of test-retest reliability). Both of these empirical excercises suggests that the pattern we see in our treatment effect correlations is not due to social skills being measured more reliably than cognitive skills.

[24]Interestingly, for our results, the relative rank of the correlation between different mediating outcomes and long-term outcomes is relatively insensitive to the number of splits—suggesting that researchers should be more confident in using this approach to assess the relative importance of different mediating channels rather than pin-point the specific correlation between effects.

4.2 Early childhood socialization and shifts in social skills

How does public childcare shape these skills linked social competence? Theory from psychology highlights the potential socializing role of early childhood environment (Black et al., 2017) and the importance of one-on-one interactions between young children and adults (Clarke-Stewart et al., 1994; Csibra and Gergely, 2009).

While the treatment in our study – access to public childcare – is relatively constant across children from different families, the way that public childcare changes each person's socialization in early childhood varies based on the counterfactuals of the children. In a companion paper we show that public childcare access substantially increases maternal employment (Mäkinen and Silliman, 2022), which is consistent with the idea that public childcare may serve as a substitute for home-care. That said, it is also possible that public childcare access substitutes for other forms of childcare, such as private care by a nanny, care by grandparents, or informal care arrangements between neighbors. Unfortunately, we cannot observe these counterfactuals in the data.

Recognizing this potential substitution between public childcare and other forms of care, other papers have studied the differential effects of public childcare on children born to families with different levels of resources (Havnes and Mogstad, 2015). Our results are consistent with this pattern and suggest that children born to poor families may benefit from public childcare, while children born to higher income families can be hurt by public childcare. Such heterogeneity may also explain why the effects of Head Start for early cohorts may be greater than for later cohorts (Deming, 2009; Pages et al., 2019). Of course, family income is only one dimension by which children may be exposed to different environments in the absence of public childcare. In fact, results from Cornelissen et al. (2018) suggest that heterogeneity in the benefits of public childcare may follow several other – though in their case unobserved – dimensions.

To probe this hypothesis, we test for heterogeneity in the effects of public childcare by a dimension known to be tied to the quality of early life socialization – first-born status (Price, 2008; Black et al., 2018) – but not tied to between-family variation in resources.[25] Price (2008) shows that older parents spend less time with children, and that as a result first born children receive greater attention from their parents. Black et al. (2018) shows that first born chilren attain higher levels of skills, and are more likely to become leaders. First, we corroborate results from Black et al. (2018) and show that first-born male children perform significantly more highly than their siblings in terms of our measures of visual-spatial, academic, and social skills. Second, we study heterogeneity in the effects of public childcare by first-born status. Our results are in line with theory, suggesting that

[25] An advantage of looking at this specific cut of heterogeneity – compared to for example that by maternal education – is because the theory guiding it is more clear. While a large literature in psychology suggests that more educated mothers provide higher quality mother-child interactions than less educated mothers (ex. Aunola et al., 1999), maternal educational investments are also related to maternal employment, and public childcare access might affect the likelihood of working differently for higher versus lower educated mothers.

when families substitute other care arrangements such as home-care for public childcare, some of the advantage (about 25%) of first-born children disappears in terms of social competence; we are unable to detect effects here for visual-spatial or academic skills (Appendix Table 8). Perhaps this is because these skills are taught by parents with or without public childcare, because the development of social competences is a greater focus for public childcare programs, or because social competence is more sensitive to change between the ages 3-6 than are visual-spatial or academic skills.

If the change in early life socialization resulting from the substitution between public childcare access and counterfactual care options drives the heterogeneity in the treatment effects of public childcare, we should expect heterogeneity in treatment effects to also extend to further dimensions beyond just family income and first-born status. The machine learning predictions of treatment effect heterogeneity we use to divide our data into granular subgroups offer us a way to examine heterogeneity across a number of measures of family characteristics. Appendix Figure 17 and Appendix Table 12 describe this heterogeneity. In Appendix Figure 17, our sample is split into ten equal size bins based on predicted treatment effect rank are plotted along the x-axis. Those in the rightmost bins are expected to benefit the most from public childcare access, while those in the leftmost bins are expected to benefit the least from public childcare access. The extent to which the bin-mean differs from the overall mean for a variable determines the color assigned to that bin for that variable. The deepest red squares indicate that the bin-mean is greater than 0.15 standard deviations larger than the overall mean; conversely, the darkest-blue squares indicate that the bin-mean is at least 0.15 standard deviations less than the overall mean. Appendix Table 12 shows the mean values of the covariates for individuals in the bottom and top quintiles of predicted treatment effects (these correspond to the two left(/right)most bins in Figure 17).

Together, Appendix Figure 17 and Appendix Table 12 suggest that in addition to children from poor families, children with less socialization in their homes benefit most from public childcare. For example, we see children of mothers most likely to be attached to work full time (those who are older and more educated), and children with few older siblings benefit most from public childcare access. Additionally, there is some evidence that children without grandparents to care for them nearby may benefit from public childcare. Overall, this ranking of predicted treatment effects is only somewhat correlated with the one based on family income (0.15), suggesting that other characteristics may be important in determining how children respond to public childcare.

Without observing the counterfactual form of childcare, none of these heterogeneity excercises provides direct evidence on how changes in early childhood environment affect long-term outcomes. Still, each of the patterns of heterogeneity we study – by family income, first-born status, and our predictions of treatment effect heterogeneity – emphasizes different dimensions of quality in childhood interactions with adults. Together these provide evidence that is consistent with the hypothesis that public childcare shifts long-term outcomes as a result of shifting the quality of early childhood socialization.

5 Conclusion

We study the effects of the expansion of public childcare access in Finland on education, skills, and labor market outcomes — making several contributions to the existing literature. Our evaluation of the effects of the policy suggests that public childcare access benefits children from poor families, and increases intergenerational mobility, but may even hurt children from more affluent families. Moreover, we seek to understand how and why public childcare shapes childrens long-term outcomes. We show that treatment effects on social competence are strongly tied to treatment effects on adult income, while treatment effects on cognitive skills are not – suggesting that long-term effects of childcare programs on economic outcomes may be explained by lasting effects on social skills. We also present suggestive evidence consistent with the idea that access to public childcare shifts the mode of early childhood socializiation by substitutinng for home-care. Understanding how childcare programs can be designed to target specific skills linked to social competence that shift long-term outcomes remains an important area for further research.

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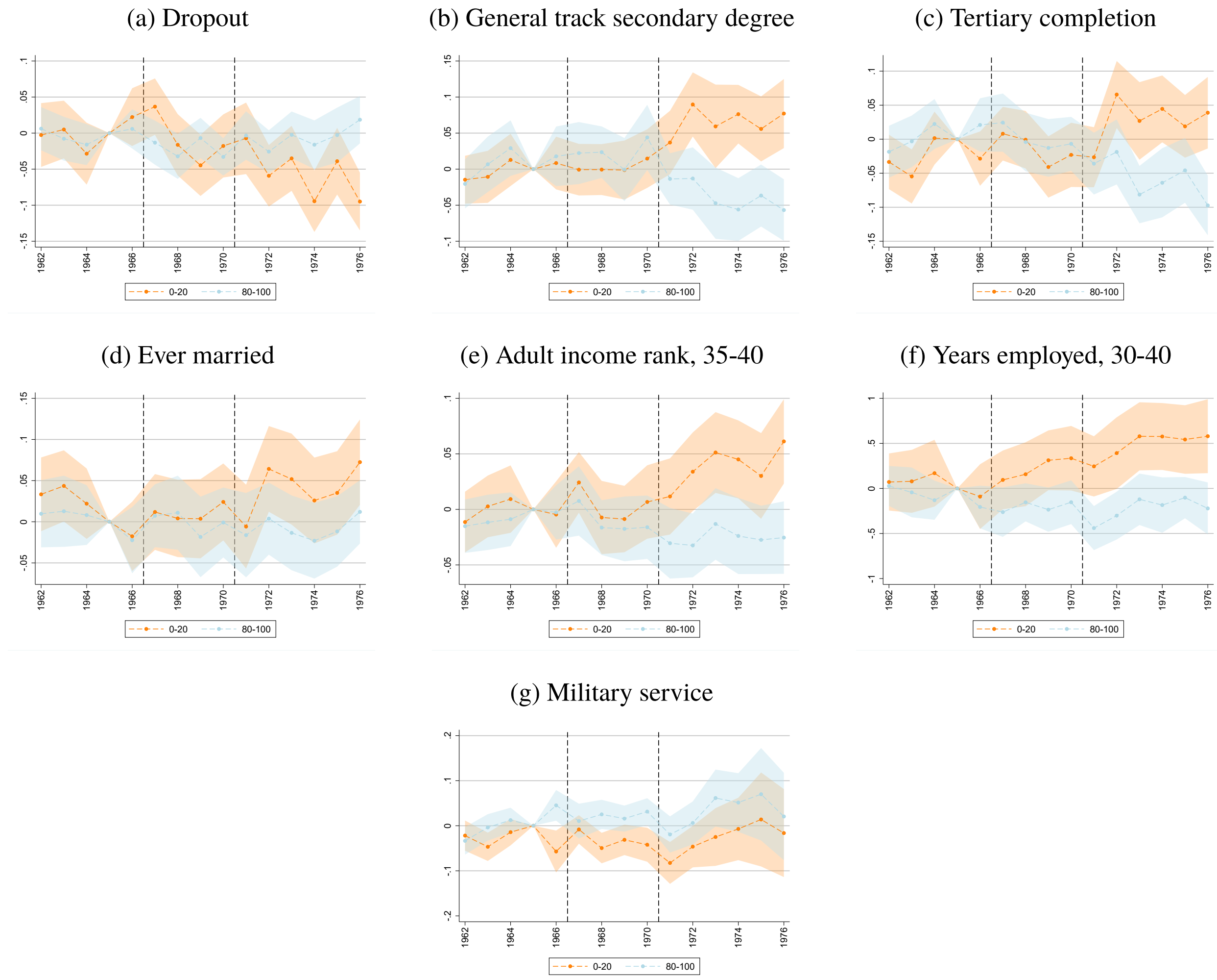
APPENDIX

1 Figures

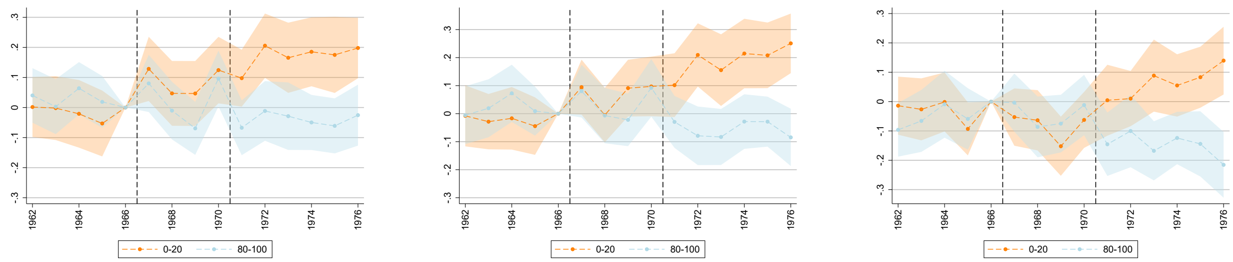
Figure 1: Event-study plots of long-run outcomes based on family income



Notes: This figure shows event-study plots for measures long-term outcomes for children from the richest and poorest fifth of families in our estimation sample using the specification from Equation 4.

Figure 2: Event-study plots of adult skills based on family income

(a) Visual-spatial skills (b) Academic skills (c) Social competence

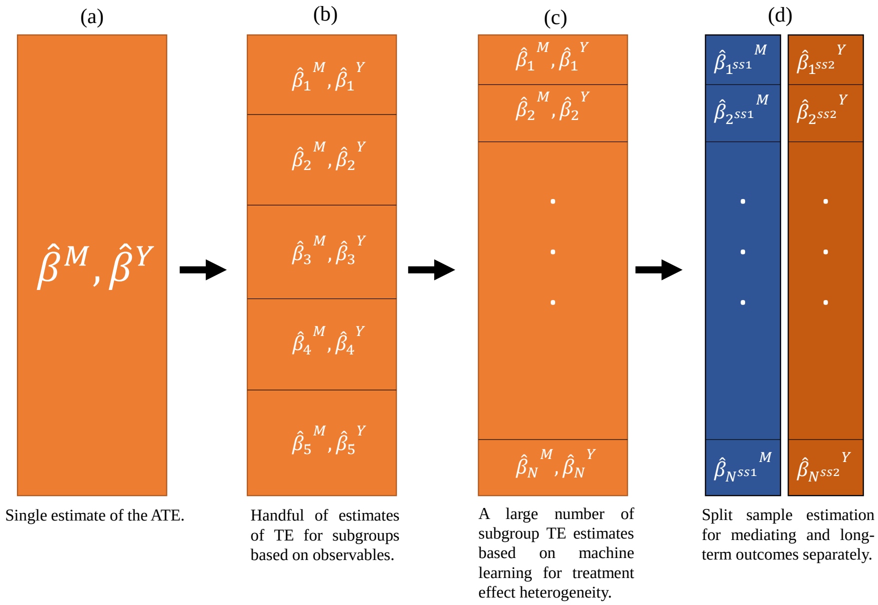


Notes: This figure shows event-study plots for measures of adult skills for children from the richest and poorest fifth of families in our estimation sample using the specification from Equation 4.

Figure 3: Event-study plots for measures of adult skills based on family income

Notes: This figure shows event-study plots for measures of adult skills for children from the richest and poorest fifth of families in our estimation sample using the specification from Equation 4.

Figure 4: Our approach to estimating the coviarance in treatment effects between medium-term and long-term outcomes



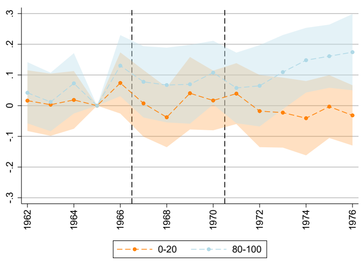
Notes: This diagram depicts the thinking behind our approach to estimating the covariance between medium-term (skills) and long-term (income, educations) treatment effects. As is common in the context of teacher effects, one way to assess the relationship between mediating variables and long-term outcomes is by estimating the covariance in their treatment effects. While insufficient for causal mediation, a relationship between the two is typically assumed for a mediation hypothesis to be true: For some mediator (M) to drive the effects of D on Y, individuals who experience effects on Y must also experience effects on M. As shown in Figure (a), in typical reduced form contexts like our own, researchers often only have one main estimate of average treatment effects for each outcome. In this case, the covariance between medium-term and long-term treatment effects cannot be estimated. One way to overcome this is by dividing the full sample of data into subgroups, and then estimating the covariance across a handful of subgroups – such as, in our case, family income quintile (Figure b). These estimates are, however, likely to be upward biased for three reasons: i) small sample bias in the estimation of the sample covariance; ii) if the same general – but not granular – groups of individuals respond in terms of both outcomes; and, iii) any bias in one estimates of one outcome is likely to exist also in estimates of the other outcome. Generating a large number of subgroups will help overcome the biases in points i) and ii) – but, when done manually, may suffer from insufficient variance in treatment effects across groups or the garden of forking paths in how groups are chosen to maximize variation in these treatment effects. One way to both tie our hands in the creation of these groups as well as maximize the predicted variation in treatment effects across groups is to use machine learning for treatment effect heterogeneity (Figure c). Still, machine learning will not alleviate the problem regarding bias being correlated across outcomes. To mitigate this concern, we divide each machine-learning based subgroup into split samples – one of which we use to estimate treatment effects on medium term outcomes, and the other of which we use to estimate treatment effects for longer-term outcomes (Figure d). Not shown in this figure: To reduce attenuation bias as we estimate an increasingly large number of treatment effects we use empirical Bayes to shrink one of these-split sample estimates (M) but not the other; to increase precision, we repeat the split-sample estimates in a number of randomly chosen splits, and then choose the median of these covariances; to check for sensitivity in our estimates of the covariance, we repeat this process for a range of numbers of subgroups, ranging from ten to a hundred.

Figure 5: Event-study plots of long-run outcomes based on predicted treatment effect heterogeneity

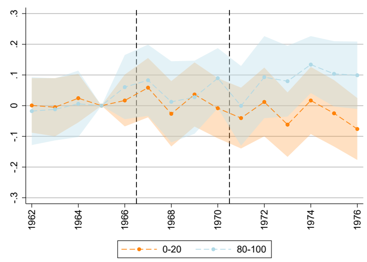
Notes: This figure shows event-study plots for measures of adult skills based on the predicted treatment effect heterogeneity ranking, using the specification from Equation 4.

Figure 6: Event-study plots for measures of adult skills based on predicted treatment effect heterogeneity

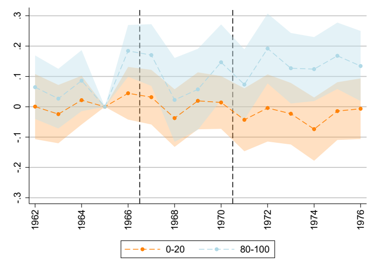
(a) Arithmetic



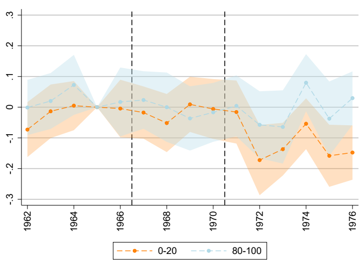
(b) Verbal



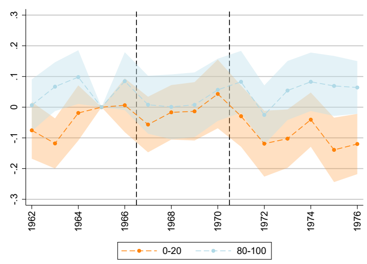
(c) Visual-spatial



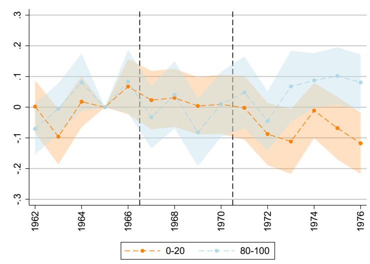
(d) Deliberation



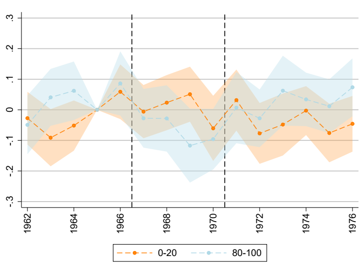
(e) Dutifulness



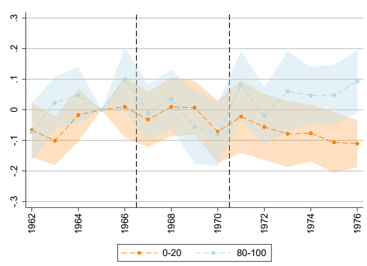
(f) Achievement-striving



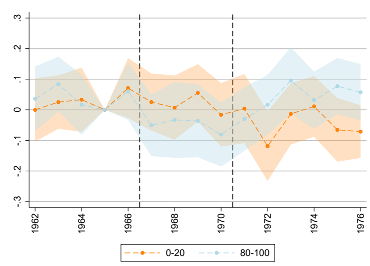
(g) Activity-energy



(h) Leadership motivation



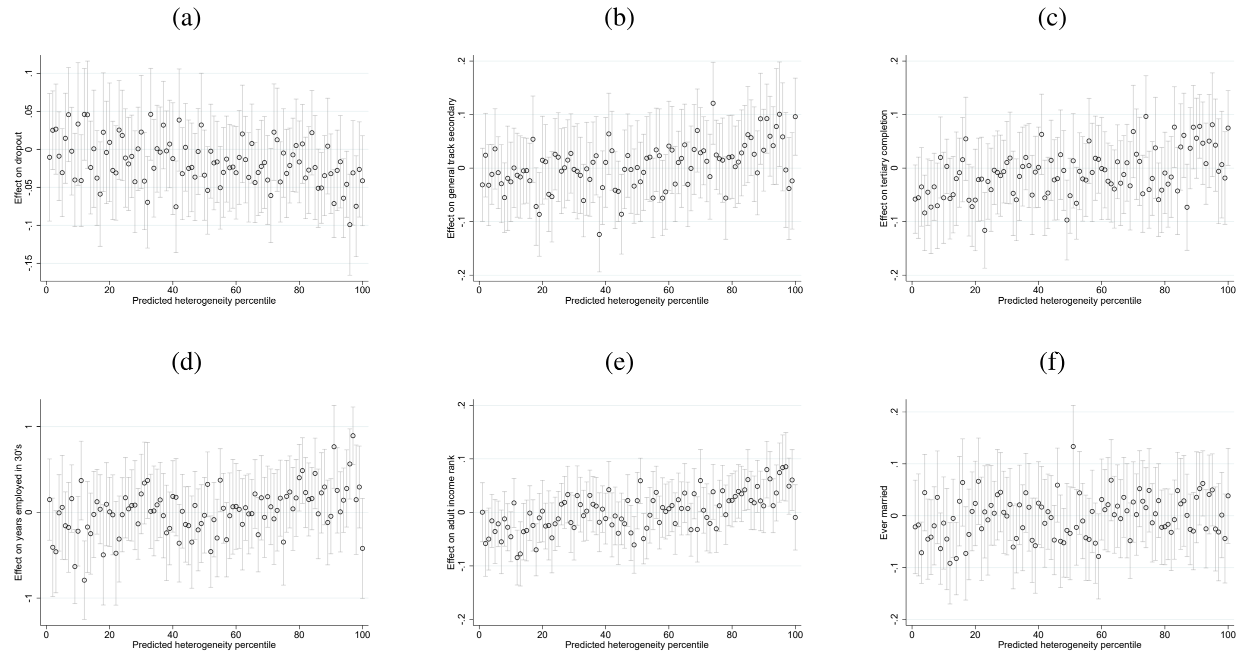
(i) Self-confidence



(j) Sociability

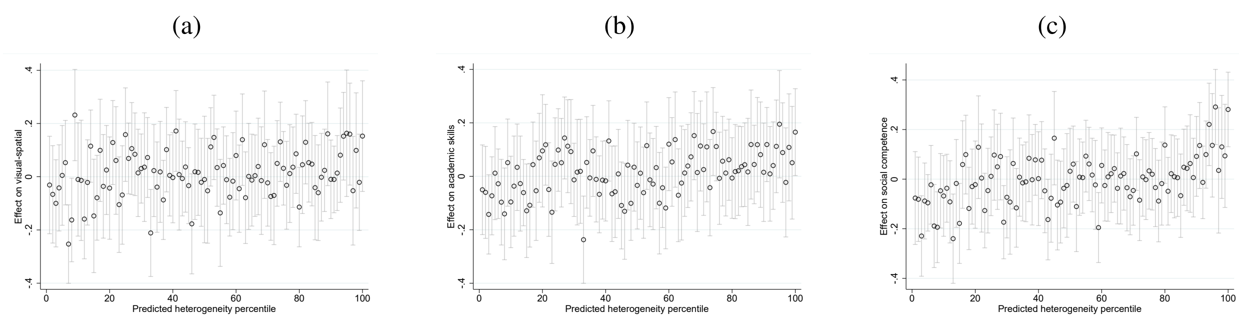
Notes: This figure shows event-study plots for measures of adult skills based on the predicted treatment effect heterogeneity ranking, using the specification from Equation 4.

Figure 7: Subgroup treatment effects on long-term outcomes



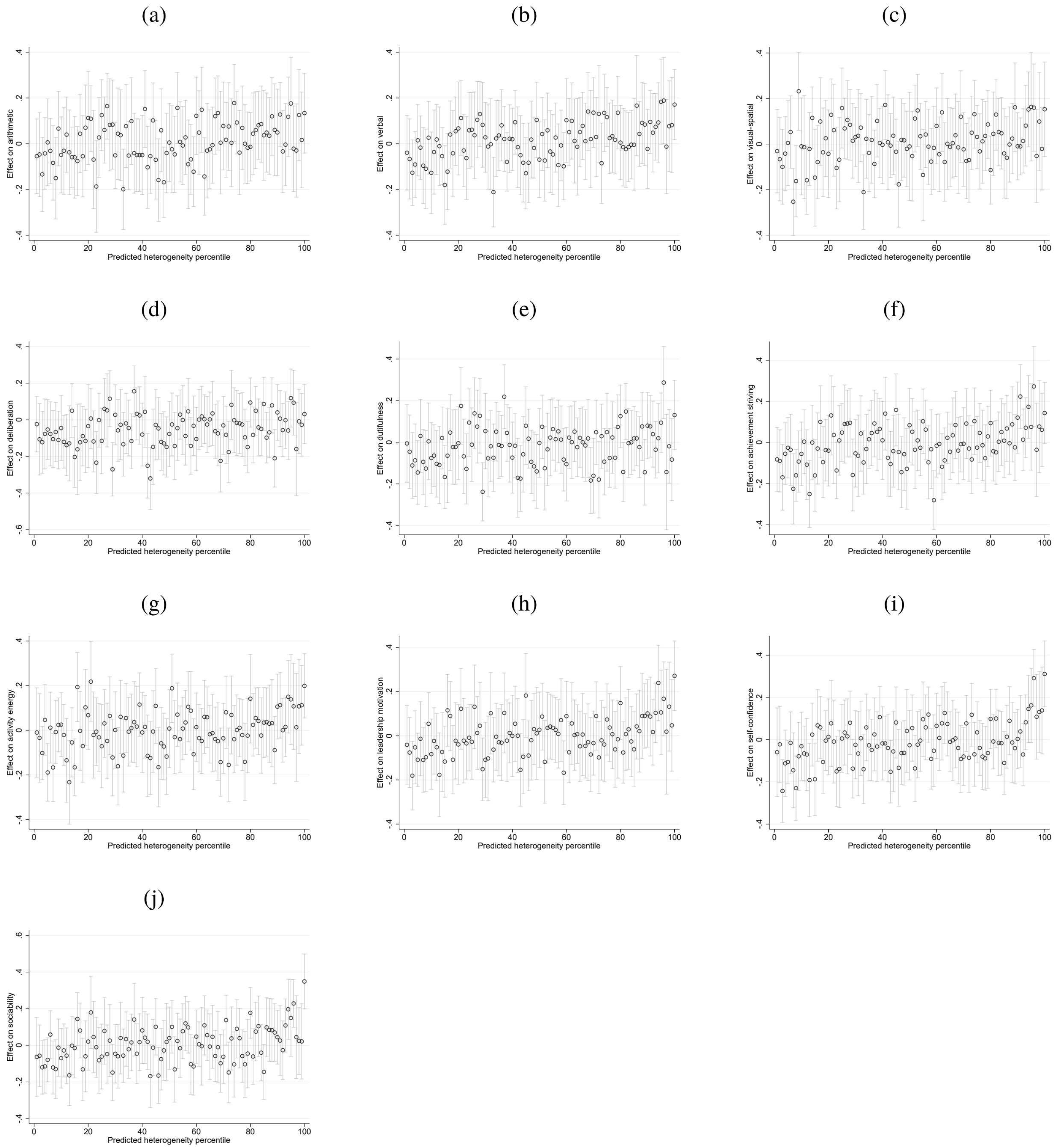
Notes: This figure plots the granular subgroup treatment effect estimates based on the predicted treatment effect heterogeneity ranking, using the specification from Equation 5.

Figure 8: Subgroup treatment effects on main skill outcomes



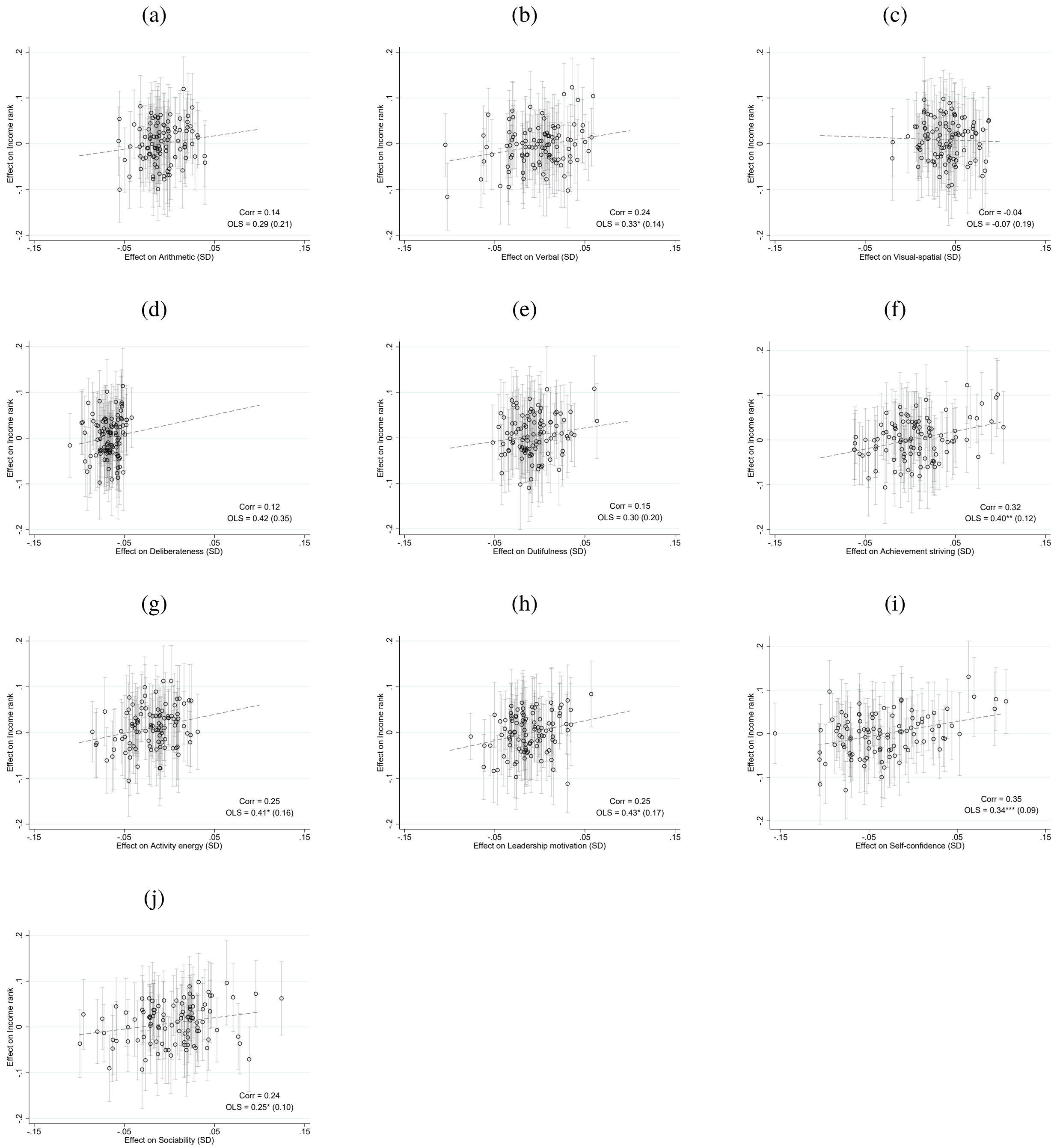
Notes: This figure plots the granular subgroup treatment effect estimates based on the predicted treatment effect heterogeneity ranking, using the specification from Equation 5.

Figure 9: Subgroup treatment effects on measures of adult skills



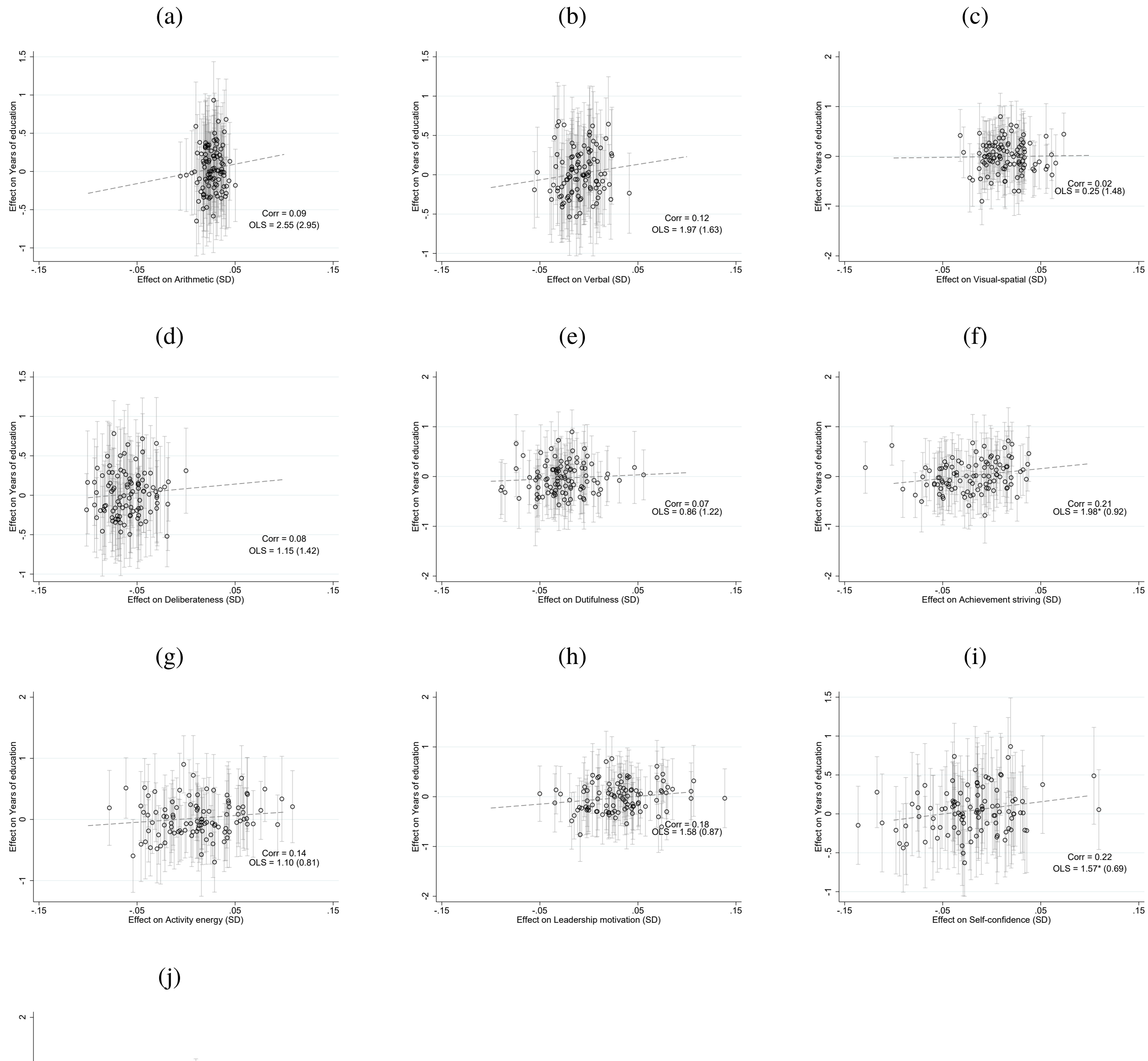
Notes: This figure plots the granular subgroup treatment effect estimates based on the predicted treatment effect heterogeneity ranking, using the specification from Equation 5.

Figure 10: Treatment effects on income rank and treatment effects on measures of adult skills



Notes: This figure plots the split-sample relationships between treatment effects on adult income rank (y-axis) and the treatment effects on measures of adult skills, shrunken using empirical Bayes (x-axis), for a hundred subgroups based on the predicted treatment effect heterogeneity index. The correlation and regression coefficients (along with their statistical significance) are reported in the bottom right of each figure.

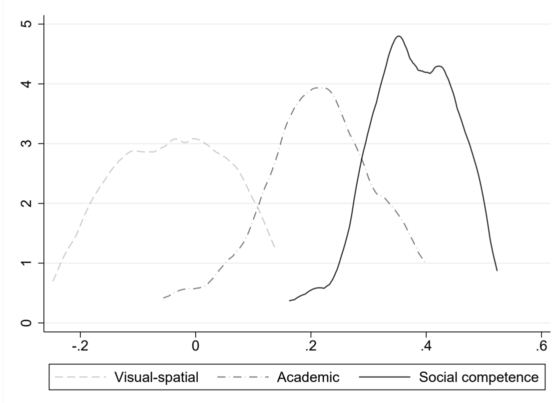
Figure 11: Treatment effects on years of education and treatment effects on measures of adult skills



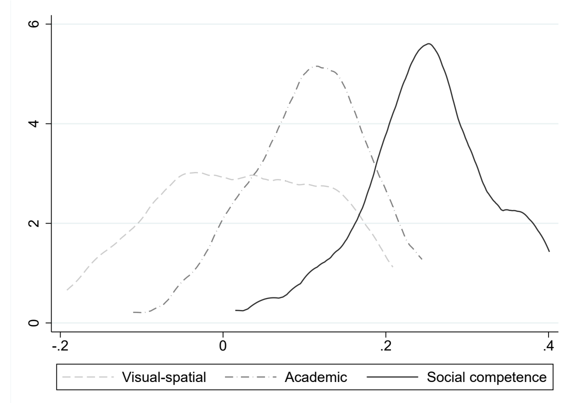
Notes: This figure plots the split-sample relationships between treatment effects on adult income rank (y-axis) and the treatment effects on measures of adult skills, shrunken using empirical Bayes (x-axis), for a hundred subgroups based on the predicted treatment effect heterogeneity index. The correlation and regression coefficients (along with their statistical significance) are reported in the bottom right of each figure.

Figure 12: Split-sample treatment effect correlations

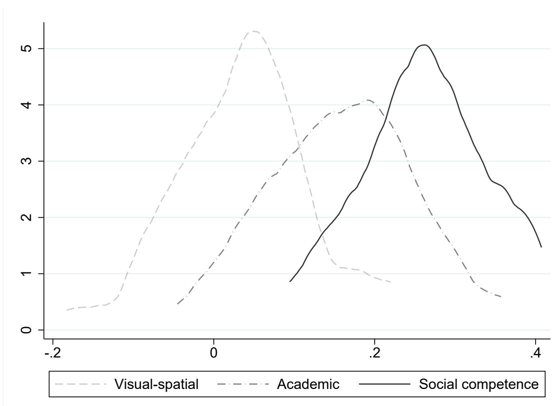
(a) Treatment effects on income (with empirical Bayes shrinkage)



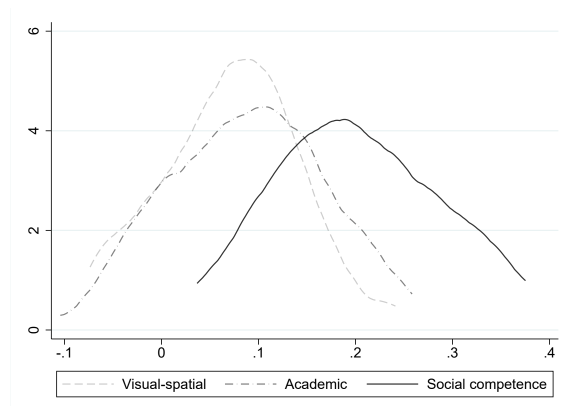
(b) Treatment effects on years of education (with empirical Bayes shrinkage)



(c) Treatment effects on income (without empirical Bayes shrinkage)



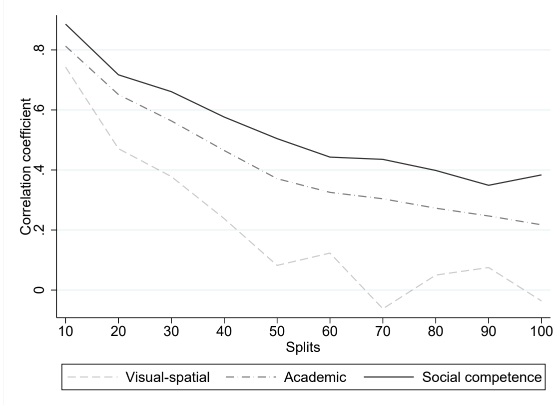
(d) Treatment effects on years of education (without empirical Bayes shrinkage)



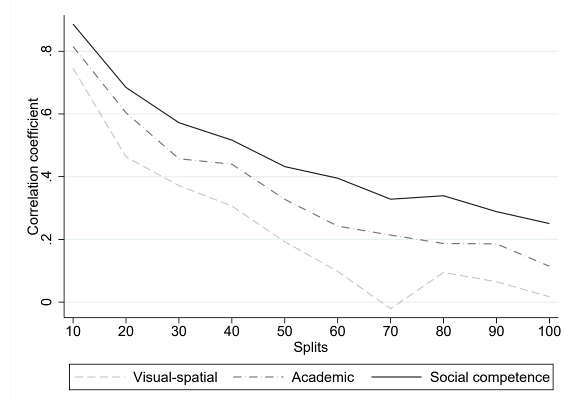
Notes: This figure plots the split-sample correlation coefficient between treatment effects on long-term outcomes and treatment effects on measures of adult skills by the number of subgroups that treatment effects are estimated for. The uppermost figures report the correlations when the estimates of treatment effects on adult skills are shrunken using empirical Bayes; the lower figures report the raw correlations. To aid interpretation, correlations with TE on measures of cognitive skills are shaded orange, while those on skills linked to conscientiousness are shaded blue, and those on skills linked to extraversion are shaded gray.

Figure 13: Sensitivity to number of splits and empirical Bayes shrinkage

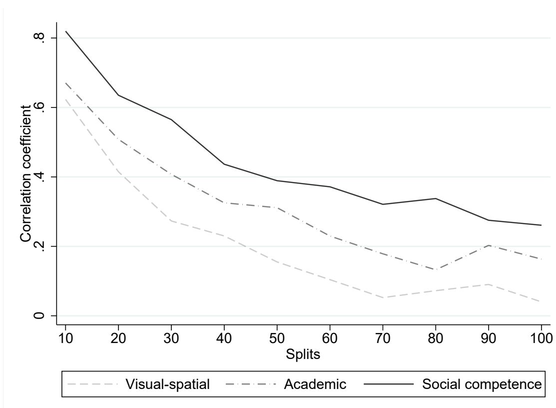
(a) Treatment effects on income (with empirical Bayes shrinkage)



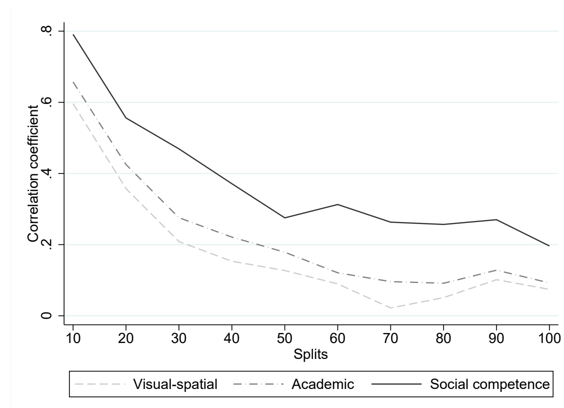
(b) Treatment effects on years of education (with empirical Bayes shrinkage)



(c) Treatment effects on income (without empirical Bayes shrinkage)



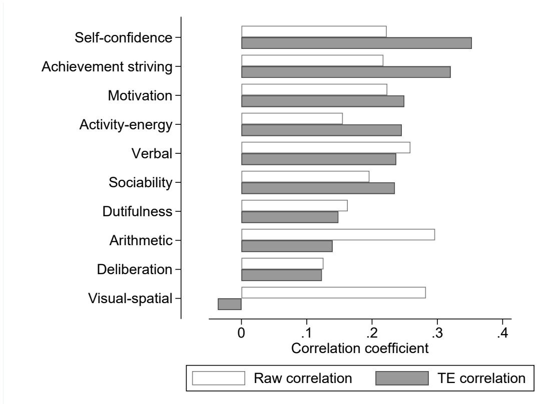
(d) Treatment effects on years of education (without empirical Bayes shrinkage)



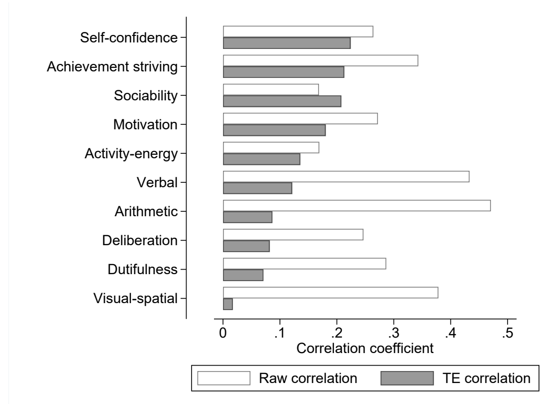
Notes: This figure plots the split-sample correlation coefficient between treatment effects on long-term outcomes and treatment effects on measures of adult skills by the number of subgroups that treatment effects are estimated for. The uppermost figures report the correlations when the estimates of treatment effects on adult skills are shrunken using empirical Bayes; the lower figures report the raw correlations. To aid interpretation, correlations with TE on measures of cognitive skills are shaded orange, while those on skills linked to conscientiousness are shaded blue, and those on skills linked to extraversion are shaded gray.

Figure 14: Correlations between long-term outcome treatment effects and skill treatment effects compared to raw correlations between outcomes and skills

(a) Adult income rank



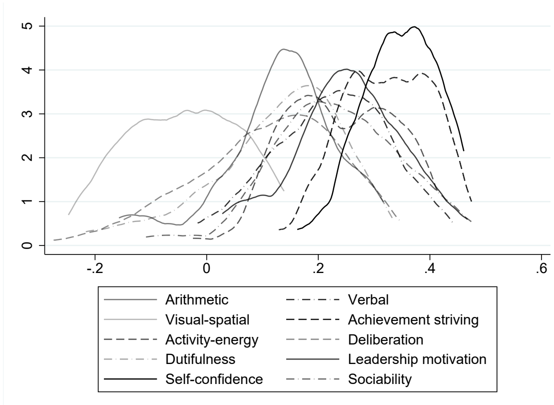
(b) Years of education



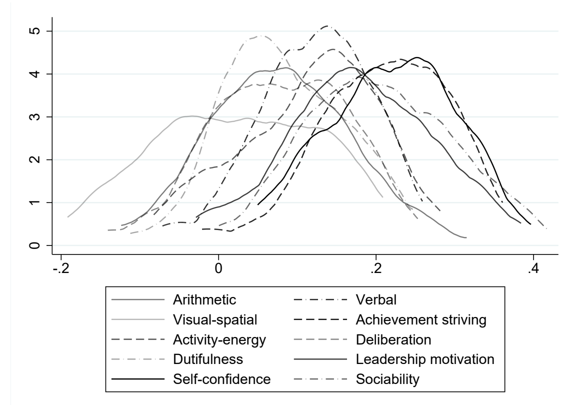
Notes: This figure plots the split-sample correlation coefficient between treatment effects on long-term outcomes (adult income rank and years of education) and treatment effects on measures of adult skills beside raw correlations of the particular skills and long-term outcomes.

Figure 15: Split-sample treatment effect correlations

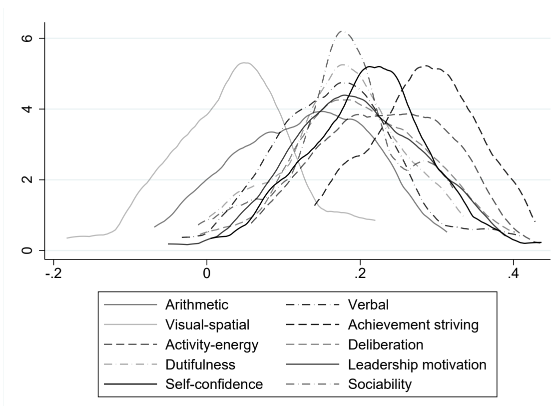
(a) Treatment effects on income (with empirical Bayes shrinkage)



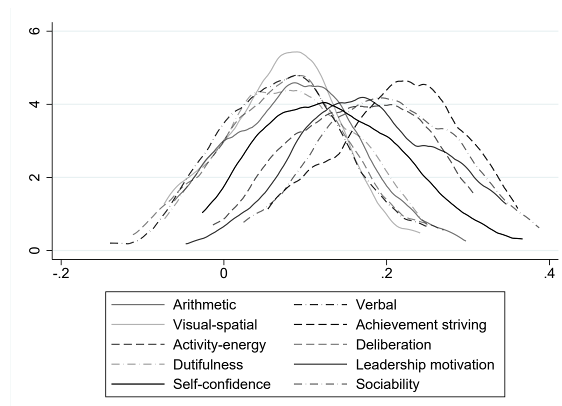
(b) Treatment effects on years of education (with empirical Bayes shrinkage)



(c) Treatment effects on income (without empirical Bayes shrinkage)



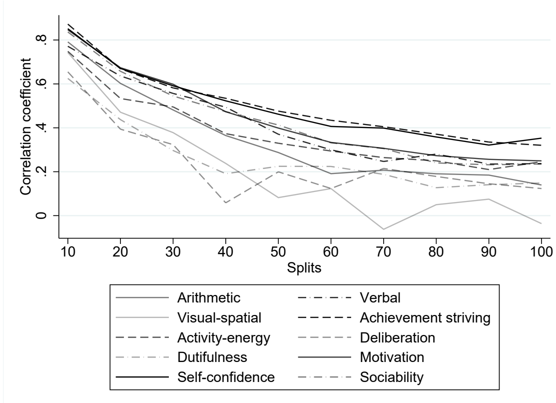
(d) Treatment effects on years of education (without empirical Bayes shrinkage)



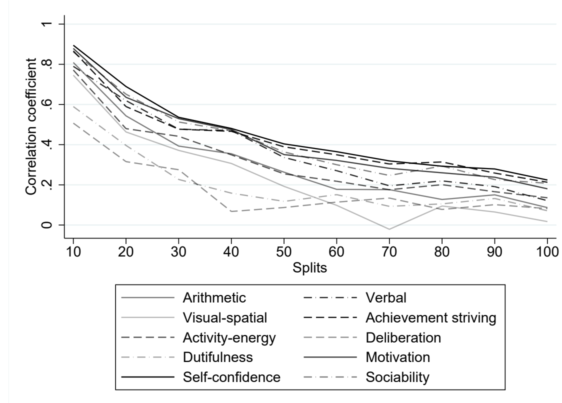
Notes: This figure plots the split-sample correlation coefficient between treatment effects on long-term outcomes and treatment effects on measures of adult skills by the number of subgroups that treatment effects are estimated for. The uppermost figures report the correlations when the estimates of treatment effects on adult skills are shrunken using empirical Bayes; the lower figures report the raw correlations. To aid interpretation, correlations with TE on measures of cognitive skills are shaded orange, while those on skills linked to conscientiousness are shaded blue, and those on skills linked to extraversion are shaded gray.

Figure 16: Sensitivity to number of splits

(a) Treatment effects on income (with empirical Bayes shrinkage)

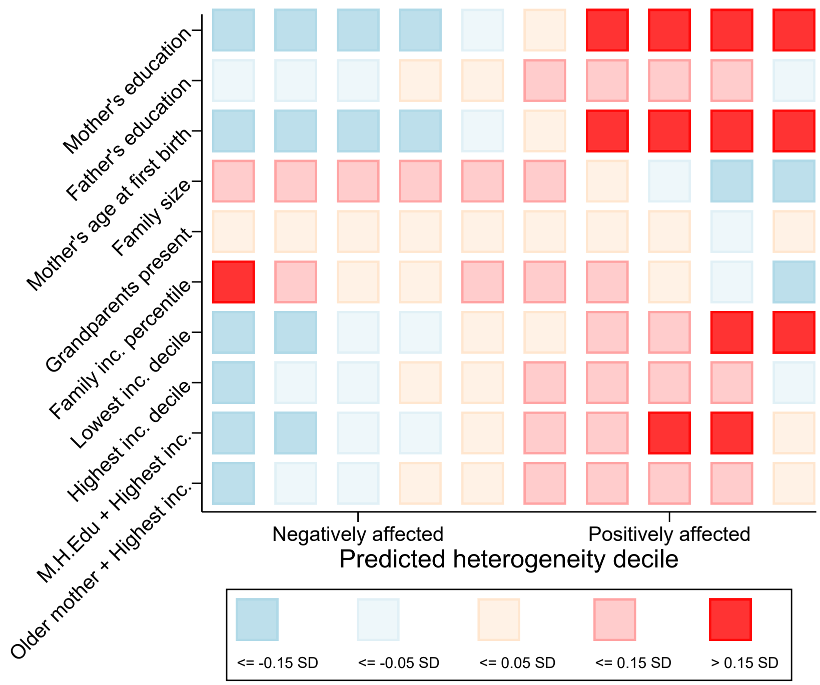


(b) Treatment effects on years of education (with empirical Bayes shrinkage)



Notes: This figure plots the split-sample correlation coefficient between treatment effects on long-term outcomes and treatment effects on measures of adult skills by the number of subgroups that treatment effects are estimated for. The uppermost figures report the correlations when the estimates of treatment effects on adult skills are shrunken using empirical Bayes; the lower figures report the raw correlations. To aid interpretation, correlations with TE on measures of cognitive skills are shaded orange, while those on skills linked to conscientiousness are shaded blue, and those on skills linked to extraversion are shaded gray.

Figure 17: Background characteristics of predicted heterogeneity deciles



Notes: This figure describes the background characteristics of individuals by predicted treatment effect heterogeneity decile. Colors are assigned to each square based on the extent that the mean values of the background covariates differ from the estimation sample mean in terms of standard deviations. Red squares denote larger values in terms of the covariates shown on the left.

2 Tables

Table 1: Estimation sample versus full sample: Outcomes

			Males	
	Full sample	Estimation	Full	Estimation
	(1)	(2)	(3)	(4)
Dropout	0.15	0.14	0.18	0.17
	(0.36)	(0.35)	(0.38)	(0.38)
HS graduate	0.43	0.38	0.34	0.28
	(0.49)	(0.49)	(0.47)	(0.45)
Tertiary education	0.39	0.37	0.31	0.29
	(0.49)	(0.48)	(0.46)	(0.45)
Years of education	12.96	12.91	12.63	12.52
	(2.44)	(2.34)	(2.43)	(2.33)
Income rank at age 35-40	0.50	0.47	0.56	0.53
	(0.29)	(0.29)	(0.31)	(0.31)
Years employed in 30s	7.97	8.01	8.27	8.34
	(2.96)	(2.92)	(2.92)	(2.85)
Ever married	0.63	0.63	0.60	0.59
	(0.48)	(0.48)	(0.49)	(0.49)
Skill data exists	0.81	0.82	0.81	0.82
	(0.39)	(0.39)	(0.39)	(0.39)
Municipalities	463	229	463	229
Individuals	928,500	177,808	174,126	90,434

Notes: This table reports the means and standard deviations of the outcomes for the full and estimation samples in this paper (Columns 1 and 2) and males (Columns 3 and 4).

Table 2: Estimation sample outcomes by the availability of skill data

	Skill data	No skill data
	(1)	(2)
Dropout	0.16	0.26
	(0.36)	(0.44)
HS graduate	0.28	0.28
	(0.45)	(0.45)
Tertiary education	0.30	0.27
	(0.46)	(0.44)
Years of education	12.58	12.24
	(2.27)	(2.53)
Income rank at age 35-40	0.54	0.46
	(0.30)	(0.33)
Years employed in 30s	8.58	7.28
	(2.57)	(3.69)
Ever married	0.60	0.52
	(0.49)	(0.50)
Skill data exists	1.00	0.00
	(0.00)	(0.00)
Municipalities	223	229
Individuals	73,999	16,435

Notes: This table estimates the mean outcomes for the individuals in our sample with (Column 1) and without (Column 2) skill data.

Table 3: Treatment effects for females

	ATE (1)	Treat X family inc. percentile (2)	Effect at 10th percentile (3)	Effect at 50th percentile (4)	Effect at 90th percentile (5)
		Panel A: Effects on education and the labor market			
Dropout	0.005	0.030**	-0.003	0.006	0.015*
	(0.006)	(0.010)	(0.007)	(0.006)	(0.007)
HS graduate	-0.001	-0.166***	0.042***	-0.008	-0.058***
	(0.009)	(0.017)	(0.010)	(0.009)	(0.011)
Tertiary education	-0.013	-0.134***	0.021*	-0.019*	-0.060***
	(0.010)	(0.017)	(0.011)	(0.010)	(0.011)
Years of education	-0.053	-0.610***	0.102*	-0.081	-0.264***
	(0.045)	(0.071)	(0.048)	(0.045)	(0.051)
Income rank	0.004	-0.044***	0.015**	0.002	-0.011*
	(0.005)	(0.008)	(0.005)	(0.005)	(0.005)
Years employed in 30's	0.015	-0.285**	0.091	0.006	-0.080
	(0.049)	(0.086)	(0.056)	(0.049)	(0.054)
Ever married	-0.003	-0.041**	0.008	-0.004	-0.017
	(0.007)	(0.015)	(0.008)	(0.007)	(0.009)
Municipalities	229	229			
Individuals	87,374	87,374			

Notes: Column 1 of this table reports the coeffient β_2 from Equation 3. This coefficient measures the difference in effect of public childcare access between a child at the very bottom of the family income distribution compared to a child at the very top of the family income distribution. Column 2(/4) evaluates this expected treatment effect for the fifth of children from the poorest(/richest) families. Column 3 evaluates the treatment effect for families at the middle of the family income distribution. *= p<0.05, **=p<0.01,***<p<0.001.

Table 4: Treatment effects by family income (raw measures of skills)

	Treat X family inc. percentile (1)	Effect at 10th percentile (2)	Effect at 50th percentile (3)	Effect at 90th percentile (4)
Arithmetic	-0.221***	0.101***	0.013	-0.075**
	(0.032)	(0.022)	(0.019)	(0.023)
Verbal	-0.255***	0.118***	0.016	-0.086***
	(0.030)	(0.021)	(0.018)	(0.023)
Visual-spatial	-0.190***	0.088***	0.012	-0.064**
	(0.032)	(0.021)	(0.018)	(0.022)
Achievement striving	-0.194***	0.071**	-0.006	-0.084***
	(0.036)	(0.022)	(0.018)	(0.024)
Activity energy	-0.167***	0.064**	-0.003	-0.069**
	(0.032)	(0.020)	(0.018)	(0.024)
Deliberateness	-0.055	-0.031	-0.053***	-0.075***
	(0.032)	(0.019)	(0.016)	(0.021)
Dutifulness	-0.149***	0.044*	-0.016	-0.075**
	(0.032)	(0.019)	(0.017)	(0.023)
Leadership motivation	-0.254***	0.101***	-0.001	-0.102***
	(0.034)	(0.020)	(0.019)	(0.026)
Self-confidence	-0.238***	0.081***	-0.014	-0.109***
	(0.030)	(0.020)	(0.017)	(0.022)
Sociability	-0.242***	0.097***	0.000	-0.097***
	(0.030)	(0.020)	(0.018)	(0.022)
Municipalities	222			
Individuals	75,996			

Notes: Column 1 of this table reports the coeffient β_2 from Equation 3. This coefficient measures the difference in effect of public childcare access between a child at the very bottom of the family income distribution compared to a child at the very top of the family income distribution. Column 2(/4) evaluates this expected treatment effect for the fifth of children from the poorest(/richest) families. Column 3 evaluates the treatment effect for families at the middle of the family income distribution. *= p<0.05, **=p<0.01,***<p<0.001.

Table 5: Treatment effects by family income percentile: Occupational task shares

	Treat X family inc. percentile (1)	Effect at 10th percentile (2)	Effect at 50th percentile (3)	Effect at 90th percentile (4)
Non-routine cognitive analytic	-0.151***	0.072***	0.012	-0.048*
	(0.026)	(0.017)	(0.015)	(0.020)
Non-routine cognitive personal	-0.152***	0.072***	0.011	-0.050*
	(0.028)	(0.017)	(0.015)	(0.021)
Non-routine manual physical	0.229***	-0.092***	-0.001	0.091***
	(0.036)	(0.020)	(0.016)	(0.024)
Non-routine manual personal	-0.195***	0.087***	0.009	-0.069***
	(0.030)	(0.019)	(0.015)	(0.020)
Routine cognitive	-0.014	0.013	0.008	0.002
	(0.024)	(0.016)	(0.012)	(0.015)
Routine manual	0.207***	-0.089***	-0.006	0.076**
	(0.033)	(0.019)	(0.018)	(0.024)
Municipalities	77,154			
Individuals	77,154			

Notes: Column 1 of this table reports the coeffient β_2 from Equation 3. This coefficient measures the difference in effect of public childcare access between a child at the very bottom of the family income distribution compared to a child at the very top of the family income distribution. Column 2(/4) evaluates this expected treatment effect for the fifth of children from the poorest(/richest) families. Column 3 evaluates the treatment effect for families at the middle of the family income distribution. *= p<0.05, **=p<0.01,***<p<0.001.

Table 6: Treatment effects by family income percentile: Robustness and bounds

	Family inc. only (1)	Demographic covariates (2)	Dem. + Edu. covariates (3)	Within municipality (4)	Restricted sample (5)	Missings as low (6)	Missings imputed (7)	Missings as high (8)
Panel A: Effects on education, marriage, and the labor market								
Dropout	0.051***	0.052***	0.032**	0.057***	0.049***			
	(0.011)	(0.011)	(0.011)	(0.012)	(0.013)			
HS graduate	-0.102***	-0.095***	-0.040**	-0.109***	-0.101***			
	(0.016)	(0.015)	(0.013)	(0.017)	(0.017)			
Tertiary education	-0.106***	-0.104***	-0.060***	-0.110***	-0.104***			
	(0.015)	(0.014)	(0.013)	(0.017)	(0.015)			
Years of education	-0.564***	-0.554***	-0.301***	-0.598***	-0.532***			
	(0.076)	(0.072)	(0.066)	(0.080)	(0.078)			
Income rank	-0.051***	-0.046***	-0.032**	-0.048***	-0.054***			
	(0.010)	(0.010)	(0.010)	(0.011)	(0.010)			
Years employed in 30's	-0.378***	-0.412***	-0.366***	-0.348***	-0.374***			
	(0.089)	(0.087)	(0.085)	(0.088)	(0.080)			
Ever married	-0.054***	-0.052***	-0.038*	-0.058***	-0.064***			
	(0.015)	(0.015)	(0.015)	(0.015)	(0.016)			
Military service	0.022	0.017	0.014	0.014*				
	(0.013)	(0.012)	(0.012)	(0.012)				
Municipalities	223				223			
Individuals	90,434				75,996			
Panel B: Effects on skills								
Visual-spatial	-0.190***	-0.171***	-0.095**	-0.185***	-0.190***	-0.109**	-0.192***	-0.202***
	(0.032)	(0.032)	(0.031)	(0.033)	(0.032)	(0.039)	(0.029)	(0.036)
Academic	-0.258***	-0.239***	-0.144***	-0.258***	-0.258***	-0.166***	-0.255***	-0.260***
	(0.031)	(0.030)	(0.028)	(0.032)	(0.031)	(0.038)	(0.030)	(0.038)
Social competence	-0.269***	-0.246***	-0.173***	-0.262***	-0.269***	-0.154***	-0.252***	-0.286***
	(0.035)	(0.034)	(0.032)	(0.035)	(0.035)	(0.040)	(0.031)	(0.038)
Municipalities	223					223	223	223
Individuals	75,996					90,434	90,434	90,434

Notes: Column 1 of this table reports the coeffient β_2 from Equation 3. Columns 2-4 include successively more background covariates to the specification: demographic controls, and parental education controls. Column 5 reports effects on registry data outcomes using a sample restricted to individuals for whom there exists data on skills. Columns 5 and 7 report estimates where missing skill measures are imputed as extremely low or high outcomes, and column 6 reports estimates where missing skill measures are imputed using later measures from registry data. *= p<0.05, **=p<0.01,***<p<0.001.

Table 7: Sibling correlations in skills

	Sibling correlation
	Panel A: Main outcomes
Visual-spatial	0.37
Academic	0.47
Social competence	0.34
	Panel B: Cognitive measures
Arithmetic	0.44
Verbal	0.42
Visual-spatial	0.37
	Panel C: Socio-emotional measures
Achievement striving	0.27
Activity energy	0.23
Deliberateness	0.19
Dutifulness	0.24
Leadership motivation	0.33
Self-confidence	0.26
Sociability	0.24
Sibling pairs	69,015

Notes: This table presents the correlations between skills across siblings, using data from the full sample.

Table 8: Treatment effects on skills by first-born status

	Visual-spatial	Academic	Social competence
	Pre-period difference compared to other siblings		
Oldest child	0.153***	0.213***	0.176***
	(0.013)	(0.012)	(0.012)
Sibship size	Yes	Yes	Yes
Municipality and cohort FE	Yes	Yes	Yes
Observations	30,688	30,688	30,688
	Childcare treatment effect heterogeneity		
DiD	0.024	0.026	0.013
	(0.020)	(0.020)	(0.021)
DiD X Oldest child	-0.025	-0.024	-0.040*
	(0.018)	(0.018)	(0.019)
Sibship size	Yes	Yes	Yes
Municipality and cohort FE	Yes	Yes	Yes
Observations	76,018	76,018	76,018

Notes: Panel A replicates results from Black et al. (2018), suggesting first-born men have higher skills across each of the three dimensions we focus on. In panel B, we study how these skills are shaped by access to public childcare. *= p<0.05, **=p<0.01,***<p<0.001.

Table 9: Treatment effects by predicted heterogeneity percentile

	Treat X predicted het. percentile (1)	Effect at 10th percentile (2)	Effect at 50th percentile (3)	Effect at 90th percentile (4)
	Panel A: Effects on education, marriage, and the labor market			
Dropout	-0.041***	0.000	-0.016	-0.032**
	(0.012)	(0.013)	(0.010)	(0.010)
HS graduate	0.071***	-0.022	0.006	0.034**
	(0.015)	(0.012)	(0.009)	(0.011)
Tertiary education	0.075***	-0.039***	-0.009	0.021
	(0.013)	(0.010)	(0.009)	(0.011)
Years of education	0.421***	-0.162*	0.007	0.175**
	(0.070)	(0.067)	(0.059)	(0.064)
Income rank	0.077***	-0.028**	0.002	0.033***
	(0.008)	(0.009)	(0.008)	(0.009)
Years employed in 30's	0.383***	-0.141	0.012	0.165*
	(0.075)	(0.075)	(0.073)	(0.083)
Ever married	0.033**	-0.014	-0.001	0.012
	(0.012)	(0.010)	(0.009)	(0.010)
Military service	0.027*	-0.000	0.011	0.022
	(0.011)	(0.033)	(0.033)	(0.034)
Municipalities	223			
Individuals	90,434			
		Panel B: Effects on skills		
Visual-spatial	0.082**	-0.022	0.011	0.044
	(0.030)	(0.020)	(0.018)	(0.023)
Academic	0.137***	-0.040	0.015	0.069**
	(0.035)	(0.022)	(0.018)	(0.024)
Social competence	0.171***	-0.075***	-0.007	0.061**
	(0.029)	(0.021)	(0.019)	(0.023)
Municipalities	222			
Individuals	75,996			

Notes: Column 1 of this table reports the coeffient β_2 from Equation 3. This coefficient measures the difference in effect of public childcare access between a child at the very bottom of the predicted treatment effect heterogeneity ranking compared to a child at the very top of the predicted treatment effect heterogeneity ranking. Column 2(/4) evaluates this expected treatment effect for the fifth of children expected to be affected most negatively(/positively) by public childcare access. Column 3 evaluates the treatment effect for families at the middle of the predicted treatment effect ranking. *= p<0.05, **=p<0.01,***<p<0.001.

Table 10: Treatment effects by predicted heterogeneity percentile (raw measures of skills)

	Treat X predicted het. percentile (1)	Effect at 10th percentile (2)	Effect at 50th percentile (3)	Effect at 90th percentile (4)
Arithmetic	0.112**	-0.033	0.012	0.057*
	(0.037)	(0.022)	(0.018)	(0.025)
Verbal	0.138***	-0.039	0.016	0.071**
	(0.032)	(0.022)	(0.018)	(0.022)
Visual-spatial	0.082**	-0.022	0.011	0.044
	(0.030)	(0.020)	(0.018)	(0.023)
Achievement striving	0.145***	-0.065**	-0.006	0.052*
	(0.026)	(0.020)	(0.018)	(0.021)
Activity energy	0.094***	-0.039	-0.001	0.037
	(0.028)	(0.022)	(0.018)	(0.021)
Deliberateness	0.081**	-0.086***	-0.054***	-0.021
	(0.031)	(0.019)	(0.015)	(0.020)
Dutifulness	0.072**	-0.045*	-0.016	0.012
	(0.027)	(0.020)	(0.016)	(0.019)
Leadership motivation	0.134***	-0.054*	-0.000	0.053*
	(0.029)	(0.021)	(0.019)	(0.023)
Self-confidence	0.155***	-0.075***	-0.013	0.049*
	(0.032)	(0.022)	(0.017)	(0.021)
Sociability	0.120***	-0.046*	0.002	0.050*
	(0.034)	(0.021)	(0.018)	(0.023)
Municipalities	222			
Individuals	75,996			

Notes: Column 1 of this table reports the coeffient β_2 from Equation 3. This coefficient measures the difference in effect of public childcare access between a child at the very bottom of the predicted treatment effect heterogeneity ranking compared to a child at the very top of the predicted treatment effect heterogeneity ranking. Column 2(/4) evaluates this expected treatment effect for the fifth of children expected to be affected most negatively(/positively) by public childcare access. Column 3 evaluates the treatment effect for families at the middle of the predicted treatment effect ranking. *= p<0.05, **=p<0.01,***<p<0.001.

Table 11: Treatment effects by predicted heterogeneity percentile: Occupational task shares

	Treat X predicted het. percentile (1)	Effect at 10th percentile (2)	Effect at 50th percentile (3)	Effect at 90th percentile (4)
Non-routine cognitive analytic	0.124***	-0.038*	0.011	0.061**
	(0.026)	(0.019)	(0.015)	(0.018)
Non-routine cognitive personal	0.090***	-0.025	0.011	0.047*
	(0.024)	(0.017)	(0.016)	(0.019)
Non-routine manual physical	-0.136***	0.053*	-0.001	-0.055**
	(0.030)	(0.022)	(0.016)	(0.018)
Non-routine manual personal	0.052	-0.012	0.009	0.030
	(0.026)	(0.018)	(0.015)	(0.020)
Routine cognitive	0.031	-0.003	0.009	0.022
	(0.022)	(0.015)	(0.012)	(0.015)
Routine manual	-0.130***	0.046*	-0.006	-0.058**
	(0.032)	(0.023)	(0.018)	(0.021)
Municipalities	204			
Individuals	77,154			

Notes: Column 1 of this table reports the coeffient β_2 from Equation 3. This coefficient measures the difference in effect of public childcare access between a child at the very bottom of the predicted treatment effect heterogeneity ranking compared to a child at the very top of the predicted treatment effect heterogeneity ranking. Column 2(/4) evaluates this expected treatment effect for the fifth of children expected to be affected most negatively(/positively) by public childcare access. Column 3 evaluates the treatment effect for families at the middle of the predicted treatment effect ranking. *= p<0.05, **=p<0.01,***<p<0.001.

Table 12: Background characteristics of predicted heterogeneity quintiles

	Negatively affected quintile (1)	Positively affected quintile (2)	Pos. - Neg. affected (3)
Moether's education	9.53	10.93	1.40***
	(0.03)	(0.03)	
Father's education	10.08	10.31	0.24***
	(0.02)	(0.02)	
Mother's age at first birth	21.69	26.02	4.33***
	(0.07)	(0.07)	
Family size	2.24	1.76	-0.47***
	(0.02)	(0.02)	
Grandparents present	0.62	0.60	-0.02**
	(0.00)	(0.00)	
Family income percentile	47.37	35.88	-11.49***
	(0.37)	(0.39)	
Lowest income decile	0.03	0.14	0.11***
	(0.00)	(0.00)	
Highest income decile	0.05	0.11	0.06***
	(0.00)	(0.00)	
Older mother + Highest inc.	0.00	0.06	0.06***
	(0.00)	(0.00)	
M.H.Edu + Highest inc.	0.01	0.04	0.04***
	(0.00)	(0.00)	
Correlation of index w/ family income		0.15	

Notes: This table presents the mean background characteristics and standard errors of the twenty percent of families predicted to experience the most negative (Column 1) and most positive (Column 2) effects of access to public childcare. The quintiles in this table correspond to the two leftmost and rightmost deciles in Figure 17. The differences between these groups are plotted in Column 3. *= p<0.05, **=p<0.01,***<p<0.001.

3 Data details

Our measures of skills come from the The Finnish Defence Forces. These data include measures of cognitive skills (arithmetic, verbal skills, and visual-spatial skills) as well as socio-emotional skills (achievement striving, activity energy, deliberation, dutifulness, leadership motivation, self-confidence, and sociability) measured upon conscription at age 19 through a battery of tests and surveys. Researchers are only able to able to access the raw composite scores, not the items or item level data. The Finnish Defence Forces report that the Cronbach alphas for the set of cognitive skills ranges between 0.76 and 0.88 and socio-emotional skills ranges between 0.6 and 0.9, but do not allow researchers to see which skills are measured with which reliabilities. The following descriptions of the different dimensions measured can be found in Nyman et al. (2007) and Jokela et al. (2017).

Arithmetic reasoning. Arithmetic reasoning is measured through numeric pattern completion, solving verbal problems, simple arithmetic operations, and choosing relationships between pairs of numbers.

Verbal reasoning. This test measures verbal abilities, focusing on the definitions of words, as well as relationships between words.

Visual-spatial skills. This test measures pattern recognition and matrix completion in a manner similar to Raven's Progressive Matrices.

Achievement striving. 24 items measuring the extent that an individual wants to perform well and achieve socially valued life goals. This measure includes questions aimed at revealing the extent a person is ready to make sacrifices to achieve success.

Activity energy. 28 items measuring the way that individuals approach their day to day activities, including how fast or vigorously someone gets things done, as well as their preferences for fast-paced work.

Deliberation. 26 items measuring the extent that some plans ahead rather than acts in the moment, related to for example, a person's ability to save money rather than spend it right away.

Dutifulness. 18 items measuring the degree that someone follows social norms, for example if they would return incorrectly given change at the store.

Leadership motivation. 30 items measuring people's preferences for taking charge in group situations and abilities to influence others.

Self-confidence. 32 items measuring a person's self-esteem and beliefs regarding their own abilities. Two examples of underlying concepts are whether a person feels as if they are as good and able as others, and whether the person can meet other people's expectations

Sociability. 27 items measuring a persons's gregariousness and preference for socialization. These include measures such as a person's preference for hosting parties and not withdrawing from social events.

4 Conceptual framework

4.1 Social competence as an organizing concept

In this paper we are interested in how public childcare might shape the social and emotional skills of children aged between three and six years old. The literature in child development and psychology provide an important base from which to approach the potential effects such an intervention may have. We outline the relevant literature from child development here, and show how these concepts may be incorporated into an economics framework in the context of this paper.

The importance of early childhood is documented in prior research across a wide range of disciplines including economics, pscyhology and child development, as well as sociology (Duncan et al., 1994, 2010; Currie and Almond, 2011; Black et al., 2017). In a recent overview of the science of child development, Black et al. (2017) suggest that childhood is a period consisting of ordered stages in which perceptual, motor, cognitive, language, socio-emotional, self-regulation, and cultural skills develop through a rich series of interactions. They explain that several factors can affect the development of these skills, including play, socialization, responsive caregiving and early learning.

For children three to six, the literature on child development has long emphasized that how children are socialized shapes their behavior in later years (Erikson, 1950; Piaget, 1954; Baumrind, 1967). The treatment we study is exposure to public childcare between the ages of three and six. While we might expect public childcare itself to be relatively constant in our context, the family environment or other type of informal care which public childcare subsitutes for may vary drastically. And, understanding the role of public childcare involves understanding how it may potentially substitute for this informal childcare option, often in the family (Clarke-Stewart et al., 1994; Busch-Rossnagel and Knauf-Jensen, 1995; Maccoby and Lewis, 2003; Csibra and Gergely, 2009). Moreover, as has been long understood, the family presents not only the likely counterfactual for public childcare, but also the first place where young children are socialized (Clausen, 1966). As such, the actual treatment we study is likely to vary at the family level and be defined by the difference in early childhood environments between informal or home care and public childcare.

Waters and Sroufe (1983) argue that social competence – the ability to recruit personal and interpersonal resources in the context of goal achievement – is the central organizing construct of early childhood. Since then, social competence has played an important organizing role in early childhood research (Campbell et al., 2000; Denham et al., 2003; Vaughn et al., 2009). Vaughn et al. (2009) describe that social competence consists of three parts: i) behavioral and cognitive skills for successful goal achievement with social contexts; ii) the ability to discover the goals of interactive peers; iii) the understanding of a child's relative value as a preferred playmate. For example, focusing on parents, Pomerantz et al. (2005) highlight the role of parental socialization as a determinant of how children approach achievment, and Gunderson et al. (2013) describe one nice example of how such skills might develop, focusing on how parental praise can lead to persistent improvements the self-confidence and particularly motivation of young children still several years after treatment. Phillips et al. (1987) emphasize verbal interactions between caregivers and children more broadly in childcare settings.[26]

[26]Another potential mechanism behind the development of social skills in childcare is simply the informal interac-

In turn, social competence – through motivation in social contexts – may shape other learning outcomes (Dweck, 1986). Of course, in addition to shaping a child's social competence, early childhood socialization may directly affect other areas of learning such as verbal skills (Hart and Risley, 1995).

4.2 Life-cycle skill development, a framework

However, as has been noted in the prior literature in economics, the way early experiences may affect later outcomes is not necessarily obvious. We formalize key points using a multi-period model of childhood investment (Becker and Tomes 1986; Heckman 2006; Cunha and Heckman 2010; Heckman et al. 2013). People's skills (θ) across various dimensions (k) develop over multiple periods of childhood and adolescence ($t \in 1, 2, ..., T$)—shaping various adult (A) outcomes. Skill development in one period is a function of household investments (H)[27], public investments (D), and skills in the prior period such that,

$$\theta_{k,t+1} = f_{t,k}(H_{k,t}, D_{k,t}, \theta_{k,t}).$$

Self productivity. Higher levels of skills in one period may allow for more efficient learning of the same skill in later periods, suggesting that the possibility for effects of childhood investments measured at later stages to be larger than those measured initially.

$$\frac{\partial f_{k,t}(H_{k,t}, D_{k,t}, \theta_{k,t})}{\partial \theta_{k,t}} > 0$$

Dynamic complementarity. Individuals with greater early childhood skills in one domain may be more efficient in learning other types of skills later (say in elementary school). This idea highlights the potential for initial effects in one area to result in later effects in others, and stresses the highly interactive nature of skill investments across periods. This is referred to dynamic complementarity between investments in one skill (k) and the development of other skills (l) in later periods:

$$\frac{\partial^2 f_{k,t}(H_{k,t}, D_{k,t}, \theta_{k,t})}{\partial D_{k,t} \partial \theta_{l,t}} > 0$$

tions between children themselves. The role of peer interactions in formal and informal contexts in early childhood and elementary school has been a large area of research (see, for example, Ladd 1990; Coolahan et al. 2000; Lenard and Silliman 2021).

[27] While children themselves may be unlikely to make consequential investment decisions in early childhood, we consider the household to include the child themselves—whose own investments become more consequential in later years.

Endogenous investments and substitution. Additionally, we might imagine that household and public investments are endogenously determined.[28] Accordingly, households may react to public investments in childcare by changing their own investment behavior - potentially substituting away from other forms of childcare:

$$H_{k,t+1} = f(H_{k,t}, D_{k,t}, \theta_{k,t})$$

$$D_{k,t+1} = g(H_{k,t}, D_{k,t}, \theta_{k,t})$$

Skills, education, and the labor market. Lastly, educational attainment is a function of skills as well as household and public investments. Following seminal models in education and labor economics, we consider labor market performance to be a function of education (Becker, 1962; Mincer, 1974) potentially in addition to the direct effect of skills on labor market outcomes (Deming, 2017; Papageorge et al., 2019; Izadi and Tuhkuri, 2021).

$$E_{k,t+1} = f(\theta_{k,t}, H_t, D_t)$$

$$Y_{k,t+1} = f(E_t, \theta_{k,t})$$

Empirical implications. Thus, an empirical implication of the above model is that if changes in some skill $\theta_{k,t}$ are part of the reason we see effects on a long-term outcome $Y_{k,t+1}$, it should be the case that the people who experience effects on the long-term outcome also experience effects in that particular skill:

$$corr[(\frac{\partial \theta_k}{\partial D})_i, (\frac{\partial Y}{\partial D})_i] \neq 0$$

An important note, here, is that this correlation can be different from zero even if some particular skill (θ_{kt}) does not causally drive the the effects on Y. It could, for example, be that some skill adjacent to k is driving the effect, and we simply happen to obsere the effect on k. Likewise, since it is not necessarily some particular skill (k) that shifts Y, it is possible for effects on multiple skills (k, l, m) to all be correlated with the effect on Y such that the sum of these correlations is greater than one.

In the context we study – where public investment in early childhood (D) changes – this framework suggests the following points: i) public investments in early childhood can shift skill development in specific domains, and affect the level of these skills at different points in time; ii) skills acquired in one domain (say social competence) can shape the productivity of later investments in other domains (say verbal skills); iii) changes in public investments in early childhood may affect

[28] If households are more nimble to respond to public provision than the government is in responding to household provision, the function (f) may include an additional term for same-period public investment ($D_{k,t+1}$).

household investments in skills; iv) skills and education may have distinct effects on labor market outcomes; v) since childcare investments are endogeneously determined (by both municipalities and households), the relationship between household or public investments in childcare and later outcomes is not identified by a cross-sectional comparison of households accessing public childcare with those that do not.

5 Methods

5.1 Estimating correlations between skill and labor market treatment effects

We are interested in understanding how the effects of public childcare on skills and long-term outcomes are related to each other. More concretely, we ask: Do children who benefit from access to public childcare in terms of particular skills also experience improvements in longer-run outcomes such as income? Answering this question probes a basic assumption underlying causal mediation.[29]

In an ideal world, we might look at whether the specific individuals who experience an improvement in skills as a result of access to public childcare are the same people that earn more as a result of public childcare. Recapitulating the points from Section 4, one estimator for this relationship is the correlation:

$$\theta = corr(TE_i^M, TE_i^Y)$$

This framing takes a similar form as the study of the covariance between treatment effects on test scores and treatment effects on wage earnings in the context of teacher value added (Chetty et al., 2011).[30] Unfortunately, as opposed to the case of teacher value-added, in our context – as in many others – estimating individual level treatment effects rests on untenable assumptions. One way to overcome this challenge is to estimate a large number of subgroup treatment effects

[29] In a common approach to a causal mediation (see, for example, Imai et al. (2010) or Heckman et al. (2013)) authors ask: How much of the effect of treatment (D) on an outcome (Y) can be explained by the mediator (M)? While this approach to mediation has the potential to provide an estimate of the degree to which particular mediators drive the effects of D on Y, it relies on strong assumptions. In addition to standard causal assumptions, Imai et al. (2010) term the additional assumption required for causal mediation as *sequential ignorability*, consisting of a first part that requires that the potential outcomes of mediators to be independent of treatment, and a second part that requires the relationship between the causal mediator (M) and the outcome (Y) to be uncorrelated with any unobserved covariates. In most applications, however, as in our own, this assumption is unlikely to hold, since – as described in Section 4 – effects on, say social skills, are likely to be correlated with effects on, say cognitive skills.

[30] See also, for example, Jackson (2018) who studies the relationship between teacher value added based on academic outcomes compared to teacher value added based on behavioral outcomes, and their relationship to longer-term outcomes.

for individuals who are likely to respond to treatment in different ways, and correlate these. For example, Angrist et al. (2022) split up their sample in a number of ways based on a number of background characterstics such as race, gender, and prior academic performance.

We extend this work by using machine learning to group individuals by their predicted response to public childcare access, discussing potential biases in the estimation of treatment effect covariances, and outlining ways to test the robustness of the estimates.

For each group (g), we estimate treatment effects (β) on both measures of adult skills (M) as well as longer-run outcomes (Y).

$$\hat{\theta} = corr(\hat{\beta}_g^M, \hat{\beta}_g^Y)$$

Assuming we can estimate the β's precisely and without bias, $\hat{\theta}$ is likely to be biased upwards as long as the number of groups is small. This may be because the extent that the same *general* group of people experiences effects on M also experience effects on Y may conceal differences in effects for more granular groups. Additionally, in small samples correlations are estimated with bias, since the sample covariance is divided by $n-1$ rather than just n. For both these reasons, as the number of groups grows, $\hat{\theta}$ should approach θ. We estimate $\hat{\theta}$ for ten to one-hundred splits, and show that $\hat{\theta}$ does indeed decrease as the number of splits increases, but plateaus after about seventy splits $\hat{\theta}$.

$$\theta = lim_{g\to\infty}\hat{\theta}$$

At the same time, however, as the number of groups increases, however, it is likely that the β's will be estimated increasingly imprecisely, thereby inducing our estimate $\hat{\theta}$ to be biased downwards. To avoid some of this downward bias, we use empirical Bayes to produce shrunk estimates of our mediating outcomes $\tilde{\beta}_g^M$.[31]

$$\tilde{\beta}_g^M = \lambda\hat{\beta}_g^M$$

So long as any bias in the estimate of the TE on the mediator ($\tilde{\beta}_g^M$) is correlated with bias in the estimate of the TE of the long-term outcome ($\hat{\beta}_g^Y$) for the same group (g), however, the two estimates will be mechanically correlated, and this estimator is likely to be upward biased. To avoid such mechanical correlation we use a split-sample approach, where we estimate $\tilde{\beta}^M$ in using data from one half of each group (g^{ss1}) and estimate $\hat{\beta}^Y$ with data from the other half of each group (g^{ss2}).

$$\hat{\theta}^{ss} = corr(\tilde{\beta}_{g^{ss1}}^M, \hat{\beta}_{g^{ss2}}^Y)$$

[31] Shrinking both $\hat{\beta}_g^M$ and $\hat{\beta}_g^Y$ may induce a mechanical correlation between these estimates (see Chetty et al. (2011)); we only shrink the estimates of β for the mediator. We use the split-sample estimator based on test-retest reliability from Coey and Cunningham (2019) to shrink our estimates.

Since the splitting of each group in two will induce randomness, we estimate this split sample estimator $\hat{\theta}^{ss}$ fifty-one times, and take the median of these estimates across splits ($\hat{\theta}^{\bar{s}\bar{s}}$) to improve the reliability of these estimates. This is the estimate we report in the following section.

We complement our estimates of $\hat{\theta}^{\bar{s}\bar{s}}$ with their counterparts based on linear regression ($\hat{\alpha}$).

$$\hat{\beta}^{Y}_{g^{ss2}} = \alpha_0 + \alpha_1 \tilde{\beta}^{M}_{g^{ss1}} + e_g$$

These regression based estimates of α_1 provide a natural way to assess the statistical significance of the covariances ($\hat{\theta}^{\bar{s}\bar{s}}$). Also, compared to the correlation coefficients, which simply measure the extent that two treatment effects covary, the rescaling of the regression coefficient provides what might be thought of as an upper bound on the extent to which a one unit change in the treatment effects on the mediator might explain changes in treatment effects on the outcome.

Table 9 presents our main estimates of heterogeneity in treatment effects. Column 1 shows the predicted change in outcome as families move from the very bottom of the predicted heterogeneity ranking to the very top. Across all outcomes—ranging from administrative measures of labor market performance, years of education, and measures of adult skills—these estimates suggest there is statistically significant heterogeneity in the effects of public childcare. For most individuals in our sample, however, the magnitude of these effects is relatively small. In Columns 2-4 of Table 9, we estimate the magnitude of treatment effects for families at the bottom 10th percentile, 50th percentile, and 90th percentile of the predicted heterogeneity rank. These estimates suggest that childcare access had both positive and negative effects on children, depending on what kind of family they came from.

As in the analysis of average treatment effects, the validity of these results rests on there being parallel trends in the potential outcomes of individuals in the absence of treatment, but with the stronger requirement that there are parallel trends at each level of predicted heterogeneity. Since the predicted heterogeneity rank is based on observed family background characteristics, balance along these measures no longer provides a test of the validity of the research design. Still, we can plot the pre-policy trends in outcomes for families at different points in the distribution of predicted heterogeneity. Appendix Figures 5 and 6 suggest that the outcomes of families in both the top and bottom twenty percent of predicted heterogeneity evolve in a parallel way prior to treatment.

5.2 Predicting treatment effect heterogeneity

We can imagine that potential outcomes for individuals are Y_i^1 if they have access to childcare and Y_i^0 when they do not. Further, each individual (i) can be characterized by a vector of covariates, Z. We can imagine that the baseline potential outcome in the untreated state $b(Z)$ is defined as

$E[Y^0|Z]$, and that treatment effects conditional on the vector of coefficients are defined as follows:

$$s(Z) := E[Y^1|Z] - E[Y^0|Z] \tag{6}$$

As described in the above section, we want to form granular subgroups so that we can estimate correlations in treatment effects across outcomes. Moreover, we want to do this in a way that both maximizes variation in treatment effects across subgroups while minimizing our degrees of freedom as researchers. With this goal in mind, we will use observable characteristics (Z) from our data to provide a measure of predicted treatment effect heterogeneity that can be used to split our data up into an arbitrarily large number of subgroups.

To predict variation in outcomes using information on observables we use a machine learning framework based on Chernozhukov et al. (2021). These authors acknowledge the near impossibility of consistently estimating the conditional average treatment effect (CATE) given a particular set of background variables. Instead, they accept the fact that different splits of training (auxiliary) and test (main) samples may produce different estimates of treatment effect heterogeneity – each time identifying different sets of variables predictive of heterogeneous treatment effects. This inconsistency in the particular set of characteristics predicting heterogeneity is likely to be all the more accentuated when variables are correlated with one another. Setting aside the goal identifying a single CATE, they suggest taking advantage of repeated data-splitting to avoid overfitting and provide valid estimates of feature of treatment effect heterogeneity.

The general approach to estimating treatment effect heterogeneity we implement (Chernozhukov et al., 2021) works by randomizing units into main and auxiliary samples hundreds of times, applying machine learning to the auxiliary samples, generating predictions in the main samples, and then picking median main sample parameter estimates across the splits. Given that adult income rank is the main long-term outcome that we study, we run our machine learning excercise for adult income rank. We operationalize this approach as follows.

Before implementing our machine learning procedure, we divide all of our background variables into categorical measures. Then, we interact all these measures with each other, generating several hundred variables. For investigating treatment effect heterogeneity, we then interact each of these variables with treatment status ($D_{imc} = FIRST_m \times POST_c$).

We begin by randomizing municipalities into auxiliary and main samples with equal probability 500 times (N).[32] We maximize power by ensuring that half the treated and municipality units end up in the main and auxiliary splits at each randomization (Chernozhukov et al., 2021). For each split, we then use elastic net regressions in the auxiliary (A) sample to generate predictions of adult income rank given a array of covariates (Z) for both treated units and untreated units

[32] Wager and Athey (2018) suggest randomizing units into auxiliary and main samples at the level of treatment assignment, in our case the municipality. This parallels contemporary understandings of how standard errors should be clustered when estimating treatment effects (Bertrand et al., 2004).

($D_{imc} = FIRST_m \times POST_c$).

$$\forall i \in N, A \begin{cases} \hat{Y}_i^1 | Z, D = 1 \\ \hat{Y}_i^0 | Z, D = 0 \end{cases}$$

Then, we take these predictions to the main sample (M), where we create a measure of predicted treatment effect heterogeneity ($\hat{S}_i$) for each individual, given the machine learning estimates from the auxiliary sample of that split.

$$\hat{S}_i = \hat{Y}_i^1 - \hat{Y}_i^0, \forall i \in N, M$$

Various approaches to using machine learning for the identification of heterogeneous treatment effects have been proposed in the literature (see, for example, (Imai et al., 2013; Zhao et al., 2018; Wager and Athey, 2018; Chernozhukov et al., 2021)). The key goal of these methods is to provide a way of formally selecting amongst a large number of covariates to estimate both baseline values and treatment effects using some form of regularization (see, for example, Tibshirani, 1996 or Athey and Imbens, 2019). Given our difference-and-difference setup, we first ensuring balance using uniformly valid post-double selection method (Belloni et al., 2014, 2017), and then use an elastic net approach following Zhao et al. (2018).

To simplify our empirical approach for the context of the elastic net, we residualize outcomes using untreated units to remove municipality and year effects from adult income rank (see, for example, Gardner, 2021). This allows us to operationalize our machine learning estimation of treatment effect heterogeneity as if we were working with random assignment, using just the treatment indicator and the residualized outcomes (Y_i^*).

We use an elastic net to generate our predictions (see, for example Chernozhukov et al. (2021), who find the elastic net to perform well relative to other machine learning procedures), using a uniformly valid post-double selection method (Belloni et al., 2014, 2017). In the first step of this approach, we select a set of control variables that are useful for predicting treatment (D_i) – the set of potentially important confounding factors. In the second step of this approach, we select the set of characteristics useful for predicting the outcome (Y_i). Third, we estimate heterogeneity in treatment effects by forcing the inclusion of both these sets of variables, and allowing the elastic net to select additional additional variables from the set of variables interacted with treatment status (see Zhao et al., 2018). This process helps to induce balance across all observable background characteristics that might be relevant in explaining either treatment assignment or outcomes. We use this information to generate predictions of $\hat{Y}_i^1$ and $\hat{Y}_i^0$ that we carry over to the main sample.

Following Chernozhukov et al. (2021), we then post-process these predictions of treatment effect heterogeneity to reduce sampling noise using a linear regression:

$$Y_i = \alpha' \mathbf{X}_{1i} + \beta_1(D_i - p(Z_i)) + \beta_2(D_i - p(Z_i))(\hat{S}_i - \bar{S}_{N,M}) + \epsilon_i, \forall i \in N, M,$$

$$E_{N,M}[w(Z_i)\hat{\epsilon}_i \mathbf{X}_i] = 0$$

The coefficient β_2 measures the extent to which $S(Z)$ predicts treatment effect heterogeneity. The vector $\mathbf{X}_i$ includes municipality and cohort fixed effects, a phase-in dummy, and the predicted outcome of each main sample individual in the absence of treatment ($\hat{Y}_i^0$). The term $p(Z_i)$ is the propensity score, bounded between zero and one, that an individual with characteristics Z is treated and $w(Z_i)$ are weights defined by $(p(Z)(1-pZ))^{-1}$.

The coefficient β_2 measures the extent to which $S(Z)$ predicts treatment effect heterogeneity. When $\hat{S}_i$ provides a strong signal of TE heterogeneity in the main sample, this has little effect. Under the unlikely scenario that $S(Z)$ provides a perfect proxy for treatment effect heterogeneity, $\beta_2 = 1$. In the case that β_2 contains no information on heterogeneity, $\beta_2 = 0$. Rejecting the null hypothesis that across all splits the median $\beta_2 = 0$ suggests both that there is heterogeneity in treatment effects, and that $S(Z)$ provides relevant information by which to predict TE heterogeneity. However, as noted by Chernozhukov et al. (2021), failing to reject the null hypothesis ($\beta_2 = 0$) does not necessarilly mean that there is no TE heterogeneity; given the demands of this approach on statistical power, this is often the case. In our case, the median value of β_2 is 0.26 – lower than in the author's example application, suggesting that our machine learning excercise produced a noisy signal of heterogeneity. Since each split is estimated from just half of the overall sample, we lack statistical power to reject the null hypothesis. We take one of the target parameters the authors outline and show that it can be useful even when there is insufficient statistical power for variational analysis that takes into account split-level uncertainty that arises from randomness in splits.

The target parameter for us will be the "personalized prediction" of $S(Z)$ for each individual:

$$\hat{\theta} = \hat{\beta}_1 + \hat{\beta}_2(\hat{S}_i - \bar{S}_{N,M}), i \in N, M$$

This parameter is estimated in the main sample about 250 times for each individual (the other half of the time the individuals' municipality is randomized into the auxiliary sample). As per Chernozhukov et al. (2021), we take the median across all main splits to provide the best guess of the individual personalized prediction for each individual. Given that we can generate these out of sample predictions not just for half our sample, but the full sample we can estimate heterogeneous treament effects without losing power.

To facilitate interpretation, we convert these main sample medians of the personal predictions to a continuous measure of rank on a scale of zero to a hundred (Davis and Heller, 2020). Since these ranks are generated from aggregating predictions of features relating to TE heterogeneity across hundreds of splits, and include sampling noise, there is no one index that corresponds to this ranking. In fact, given randomness induced by sample splitting, it is even possible that two individuals born to families with identical observables have slightly different ranks. Thus, this approach is not useful for trying to predict how a new sample might respond to a treatment in heterogeneous ways, but rather to describe the heterogeneity in the responses of the estimation sample.

We can use this index of predictions to estimate heterogeneous treatment effects just as we did with family income rank. First, we test for the identifying assumption using event-study estimates, focusing on the top fifth of and bottom fifth of the predicted heterogeneity index. Appendix Figures 5 and 6 show that prior to treatment there is no evidence of trends in the pre-period. The machine learning excercise we use for our (out-of-sample) predicted treatment effect index is based on adult income rank as an outcome – so perhaps it is not surprising to see evidence of parallel trends in the pre-period for this outcome. Reassuringly, both the other long-term outcomes and skill outcomes also show no evidence of trends in the pre-period.

Parallel to the parametric estimates we report across family income percentile, we report the parametric estimates across predicted treatment effect percentile in Appendix Tables 9 and 6.

To test for whether or not such a parametric approach is justified, we can test for the linearity of treatment effects across the distribution. While we lose the statistical power that we gain from pooling heterogeneous effects into one index, we can estimate these seperately for each percentile of the predicted heterogeneity index to provide a non-parametric version of these estimates. These are presented in Appendix Figures 7 and 9 for all outcomes, and, despite considerable noise in individual point estimates, suggest that there is indeed a linear pattern in the effects. We further split these subgroups in two for the granular subgroup estimates we use to estimate the correlations in the treatment effects between medium and longer-term outcomes.

Appendix Figure 17 and Table 12 describe how family background characteristics vary between individuals with different predicted treatment effects.