Early child care and adult outcomes: the effect of attending the *École Maternelle* in France*

Francesco Andreoli[†] Arnaud Lefranc[‡] Vincenzo Prete[§]

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Abstract

Recent literature has shown the importance of early life period on production of skills and the subsequents benefits obtained in labor market outcomes. Publicly provided child care plays a decisive role on the purpose. Making use of two large cross sections of 40 cohorts of employees, we estimate the effects of participation in *École Maternelle* (EM), a unique French publicly provided child care policy targeting children aged 2 to 6, on future earnings and economic success. After correcting for background heterogeneity in the selection process by instrumenting EM, returns amount to 14.1% of expected wages and a positive probability of finding in the top quartile of income distribution. No significant effect is found for cohorts or EM duration, while children coming from more advantaged background face substantially lower returns.

Keywords: Education, pre-school, skills, FQP, wage inequality.
JEL codes: I24, I26, J24, J31.

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[†]DSE, University of Verona, via Cantarane 24, 37129 Verona, Italy and Luxembourg Institute of Socio-Economic Research (LISER), MSH, 11 Porte des Sciences, L-4366 Esch-sur-Alzette/Belval Campus, Luxembourg. E-mail: francesco.andreoli@liser.lu.

[‡]THEMA - University of Cergy-Pontoise, boulevard du Port, Cergy Pontoise Cedex, France. E-mail: arnaud.lefranc@u-cergy.fr.

[§]DSE, University of Verona, via Cantarane 24, 37129 Verona, Italy. E-mail: vincenzo.prete@univr.it.

1 Introduction

Two recent reports by OECD (2001, 2006), focusing on cross country comparisons of preschool care policies, have put clearly in evidence the priority of access and quality in the early childhood education policies devoted to strengthen the foundations of lifelong *learning*. Heckman *et al.* (2001, 2006, 2007, 2008) also show that early intervention and child care oriented policies promoting equality are also efficient. There is in fact evidence that preschool years are critical periods for skills production, and also a strong complementarity of skills investments during life cycle on the human capital production exists: early child intervention is then essential to strengthen the foundations of lifelong *earnings*. We focus on French early care market, dominated by the *École Maternelle* care system: it is a unique long-lasting free public care program designed for children between 2 and 6 years old without any distinction for nationality, race or religion. This papers deals with the effects of *École Maternelle* on lifelong economic success and earnings in a causal way, providing a credible source of identifying information and estimating a return to participation.

The *École Maternelle* (EM hereafter) is considered by the Ministry of Education the first introductory step in the French pupils' educational career. This preschool program may well be considered unique between OECD surveyed countries[1] for a large set of characterizing attributes. Created in 1881, EM is a voluntary, free, non-confessional care institution integrated with primary schooling under State responsibility and financing. During the first half of 20th Century, EM supply consistently increased in all France while instructors quality (certified by state mandatory examinations) and compensations were aligned to primary

[1] The surveys can be found at: `www.oecd.org/edu/startingstrong2`. See also this page for more information on OECD early education policies: `www.oecd.org/edu/earlychildhood`.

school colleagues. This quality jump participate in explaining the strongly increasing participation of children coming from middle class families occurring after WWII, as Figure 1 shows. As a response to positive effects of EM on schooling duration and achievement, as reported by Norvez (1990) in a survey of many studies on the topic, in the late Seventies many policies were promoted to increase the supply of EM in rural areas and in bigger cities disadvantaged neighborhoods. The enrollment trend over cohorts by duration reported in Figure 2 is nearly 100% for children taking at least 2 years while currently full participation of 3 to 6 years old children is observed, while only 25% of 2 years old is enrolled. Anyway, the convergence rate to full participation has been slow and is plausible that a multiplicity of causes can take part in explaining, between cohorts, the enrollment pattern.

State is also the only supplier of the service. To guarantee equality in treatments between regions, the Ministry of Education defines the curriculum, opening hours, financing participation of the family (while contribution from the public side amounts to 0.7% of GDP), and selects, trains and remunerates teachers and inspectors. Local communities have instead significant power in buying and maintaining the teaching materials (from buildings to games) and in deciding additional services provision, depending on local financing rules and constraints.

Along with other benefits, EM enters into the cumbersome French welfare system as an instrument to stimulate mother participation to job market during early life period of the child.

These policy features are extremely valuable for the purpose of this paper since some sources of heterogeneity should be treated as exogenous. All children treated in the same cohort are likely, by mean of law requirements, to receive the same quality of treatment. Between cohorts variations in quality or access is yet a delicate issue along with community based effects. On the causal effect of EM on woman labor supply elasticity there are less concerns: in line with the American evidence by Gelbach (2002), Goux and Maurin (2008) find that elasticity of maternal labor supply is significantly affected by early care only in the case of single parent households, while the effect totally vanishes for multi-parents households. Once these effects are assumed constant in cohort-region cell, centralization of the quality supply offers support for identification. As an example, Goux and Maurin (2008) uses eligibility rules as an instrumental variable (IV) for measuring EM duration effects on school attainment and achievement measures.

Our paper is new with respect to other works on EM effects and on early care programs evaluation for three basic reasons. Firstly, the discussion is strongly backed up on a very recent research filed justifying the link between EM and job market outcomes. In a repeated series of papers Heckman *et al.* (2001, 2006, 2008) and Cunha *et al.* (2006, 2007 and 2008) provide evidence that early childhood period is critical for skills production and on the fact that skills gaps open up early in life while efficient compensation is not possible later. Skills are multiple in nature (in general divided in cognitive or non-cognitive traits as productivity, IQ or leadership, management abilities or stability and trustworthiness) and have also a strong explanatory power on equilibrium wages determination. Moreover, OECD (2006) report makes clear the quality provided by the EM and its crucial role in soften pupils impact with primary education system. Secondly, we focus on market outcomes as wages or the position in the wage distribution, as key elements of economic success. The main body of literature on child care programs evaluation establishes a connection with later achievement or attainment possibilities generated by treatment[2], but they often rely on a very specific set of skills captured by available tests (mainly cognitive), leaving aside any other important component of a person's ability "bundle" that contributes in shaping the human capital accumulation profile along life cycle. In line with Bowles and Gintis (2002) and Bowles, Gintis and Osborne (2001), we assume that wages determined by a market institution (although strong contract laws, unions and state subsidies make the French job market far from being perfect) reflect all types of skills relevant for the task chosen by the worker, and then provide a global measure of individual abilities. If genetic inheritance contributes little to intergeneration skills persistence (and IQ, see Bowles and Nelson, 1974), and thus abilities at the entrance of EM can be considered as randomly assigned, then a correct specification for the selection mechanism allows to estimate the EM effect on wages. Third, the paper is new because a cohort evaluation is proposed making use of two large cross section of individuals born in an interval of more that 40 years, providing enough variability in EM enrollment to identify the treatment effect.

With in mind the particular ambition of exploiting possible biases in estimation and to propose a valid solution, Section 2 reports a brief review of early care literature and provides a simple model to tackle the issue of family background effects on EM selection. Section 3 provides a description of data and the identification technique used, while estimation results are discussed in Section 4. Finally, Section 5 concludes.

[2]See Goux and Maurin (2008) for EM or Reynolds and Temple (1998) for Chicago Program and Blau and Currie (2006) for a survey on the topic.

2 The Economics of Child Care and Skills Development

Skills are multiple in nature, cognitive and non-cognitive. Is well documented (see Cuhna *et al.*, 2006 for a review of the empirical studies) that all of them affect in different ways the wages of individuals. Cognitive skills (ability, IQ as an example) shape the implicit costs and benefits determining schooling decisions and the human capital accumulation. Non-cognitive skills are also strong contributors in defining the economic success of individuals, depending on the job and tasks (Bowles *et al.*, 2002). Abilities are not evenly distributed among people, and this fact explains a significant portion of inequality in later life income disparities. As documented by Cuhan and Heckman, 2007, skills gaps among peoples open up early in life and family environment has a strong role in shaping these gaps throughout genetics, family investments and environmental effects. The authors explain evidence on child development studies by strong complementarities among skills and self productivity of investments in skills, and they estimate, by mean of a large battery of standardized tests, that sensitive periods for skills investments occurs in different moments along the child life cycle, according to the type of skill analyzed. Differences in investments on early child skills enhancing activities predicts (partially) later differences in economic outcomes and more specifically in wages. Cunha and Heckman (2007) also claim that complementarity in investments makes early child intervention neutral towards efficiency and equity tradeoff: returns from early investments are higher for more disadvantaged children that otherwise would face a more constrained environment at home. However, final benefits are strongly affected by the availability of subsequent investments (Heckman and Rubinstein, 2001). The returns, anyway, varies considerably in the US case depending on the type of program selected. If a single state-wide performed policy as EM is proposed, than a unique return is expected and this information becomes crucial to the policymaker because of the sheer difficult of overcoming poor investments in human capital later in life.

A large set of pre-school intervention programs[3] surveyed by Cunha *et al.* (2006) and Blau and Currie (2006) shows that the larger effect of investments in cognitive skills production is found for children aged 6 to 9, while non-cognitive skills are more efficiently produced with later investments. The authors underline that previous periods are even more important, although lack of data makes the proposition untestable.[4] In what follows we consider jointly the maternal problem of EM selection and skills investment in a way which is in line with the exposed literature but contributes by considering the role played by a policy similar to EM and thus characterized as a "merit good" with low price and high quality.

2.1 A Simple Static Model for Child Care

This section aims at providing evidence of the importance of early child intervention, skill formation process and the relations with EM by mean of a simple model, capturing the effects of exogenous changes and predicting the behavior of mother under standard rationality assumptions. For simplicity, the model does not consider intertemporal tradeoff in investment decisions. We analyze the problem of a mother deciding the optimal investment in care, I, consumption C and leisure l, when her utility $U\left(\frac{qh(h';\ g(I))}{(1+r)^2};\ C;\ l\right)$ depends on future market outcomes of children (the first term). The child lives T periods: the firs period,

[3] Between the many, the most related to our analysis are Head Start, Perry Pre-school Program, Abecedarian, Chigago CPCP. Program are listed in increasing order of intervention intensity, ranging from schooling support to full kindergarten and family training activities.

[4] Ruhm (2004) puts a negative light on the claim, suggesting that early advantages decay rapidly and often not significant returns are found, in achievement tests in primary school years.

childhood, is the *skill - sensitive* period of life, where skills (including schooling ability) are formed. The second period is devoted to human capital accumulation valuable in market, which could range form schooling (the most important, also consistently with a signaling motive) or other task-specific abilities. The remaining $T-2$ period, working activity takes place and the present values of future earnings per unit of human capital, $\{w_t\}_{t=3}^{T}$ equates:[5]

$$q = \sum_{t=3}^{T} \left(\frac{1}{1+r} \right)^{t-3} w_t.$$

Behind this construct lies the assumption that once the human capital is set, it monotonically determines the present value of future incomes of the child. The higher the capital, the higher the value. Human capital is produced in second period according to the following rule:

$$h_c = h\left(h'; \delta\right) \tag{1}$$

where h' is the human capital of mother while δ is a unidimensional index of *quality* of the care service purchased by the mother, containing all the information related to each possible modality of child care. If EM is not present in one region, than $\delta = 0$. Implicitly the model assumes that once the vector of cognitive and non-cognitive first period skills is set, second period compensation is not possible and the final human capital is then mechanically generated.[6]

[5] As in Cunha *et al.* (2007), we abstract from endogenously chosen on-the-job training, learning-by-doing and other forms of accumulation of abilities that enrich, but complicate, the structure.

[6] This point makes our model differ from Heckman theoretical structure. The author assumes that second period compensation is possible, although extremely costly, by increasing investment in the schooling period. This approach can also be sustained by our type of modelling, adding a dynamic setting for child development investment decisions and adding the second period compensation which do not constraint the working decision of mother, but is highly dependent on the wealth of the household. These assumptions lead to a more credible

The interpretation is easy: once skills are set, the optimal human capital (schooling, although restrictive, can be a good approximation) and then wage actualized value is obtained comparing marginal benefits and costs from the choice. If the marginal opportunity cost for investment r is exogenously given equal for all agents, abilities alone model the choice through marginal benefits. The second key assumption relates to the determination of benefits. Quality δ is assumed to be a deterministic function of resources I that mother is willing to invest in child care, following a rule $\delta = g(I)$ with $g(0) = 0$ and $g' > 0 > g''$. Choosing resources I, mother can buy in the market a quality δ of the care service and the quality offered is increasing with the monetary investment. As an example, δ can be thought as the productivity of the resources in producing skills that are valuable in the h_c accumulation, due to the process of complementarity previously described.[7] In this setting EM is intended as a public policy requiring very low I, while the availability of high or low δ depends on the regime of the EM provision and on some exogenous elements as public spending or educational laws.

After normalizing to 1 the aggregate consumption price, the mother chooses I^*, C^*, l^* to

and dynamic framework, but also introduce more interpretation issues not arising in Heckman, due to fixed family resources assumption.

[7]Our idea is that in market can be found many levels of productivity, each associated with a *modality* of provision. It is arguable that the mother opting for self-performed care needs to buy some formal support to children development; baby sitting requires additionally a wage which sensibly increase if the educator is professionally trained. Pre-elementary private school requires an higher investment to cover tuition, transport and material costs. Although I can be modeled as Blau and Robbins (1988) or Blau and Currie (2006) as a negative component of hourly wages, we would like to stress here that investment is made in the direction of buying a quality of the service and not directly the care-time. The results, in both cases, converge.

solve:

$$
\begin{aligned}
& max_{I,l,C}\ \ U\left(\tfrac{[qh_c+b]}{(1+r)^2},\ \ l,\ \ C\right) \\
& st. \quad wh'L + W_f = C + I + \tfrac{1}{(1+r)^2}b \\
& h_c = h\left(h',\ \delta\right) \\
& \delta = g(I) \\
& L + l = 1 \\
& b \geq 0
\end{aligned} \tag{2}
$$

where L is working time, W_f is the family aggregate wealth also generated by other components earnings (if any) and b are bequests constrained to be null.[8]

Under well-behaved preferences and decreasing returns from quality in human capital production ($\partial h/\partial\delta \geq 0 > \partial^2 h/\partial\delta^2$), problem (2) boils down to the following maximization:

$$max_{I,l}\ \ U\left(h(h',\ g(I)),\ \ l,\ \ wh'(1-l) + W_f - I\right).$$

Necessary and sufficient conditions for the program are:

$$\frac{\partial U}{\partial h_c}\ \frac{\partial h}{\partial \delta}\ \frac{\partial g}{\partial I} = \frac{\partial U}{\partial C} \tag{3}$$

$$\frac{\partial U}{\partial l} = wh'\frac{\partial U}{\partial C}. \tag{4}$$

The previous equations exploit the tradeoff among investment and leisure in the mother choice rule. By equation (3), the choice of optimal investment balances from one side the marginal utility from child expected outcome, weighted by the child-specific returns from the

[8]Positive - non-binding constraint - bequests would not change significantly results as long as we are not interested in the effect of marginal cost r on investment strategies and the choice is represented as a static optimal solution for the first period.

production of human capital and the supply of quality elasticity, and from the other side the marginal utility of consumption. Equation (4) describes the working decision rule: marginal utility of leisure is compared to the wage opportunity of the mother (depending on her specific human capital h'), weighted by the marginal utility of consumption. We use marginal benefit from consumption as a link between the two equations. In what follows, we make a comparative static exercise, assuming that δ, w or h' and family wealth are exogenously moved, to see the prediction of the model. Two possible scenarios are analyzed to present schematically the results.

Scenario 1: *quality of EM is in line with $g(I)$ prediction.* As long as EM is a public policy of pre-school care, we expect low I and then low δ. A small investment generates high returns, by conditions on (1). An increase in family wealth generates, *cetris paribus*, a decrease in mother's labor supply, thus decreasing marginal value of consumption by (4). By (3) also utility returns from human capital must decrease, as an effect, in this scenario, of larger child care investments. Mothers from wealthy families facing valid market alternatives for EM will prefer not to work and to opt for a better (and more expensive) form of care. A similar result occurs if mother supply labor extremely inelastically and some exogenous shock occurs to increase h' levels or wages. Wealthier mothers are expected to buy less EM and instead to adjust I in favor or private valuable alternatives[9] while low labor elasticity makes the prediction even stronger.

[9] Child labor can be interpreted as an early child "care" solution implementing $\delta < 0$ (as the child develops some manual skills that have nothing to do with human capital accumulation) and mother select negative investments, i.e. the wage of child. For those children, returns from an additional unit of I invested in the production of skills and then human capital is extremely high. High level of those weights in the left hand of equation (3) calls for a situation where mother faces extremely low wage (or suffer from low h') if work supply is rigid or at the limit mother does not supply work. These conditions are likely to be met in many areas of LDC.

Scenario 2: *an exogenous shock in EM quality occurs, making it a valid alternative to other market δ suppliers, but the required investment I remains low.* The jump in supplied quality makes very costly for mother to invest in the alternatives because $\partial g/\partial I$ is low and I must be extremely high. From (3), if mother chooses the EM a lower marginal return from consumption is expected, but the pattern of variation is not always clear as investment in this case is contained. Holding h' fixed, mothers living in wealthier families (high W_f) find it convenient to enter the policy as predicted by eq. (3), while this convenience increases with the constrains on the side of family wealth (low W_f, as single parent households). Eq. (4) also suggests that an increase in h' or in female wages may decrease the impact of EM policy on the marginal benefits from leisure, increasing elasticity of labor supply. Welfare reforms in the direction of making mother working decision independent from child age, in combination with policies boosting the quality of EM could, exogenously, explain an increase in elasticity for maternal labor supply. Summing up, if EM quality (or presence in the care market) is reinforced, the model predicts that more constrained mothers have always stronger incentives to participate, although this difference partially vanishes when mother human capital increases. Moreover, returns from investments, marginal productivity of quality on the child's human capital production function and woman human capital could change the EM participation outcome according to resources.

In its ease, using standard assumption the model predicts the evidence previously shown. It provides an explanatory device for the effects of some key unobserved components in the empirical model (notably the family wealth, the mother wage and the returns to human capital) which causes biased estimations. On the other side, quality plays an extreme important role in shaping these effects.

3 Data and Identification Strategy

Estimating the effects of EM on labor outcomes requires strong variability of EM enrollment for at least part of the sample at hand. Current full EM enrollment for pupils aged 3 to 6 defines strong data requirement and an analysis based on past cohorts of students. The surveys *Formation, Qualification Professionnelle* for the years 2003 and 1993 (in brief FQP03 and FQP93) provided by INSEE are two separated cross sections of 39877 and 18300 individuals aged 18-65 (born between 1938 - 1985 and 1929 - 1976 respectively) in the surveyed years, covering nearly 0.2% and 0.1% of the French population. Information on after tax yearly wages is collected for all employed workers, along with job contracts features (as part-time status, number of months worked and so on) which allow us to build an hourly and monthly equivalent wage. Detailed education and training history of the surveyed (including EM attendance), along with socioeconomic status of the family of origin (SES) and the actual household structure is provided. Although the large sample size makes the FQP03 more appealing, the FQP93 reports some characteristics of surveyed otherwise excluded: the permanence in EM in years and a variable summarizing work bonuses, prizes and assets remuneration. An analysis based on both databases contributes in shaping the participation and duration of EM effects on the outcomes of interest. Table 1 in the Appendix summarizes the variables used in a within cohort perspective.[10]

[10]For the following analysis, only a subsample of employed men living in Continental France before they turned 6 years old is considered as our population of interest. Self-employment decision or woman labor market participation may be driven by some unobserved characteristics correlated with EM enrollment in the past, which in turn would lead to inconsistent estimations due to strong selection effects. Also, immigrants coming to France after the age of 6 are even not eligible to EM and so not considered in the population of reference. The final sample consists in nearly 11000 and 7000 observations for FQP03 and FQP93 respectively.

3.1 Heterogeneity Bias: an IV Solution

The most relevant market outcome we are interesting in is the market wage of employees (independently from his socio-professional group) both in a *cardinal* perspective, which captures the returns to EM for the average worker as a difference in linear or log wages, and in an *ordinal* perspective, in which case the single wage entry for each observation is compared to some thresholds levels as median, upper and lower quartile wage from the sample distribution. The former setting gives an average return, in absolute or relative values,[11] while the the latter setting focuses on changes in probability to end up above the median, above the 75th percentile or below the 25th percentile of the income distribution, attributable in a causal way to EM attendance. Other outcomes like contract types, unemployment or profession are interesting but the contribution of EM participation appears less clear and other unobserved factors may generate considerable biases in the EM effect, which would be also not comparable among different subgroups.[12]

In what follows, we treat the EM as a policy participation dummy variable and, in line with Heckman (1990), Angrist and Imbens (1991), Imbens and Angrist (1994) causal effects of EM are defined by the comparison of *potential outcomes* $Y_i(EM_i)$. $Y_i(1)$ is the outcome in the case where$i$ is enrolled in EM (so $EM_i = 1$) and $Y_i(0)$ is the counterfactual. In our case outcome Y_i can be either a measure of wage of individuals (in years, or month/hourly equivalent) or an

[11]Here the relative value comes form a log specification of labor income. Once multiplied by 100, the percentage rate of return to EM attendance is obtained.

[12]Firms characteristics and market power, being a unionized worker and other unobservables are relevant in the determination of job contracts and equilibrium wages, but often they constitute *a priori* conditions not comparable across jobs and economic sectors. Moreover, we don't expect that the EM has a predicting power for the profession choice. Is in fact reasonable to assume that worker chooses the category requiring tasks that better fit to own skills (in a wide sense) and only returns from this activity provide a meaningful measure. Bowles *et al.* (2001) make clear these motivations.

indicator for the position in the sample wage distribution. The former outcome allows to obtain a direct measure of rate of return from EM if a log linear relation is specified.[13] $Y_i(1)$ and $Y_i(0)$ are never observed simultaneously for the same i, so we must rely on the information given by EM participation, EM_i along with $Y_i = Y_i(0) + EM_i\,(Y_i(1) - Y_i(0))$ and compare the information supplied by different individual in estimating average treatment effects. The treatment effect would then amount to measure parametrically $E\left[Y_i(1) - Y_i(0)\right] = \alpha$. From one side, it is reasonable to think of an averaged effect for the whole population, as long as in the sample everyone was eligible for EM participation, while it would be more important for policy usages to determine an effect that is region or better cohort dependent (α_k effect). A conditional effect would capture also variations in the quality of EM supplied and may be related to central policy advices or founding rules in EM program under analysis.

The causality of the effects we are interested in depends crucially on the identification assumptions made: in an ideal *experimental* conception of the treatment, an homogeneous population (in terms of before participation characteristics) is chosen and treatment status (EM enrollment status) is randomly assigned among individuals. When outcomes are displayed, any difference among treated group and control group (who didn't take EM) is solely attributable to a gross effect of the EM participation. Unfortunately, our data do *not* display *any* of these appreciable experimental features. We must rely on some process describing the EM selection mechanism by families of origin and account for standard heterogeneity in the wage function.

Let $\mathbf{x'}_i = \left[\mathbf{x}_{i,ses}, \mathbf{x}_{conf}\right]'$ be two vectors of variables controlling for i-th SES background (like

[13] Here the wage return should be considered as the expected difference in logs among treated and non treated. Let w_i be the observation i wage, then $exp\{Y_i(1)\} = w_i(1) = (1+\alpha)w_i(0)$. Thus follows that $E\left[ln(w_i(1)) - ln(w_i(0))\right] = ln(1+\alpha) \approx \alpha$, where α is the treatment effect (assumed) equal for all is.

mother working status, number of siblings, nationality of parents and parental education) and other confounding variables affecting relevant outcome and credibly correlated with EM (marriage status of the surveyed, number of children). Given the appropriate dimensioned vector β, EM treatment effect on outcome Y_{icr} for observation i born in cohort c and living in region r is the coefficient α in a model:

$$Y_{icr} = \delta + \lambda_c + \lambda_r + \alpha EM_i + \mathbf{x}'_i \beta + u_{icr} \tag{5}$$

where λ_c and λ_r are cohort and region of living effects. α is estimated by OLS without bias if and only if we assume that controls in $\mathbf{x}'_i$ fully describe the selection process and then EM is unrelated to any component in the error term u_{icr}. This assumption amount in requiring that, conditional on sources of EM attendance status and after controlling for other heterogeneity affecting wages, the counterfactual outcomes expected by children who took EM and who didn't are identical. Identification comes through the observation of some characteristics of the family of origin. Is this requirement likely to hold in our data? Many concerns may arise, but two in particular impose a negative answer to the question.

Firstly, information of mother[14] working status during EM years is provided with error and only status *after* the educational cycle completion by the surveyed is observed. Let $MW_i^* = 1$ if mother was working during EM years and 0 otherwise, only $MW_i = MW_i^* + \epsilon_i$ is observed, where ϵ_i takes values 1, 0, -1 according to the status. $\epsilon \leq 0$ is a favourable outcome not inducing bias in estimations. If the probability that $\epsilon = 1$ is dependent on some unobserved[15] also explaining EM enrollment, than an attenuation bias is expected due to measurement

[14] And father, but the distinction is probably not interesting for double parent households.

[15] For example: age of mother, unemployment rate at the time of EM, number of siblings on family of origins and type of contracts and job category of the mother.

error.

Secondly, an unobserved heterogeneity bias arises. Family environment, wealth and mother's human capital are not observed, but are elements arguably positively correlated with wages. The direction of the bias depends on the effect produced by heterogeneity on EM enrollment of the child and the model previously presented sheds some light on the topic. In the first scenario, wealthier families are more likely to buy on market the required (high) quality of child care. Maternal human capital has an ambiguous effect but is likely to increase her job opportunities, thus increasing labor supply but at the same time generating an external positive effect on children skills production, making the investment in skills more convenient. In this case the bias in EM effect is definitely negative. The second scenario offer a less clear picture: larger availability of higher quality EM policy generates an incentive for mothers to enter the policy. The effect of heterogeneity on selection is then positive, although the intensity depend largely on the scale of heterogeneity. In this situation, a null bias is expected because eligibility to the policy has been historically motivated by equality in opportunities and treatments concerns, as an highly centralized program.[16] Blau (1999) suggests that the bias presented must also be considered in the perspective of the compensation policies taking place in the Country and that only permanent income has considerable effects on child development, while transitory component is almost irrelevant. At this stage of the analysis, selection bias is accepted to be prevalently negative and a source of identifying information is needed. An instrument (IV hereafter) Z_i with a finite support provides this

[16] A positive bias could be sustained in a multiple periods education investment setting. Wealthier mothers are in fact more willing (and have the possibility) to invest more and more effectively, in subsequent periods of education, once skills are formed. It is also credible that in presence of high quality public provision these mothers are more willing to select EM and save resources, otherwise used to buy care in the market, for following periods investments. Despite that, first scenario bias is still predicted as negative.

information. What must be required is that EM participation status depends on the IV's realizations ($EM_i \equiv EM_i(Z_i)$) but at the same time the IV is randomly assigned among individuals *and* independent from both counterfactuals $(Y_i(0), Y_i(1))$. Choice is hardly justifiable if discontinuities (like EM enrollment rules) or quasi-natural experimental evidence are not found. Any IV related in some way to quality (as complementarity in educational techniques with following primary grades, auxiliary services offered, class sizes and educators quality) would fail these requirements as it would affect strongly the outcome in case of participation while it would not in the alternative case. If quality also relates to the region in which EM is taken and there is cohort-dependence of EM policies through financing and promotion (more industrialized areas show historically a relatively more sustained presence of institutes), random assignment of the IV is far from sustainable. Under the assumption that quality and financing effects are constants for all individuals who attended the EM in the same cohort-region group, model (5) captures with λ_c and λ_r all the variation in outcomes associated with these components. Similar controls in the selection equation solve the endogeneity problems related to an IV as the one considered. The source of identification used here consists in an index of the share of people in a region-cohort group attending EM. We suggest that under the previous necessary assumption, the IV is exogenous and, as long as it embodies the choice of other people in the same region-cohort group, orthogonal to family of origin heterogeneity in wealth and human capital. The provided assumptions along with the constant (not individual) treatment assumption are sufficient to guarantee identification (Imbens and Angrist, 1994).

4 Results and Discussion

4.1 Enrollment Effect

To measure *enrollment* effect of EM, the FQP03 data are used. The direct effect of EM before controlling for disturbing effects is controversial and often not significant. Table 2 reports in column (1) the effect of EM participation on a series of outputs, not only related to wages. This table synthethizes a series of results well known in literature. The negative relation with wages before any control is obvious. Children born in recent cohorts in the sample face an higher probability of being enrolled in EM with respect to predecessors, while lack of experience impacts negatively on wages. Focusing on returns from yearly wages, controlling for region alone reinforce this relation while when also cohort fixed effect is introduced,[17] EM returns are found to be 8.7%. This result is robust to monthly or hourly wage equivalents, obtained after considering the employee's type of contract. The change in probability of ending up in the top 50% of the distribution is also coherent with expectations. Meaningful effects are also found for probability of upper and lower quartile positioning. The effects on educational variables is right signed and consistent with findings by other cited studies on US evidence.[18]

Table 3 focuses on the objective of income returns but controlling for the background of origin with three categories: parents education, parents work-related information and family

[17]Interaction effects among controls are not significant and can be excluded from the model. Region of birth is also highly collinear to region of residence at the time of the survey. Nearly 30% of the sample moved, and in the great majority of cases to neighbor regions. A dummy with value 1 if region of birth is different from region in 2003 is used along with a dummy to signal the movement to the Paris area.

[18]For schooling and primary repetition, EM effects amount at a 0.63 more years and a 3.5% decrease in repetition probability. The effect on repetition of secondary grades is instead wrongly signed, because secondary grades enrollment has increased, through cohorts, with EM and so has the probability of repeating one year for this sub population of interest (sample units who at least took some secondary schooling).

environment. If any other source of heterogeneity in wages is considered as irrelevant for EM choice, then the selection mechanism is fully exploited and returns can be interpreted in a causal sense. The EM effect on wages in linear specification is correctly signed but highly non significant because of the strong skewness in income distribution. For log wage specification and distributional position probabilities, the effect is always rightly signed. The stronger selection effect is played by variables related to parents education and working status. EM returns on yearly wages are sensibly reduced from roughly 9% (column 1) to 5.1%, which is slightly lower than the monthly or hourly specification (5.4% and 5.8%, by column 8). A similar pattern is found for positional information. Probability of positioning in the top 25% of the distribution is also not significantly affected by EM. Moving from columns 2 to 7, the substantial variability in returns signals what type of selection bias is playing a role in attribution of average returns. After controlling for parental education and family background (non French nationality of parents, dimension of the family), returns decrease (col. 5) indicating that positive selection bias was in action: more educated parents have a positive impact on education choices and future wage profiles; numerous non-French families may have an incentive to opt for self-care and, as long as educational resources must be divided among more siblings, a negative impact on wages is expected thus justifying positive bias. *Ceteris paribus*, introducing working status (col. 8) generates a moderate increase in returns, probably due to higher family wealth impact on expected wages. This simple reasoning alone highlight the problem of unobserved heterogeneity.

Until here, gross returns from EM are estimated. EM affects schooling decisions through the ability channel, also affecting repetitions. Moreover, education is an important predictor of wages which *must* be accounted for. Coherently with literature on returns to education (see

Card, 2001), years of schooling are endogenous in this model because generated by some ability factors that in general affect economic success too. Anyway, the endogeneity bias does not propagate to EM returns, as long as child's EM enrollment never occurs (by law) as a function of child's inherited abilities, leaving apart pathological cases. The IV specified in the previous section is used in Table 4 to provide credible identification information. We focus on log yearly income and hourly equivalent for ease of exposition. Effects on income linear specification are not significant. Returns to EM after controlling for schooling (col. 2) are between one half and one third of the previous estimates and always not significant (apart from hourly equivalent wage). Conversely, education has a strongly significant and correctly signed effect on all the outcomes. Instrumenting EM by the region-cohort specific enrollment rates leads to very different conclusions as the influence of unobservables, like family wealth or mother human capital, is eliminated. Columns 4 and 5 in Table 4 show the two stage procedure models with the full set of controls, alternatively controlling for educational measures. The causal effect of EM on wages amounts to 18% without considering schooling, while when education level is controlled for, returns to EM for yearly wages lowers to 14.1%. Slightly higher returns are found for hourly equivalent wage measure. Estimates are sensibly higher than the previous ones, confirming the negative selection bias produced by measurement error and heterogeneity.[19] These high estimates are also justifiable from an economic point of view. Eliminating, under IV assumptions, the effect of wealth, a causal analysis suggest that EM is important *per se* in explaining wages, as it provides a substitute environment for children early development. As Section 2.1 model shows, families with a poor SES have always the interest to select the policy because otherwise children cannot buy

[19] Houseman tests force to reject the null hypothesis of exogeneity of EM at 1% confidence level.

an alternatively valid environment. As a result, returns from investment in those children must be considerable.[20] A similar pattern, again, is found for the positional variables. The effect is correctly signed, as a larger probability of ending up in the last quartile or above the median and a lower probability to end up in the first 25% of distribution is found. Notwithstanding, results are statistically null at conventional levels of significance. The efficient procedure suggested by Wooridge (2002) leads to the same conclusions.

Thanks to the big sample size of FQP03 dataset, it is possible to exploit whether the EM effect varies between cohorts, and within cohorts for some groups of interest. After grouping cohorts for 5 years intervals, EM treatment effects $\alpha + \alpha_c$ can be inferred from Table 5 for each of the 8 groups. We do not use IV procedure in this estimation, as a more robust specification of the identification source is needed, but the results are enough to have an intuition on how the effect evolves. Without any control, EM effect is very large for early cohorts, around 20%, and lowers down significantly with cohorts until a negative return is estimated. When controls are added, estimates are fully unreliable and the underlayng pattern becomes less clear. For younger workers in the sample, returns amount to -3% while for people born around 1970-1975 the return is 15%. F-test also forces to accept the equality of effects hypothesis. The situation is more interesting looking at the positional information. EM predicts a change of 20% in probability of being above the median for later cohorts, while this effect sensibly decreases for younger cohorts, although still positive (it is only 4.3% for people born in central cohorts). While EM seems to have no effect on the probability of belonging to the bottom of income distribution (col. 12), it has a very strong effect on probability of being at the top, as shown by col. 16. This result is surely affected by

[20] It is worth remembering that EM effect is a measure a difference among potential outcomes.

some unobserved characteristics. Despite the poorly causal interpretation of the results, it is possible that EM has had a powerful effect in shaping income success, specially for earlier cohorts when full enrollment was not the norm.

Table 6 and 7 provide estimation, after controlling for schooling, of EM effects for some groups of interest. With these groups we would like to exploit differences, if any, due to working status, education of the mother and family of origin components. We build an interaction indicator for each group and EM selection, and in the table are reported, for separate outcomes the effects of each group taken alone (col.s 1 to 7) and in the case of a comprehensive model (col. 8). For all outcomes considered, joint significance is always rejected besides single group effects. For log wages, the only significant result at conventional levels is found for the group of children with educated working mother coming from a small family: for them the effect of EM is -1.2% suggesting that the policy has not supplied a valid alternative to home environment. For all the model specifications, mother working status always impact positively on returns to EM with respect to the case in which mother is not working, but this effect is much higher when family background is characterized by low educated mothers with big families (rows 1 and 2) with respect to the case of big families with high educated mother (rows 3 and 4). For small families with high educated mother, the effect is negative (rows 5 and 6) and the difference attributable to maternal working status is more sensible. Patterns of estimated effects slightly change for other outcomes. Again, taking fixed other discriminating characteristics, maternal working status always affect in a more positive (and intensive) way the outcome at hand, although this effect varies considerably among different groups. The only significant pattern found in this figure is the one connected with *Group 5*, which is associated with lower return to EM both on income or on the likelyhood to end up at the top of the distribution (or not at the bottom). This group matches families with fewer constraints than other, for whom investments in future education possibilities are more sustained and where mother alone may offer a good alternative environment. The result is consistent with theory on early child compensation, while other types of family structures seems to have an higher but homogeneous incidence (depending on mother working status) on returns to EM. This finding also suggests that a more close look to family constraint and EM choice for well off families is needed, as log as a better understanding of alternative care choices faced by these groups.

4.2 Duration Effect

This section uses FQP93 data to exploit the effect of EM *duration*. For ease of exposition, only log yearly wages and position above median income are considered, while a dummy for wage prize takers[21] is used. By Table 7, the effect on the prize is always not significant, suggesting that the channel of EM has very low predicting power for some extra-wage components which are arguably explained by incentive enhancing mechanisms, luck or to sector-specific skills and contracts. Differently, EM predicts significant variations in wages. Let alone, the larger effect is found for observations whom duration in EM was less than 3 years with a return of 7.3% after controlling for disturbing variables. F-test forces to accept equality of these affects. The same pattern emerges in relation to probability of end in top 50% of the distribution, although the unexpected sign. Equality of duration effects can be rejected at 12% confidence level. This result is counterintuitive as higher returns are ex-

[21] A dummy varible taking value 1 if bonuses, extra months earnings and benefits are recived by i-th observation during 1993, 0 otherwise.

pected for people who stay longer in EM. Table 8 helps in solving this puzzle by controlling for SES of the family environment (the definition is the same as in the previous analysis) and education, recovering a source of identification through an IV strategy: under previous assumptions, for each duration dummy is associated a cohort-region average duration IV capturing behavior of other subjects in the sample, supposing no relation among average rate of duration and individual outcomes. OLS and PROBIT models (col.s 1, 3 and 5) find no significant effect for each EM duration dummy. Nevertheless, controlling for selection variables lead a correct estimation of intensities (3.3% if duration is lower than 2 years while less than 16% for full 4 years participation) but these effects are statistically equal. IV estimation leads to similar conclusions. Two problems may affect estimation: for one side, it is not possible to distinguish what years have been spent in EM, while an ordering of duration effects strongly relies on the fact that once entered the policy, it is never left. Also, few elements in defense of this specific set of IVs can be found.

5 Conclusions

Making use of two large cross sections of individuals we estimate EM participation effects on wages and economic success. If EM treatment is assumed as randomly assigned, treated people must expect on average a return of 8.7% from treatment and an increase of 2.4% in the probability of ending up in the top quartile of income distribution. Returns may be underestimated because of selection bias: for many cohorts in the sample, wealthier families had an incentive to opt for alternative market-provided forms of care while EM has represented a valid substitute to more disadvantaged family background. Under the assumption of fixed cohort-region effects on wages due to quality supply, we use the region-cohort specific enrollment rate as a source of identifying information. Controlling for the selection process covariates and for schooling, IV returns to EM significantly rise to 14.1% for wages, while positive but not significant variations in positional success are expected. Cohorts specific returns does not follow a clear pattern while significantly lower returns to EM are associated with more advantaged families. These results shed light on the long run treatment effect of EM policy and allow to account for another source of indirect benefits from this long-lasting and unique care service. Although external validity of results is problematic (estimation relies on French job market), the estimates are particularly useful in driving widespread investments in early care programs for Developing Countries, where child potential is often neglected.

As a purpose for future research on the topic, we suggest that more investment on the theoretical investigation could offer a better prediction of the bias direction and the selection mechanisms operating through family wealth mobility and maternal human capital. In this direction, identification conditions must be specified in a clearer way, relying on a more robust IV strategy and a exact definition of what is economic success. Wage alone is a powerful proxy for economic status, but it completely misses all the external effects (from indirect benefits toward mother work experience, to socially valuable skills accumulation) that EM participation may induce in children otherwise condemned innocently to compensate later for early life environment in a costly, and often unsuccessful, way. Moreover, EM quality must be redefined, especially in the optic of regional or within city differentials in supply of heterogeneous EM environments which could be correlated, by mean of the local financing policy connection, with segregation phenomena and the unevenness and persistence of parental human capital spatial distribution.

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A Tables and Figures

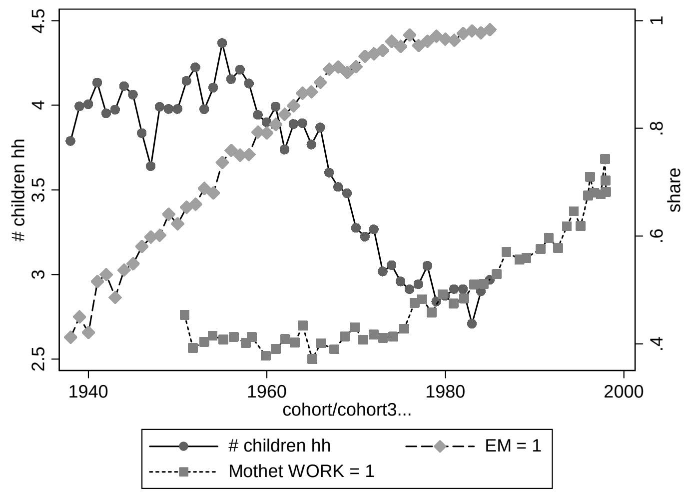

Figure 1: Cohort averaged EM partecipation, mother labor supply and hh structure

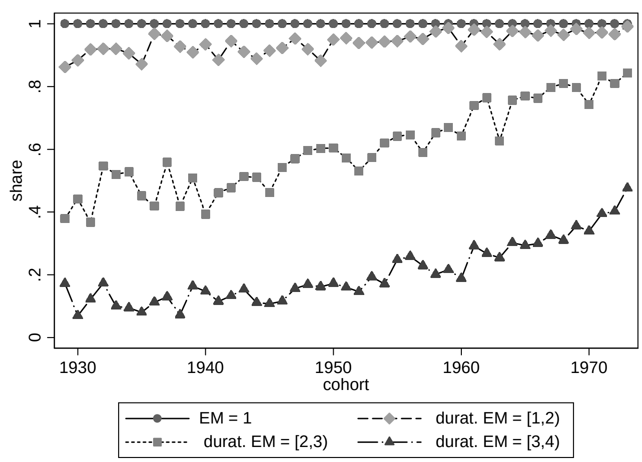

Figure 2: Cohort averaged EM duration, share of total partecipants

Survey	Mean	Stand. Dev	P<25%	Median	P<75%	N	Range
FQP03							
EM = 1	0.815	0.388	1	1	1	11646	{0, 1}
Wage (year)	22905.400	37778.290	13049	18000	25202	11735	cont.
Wage (year) – log	9.753	0.764	9.476467	9.798127	10.13468	11735	cont.
Low 25% = 1	0.146	0.353	0	0	0	11735	{0, 1}
Top 50% = 1	0.618	0.486	0	1	1	11735	{0, 1}
Top 75% = 1	0.335	0.472	0	0	1	11735	{0, 1}
Education	12.695	3.362	10.75	11.91667	14.75	11427	[0,28]
Repet. Primary	0.419	0.707	0	0	1	11662	[0, 8]
Repet. Secundary	0.598	0.720	0	0	1	10187	[0, 6]
Educ mother high = 1	0.299	0.458	0	0	1	11621	{0, 1}
Mother work = 1	0.474	0.499	0	0	1	11733	{0, 1}
# siblings	2.634	2.189	1	2	4	11735	[0, 19]
Mother not french	0.088	0.283	0	0	0	11623	{0, 1}
Same region = 1	0.657	0.475	0	1	1	11735	{0, 1}
Cohorts	-	-	-	-	-	-	1938 – 1983
Regions	-	-	-	-	-	-	1 – 22
FQP93							
EM = 1	0.696	0.460	0	1	1	5414	{0, 1}
EM < 1	0.033	0.178	0	0	0	5414	{0, 1}
EM < 2	0.208	0.406	0	0	0	5414	{0, 1}
EM < 3	0.297	0.457	0	0	1	5414	{0, 1}
EM = 3 or more	0.159	0.365	0	0	0	5414	{0, 1}
Wage (year)	117696.600	115129.600	72000	98000	136425	5414	cont.
Wage (year) – log	11.457	0.705	11.18442	11.49272	11.82353	5414	cont.
Top 50% = 1	0.627	0.484	0	1	1	5414	{0, 1}
Prize = 1	0.330	0.470	0	0	1	5414	{0, 1}
Edcucation	11.544	3.725	8.75	10.75	12.83333	5390	cont.
Repet. Primary	0.579	0.813	0	0	1	5414	[0, 8]
Repet. Secundary	0.388	0.623	0	0	1	5414	[0, 6]
Mother Educ. (years)	4.073	4.593	0	5	5	5303	[0, 18]
Mother work = 1	0.437	0.496	0	0	1	5414	{0, 1}
# siblings	2.940	2.370	1	2	4	5408	[0, 18]
Mother not french	0.116	0.320	0	0	0	5414	{0, 1}
Same region = 1	0.656	0.475	0	1	1	5414	{0, 1}
Cohorts	-	-	-	-	-	-	1929 – 1973
Regions	-	-	-	-	-	-	1 – 22

Table 1: Summary Statistics, by FQP03 and FQP93

Dep variable	OLS (1)	(2)	(3)	(4)	(5)	Obs.
W	-1.699	-1.774	0.808	-0.731	0.525	11603
	(0.749)**	(0.758)**	(0.652)	(0.740)	(0.662)	
Wm	-284.691	-299.941	137.084	-116.793	88.160	11603
	(128.679)**	(130.344)**	(112.332)	(127.418)	(114.129)	
Wy	-2,751.341	-2,917.872	1,595.571	-825.598	1,074.274	11649
	(965.505)***	(972.882)***	(1,094.071)	(1,044.571)	(1,093.020)	
lnW	-0.053	-0.057	0.100	0.014	0.088	11603
	(0.016)***	(0.016)***	(0.016)***	(0.016)	(0.016)***	
lnWm	-0.054	-0.058	0.098	0.016	0.086	11603
	(0.016)***	(0.016)***	(0.016)***	(0.016)	(0.016)***	
lnWy	-0.077	-0.080	0.097	0.007	0.087	11649
	(0.018)***	(0.018)***	(0.018)***	(0.018)	(0.018)***	
Top 50%	-0.027	-0.032	0.084	0.030	0.074	11649
	(0.011)**	(0.012)***	(0.013)***	(0.013)**	(0.014)***	
Unemp.	-0.003	-0.006	-0.003	-0.017	-0.009	13066
	(0.01)	(0.01)	(0.01)	(0.006)***	(0.01)	
Unemp. Dur.	-1.053	-1.056	-0.294	-0.809	-0.433	995
	(0.191)***	(0.191)***	(0.21)	(0.207)***	(0.210)**	
Part time	0.004	0.004	0.003	-0.001	0.004	11603
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	
Educ.	1.787	1.773	0.803	1.062	0.625	14929
	(0.068)***	(0.069)***	(0.072)***	(0.071)***	(0.073)***	
# rep prim.	-0.233	-0.261	-0.095	-0.138	-0.087	16277
	(0.016)***	(0.018)***	(0.017)***	(0.017)***	(0.019)***	
# rep sec.	0.151	0.145	0.098	0.116	0.088	13476
	(0.016)***	(0.017)***	(0.017)***	(0.016)***	(0.018)***	
Rep pr. = 1	-0.102	-0.114	-0.043	-0.057	-0.035	16277
	(0.009)***	(0.009)***	(0.010)***	(0.010)***	(0.011)***	
Rep se. = 1	0.111	0.106	0.067	0.087	0.061	13476
	(0.011)***	(0.012)***	(0.012)***	(0.012)***	(0.014)***	
CONTROLS						
Region	**n**	**y**	**n**	**n**	**y**	
Cohort	**n**	**n**	**y**	**n**	**y**	
Nationality	**n**	**n**	**n**	**n**	**y**	
Confounding	**n**	**n**	**n**	**y**	**y**	

Table 2: Returns form EM on Different Outcomes (by column)

Note: Marginal Effects of EM partecipation variable on a set of outcomes, by controls.
* significant at 10%; ** significant at 5%; *** significant at 1%.
Robust standard errors in parentheses.

	OLS							
Dep variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
W	0.660	0.140	0.548	0.645	0.156	0.143	0.532	0.153
	(0.68)	(0.69)	(0.64)	(0.68)	(0.65)	(0.71)	(0.65)	(0.66)
Wm	110.709	24.649	93.861	106.985	28.245	23.820	89.771	26.409
	(116.39)	(119.67)	(109.95)	(117.21)	(111.22)	(122.62)	(112.53)	(114.26)
Wy	1304.836	357.263	997.436	1277.666	287.253	372.803	985.089	295.094
	(1120.58)	(1146.69)	(1090.19)	(1104.92)	(1101.51)	(1147.67)	(1094.49)	(1110.57)
lnW	0.096	0.060	0.078	0.098	0.054	0.064	0.081	0.058
	(0.016)***	(0.016)***	(0.016)***	(0.016)***	(0.016)***	(0.016)***	(0.016)***	(0.016)***
lnWm	0.094	0.058	0.075	0.096	0.052	0.061	0.077	0.054
	(0.016)***	(0.016)***	(0.016)***	(0.016)***	(0.016)***	(0.016)***	(0.017)***	(0.016)***
lnWy	0.093	0.056	0.070	0.096	0.048	0.060	0.073	0.051
	(0.018)***	(0.018)***	(0.018)***	(0.018)***	(0.018)***	(0.018)***	(0.018)***	(0.018)***
Top 50%	0.079	0.052	0.059	0.079	0.045	0.053	0.060	0.045
	(0.014)***	(0.014)***	(0.014)***	(0.014)***	(0.014)***	(0.014)***	(0.014)***	(0.014)***
Lower 25%	-0.030	-0.018	-0.017	-0.028	-0.015	-0.017	-0.016	-0.013
	(0.010)***	(0.009)**	(0.009)*	(0.010)***	(0.01)	(0.009)*	(0.009)*	(0.01)
Top 75%	0.066	0.033	0.047	0.068	0.026	0.036	0.050	0.029
	(0.012)***	(0.012)***	(0.012)***	(0.012)***	(0.013)**	(0.013)***	(0.012)***	(0.013)**
CONTROLS								
A	**y**	**y**	**y**	**y**	**y**	**y**	**y**	**y**
B	**n**	**y**	**n**	**n**	**y**	**y**	**n**	**y**
C	**n**	**n**	**y**	**n**	**y**	**n**	**y**	**y**
D	**n**	**n**	**n**	**y**	**n**	**y**	**y**	**y**

Table 3: Returns to EM, Controlling for Confounding and Selection Covariates

Note: Treatment Effects of EM partecipation variable on a set of outcomes, by controls: confounding and cohort-region (A), family background varibles and mother human capital (B and C); mother working status (D).
* significant at 10%; ** significant at 5%; *** significant at 1%.
Robust standard errors in parentheses.

Dep variables	OLS (1)	OLS (2)	IV (3)	IV (4)	IV (5)	EFF. IV (6)	EFF. IV (7)
lnW							
EM	0.057	0.032	0.359	0.222	0.175	0.287	0.174
	(0.016)***	(0.015)**	(0.067)***	(0.073)***	(0.072)**	(0.063)***	(0.062)***
Education	-	0.053	-	-	0.052	-	0.052
		(0.002)***			(0.002)***		(0.002)***
lnWy							
EM	0.051	0.025	0.337	0.187	0.141	0.261	0.148
	(0.018)***	(0.02)	(0.076)***	(0.082)**	(0.081)*	(0.070)***	(0.070)**
Education	-	0.055	-	-	0.055	-	0.055
		(0.003)***			(0.003)***		(0.003)***
Top 50%							
EM	0.037	0.022	0.179	0.071	0.032	0.087	0.017
	(0.012)***	(0.012)*	(0.048)***	(0.05)	(0.05)	(0.045)*	(0.05)
Education	-	0.029	-	-	0.029	-	0.029
		(0.001)***			(0.002)***		(0.002)***
Lower 25%							
EM	-0.011	-0.006	-0.078	-0.053	-0.053	-0.077	-0.055
	(0.01)	(0.01)	(0.036)**	(0.04)	(0.04)	(0.033)**	(0.04)
Education	-	-0.006	-	-	-0.006	-	-0.006
		(0.001)***			(0.001)***		(0.001)***
Top 75%							
EM	0.122	0.040	0.919	0.343	0.173	0.613	0.284
	(0.048)**	(0.05)	(0.198)***	(0.21)	(0.21)	(0.185)***	(0.18)
Education	-	0.176	-	-	0.175	-	0.175
		(0.006)***			(0.006)***		(0.006)***
CONTROLS							
A	**y**	**y**	**y**	**y**	**y**	**y**	**y**
B	**y**	**y**	**y**	**y**	**y**	**y**	**y**
C	**y**	**y**	**n**	**y**	**y**	**y**	**y**

Table 4: Treatment Effects for EM and Returns from Schooling

Note: cohort, region, nationality effects and confoundings (A); SES controls (B); mother working status (C). Woolridge (2002) efficient and consistent estimator is reported in column (6) and (7). Probit estimation of EM attendance on the set of controls and IV is used as an instrument for EM partecipation in the second step.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Robust standard errors in parentheses.

	lnW, year				Top 50%			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EM	-1.278	-0.228	-0.279	-0.266	-0.498	0.883	0.897	0.896
	(0.046)***	(0.249)	(0.260)	(0.274)	(0.014)***	(0.022)***	(0.021)***	(0.026)***
EM (1938 – 45)	1.449	0.307	0.300	0.283	0.391	-0.661	-0.675	-0.695
	(0.070)***	(0.261)	(0.272)	(0.285)	(0.007)***	(0.007)***	(0.007)***	(0.008)***
EM (1946 – 50)	1.470	0.347	0.366	0.317	0.450	-0.747	-0.766	-0.779
	(0.050)***	(0.253)	(0.263)	(0.277)	(0.008)***	(0.016)***	(0.016)***	(0.019)***
EM (1951 – 55)	1.429	0.322	0.344	0.308	0.454	-0.759	-0.776	-0.787
	(0.049)***	(0.252)	(0.263)	(0.277)	(0.008)***	(0.017)***	(0.017)***	(0.020)***
EM (1956 – 60)	1.387	0.319	0.325	0.282	0.467	-0.786	-0.805	-0.815
	(0.047)***	(0.252)	(0.262)	(0.276)	(0.009)***	(0.019)***	(0.019)***	(0.022)***
EM (1961 – 65)	1.340	0.321	0.317	0.277	0.475	-0.816	-0.837	-0.842
	(0.047)***	(0.252)	(0.262)	(0.276)	(0.011)***	(0.022)***	(0.021)***	(0.025)***
EM (1966 – 70)	1.239	0.253	0.248	0.227	0.469	-0.829	-0.849	-0.853
	(0.047)***	(0.254)	(0.265)	(0.279)	(0.012)***	(0.023)***	(0.022)***	(0.026)***
EM (1970 – 75)	1.143	0.444	0.431	0.414	0.436	-0.804	-0.825	-0.828
	(0.047)***	(0.266)*	(0.274)	(0.287)	(0.014)***	(0.023)***	(0.022)***	(0.027)***
EM (1976 – 80)	0.765	0.345	0.300	0.235	0.320	-0.752	-0.773	-0.783
	(0.051)***	(0.274)	(0.287)	(0.304)	(0.021)***	(0.018)***	(0.018)***	(0.020)***
P-value (F-etst)	*0*	*0.75*	*0.65*	*0.67*	*0*	*0.43*	*0.45*	*0.29*
Obs.	11646	11646	11169	10890	11646	11646	11169	10881
R2	0.15	0.22	0.25	0.28	-	-	-	-
	Lower 25%				**Top 75%**			
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
EM	0.202	-0.174	-0.173	-0.168	-0.729	0.627	0.581	0.650
	(0.006)***	(0.189)	(0.185)	(0.174)	(0.045)***	(0.044)***	(0.051)***	(0.011)***
EM (1938 – 45)	-0.133	0.161	0.196	0.199	0.694	-0.348	-0.340	-0.356
	(0.004)***	(0.214)	(0.222)	(0.214)	(0.016)***	(0.008)***	(0.008)***	(0.005)***
EM (1946 – 50)	-0.162	0.115	0.130	0.137	0.733	-0.438	-0.419	-0.459
	(0.004)***	(0.186)	(0.188)	(0.181)	(0.022)***	(0.021)***	(0.023)***	(0.008)***
EM (1951 – 55)	-0.167	0.162	0.180	0.176	0.730	-0.453	-0.430	-0.472
	(0.004)***	(0.203)	(0.205)	(0.195)	(0.026)***	(0.023)***	(0.025)***	(0.008)***
EM (1956 – 60)	-0.177	0.128	0.135	0.136	0.724	-0.479	-0.456	-0.507
	(0.005)***	(0.189)	(0.188)	(0.178)	(0.032)***	(0.027)***	(0.030)***	(0.009)***
EM (1961 – 65)	-0.188	0.172	0.212	0.213	0.726	-0.519	-0.492	-0.550
	(0.005)***	(0.201)	(0.208)	(0.199)	(0.037)***	(0.034)***	(0.038)***	(0.011)***
EM (1966 – 70)	-0.188	0.176	0.202	0.202	0.695	-0.548	-0.522	-0.585
	(0.005)***	(0.201)	(0.206)	(0.196)	(0.046)***	(0.036)***	(0.041)***	(0.012)***
EM (1970 – 75)	-0.177	0.044	0.085	0.082	0.641	-0.522	-0.495	-0.547
	(0.005)***	(0.153)	(0.168)	(0.156)	(0.055)***	(0.034)***	(0.038)***	(0.013)***
EM (1976 – 80)	-0.130	0.152	0.167	0.172	0.495	-0.421	-0.402	-0.441
	(0.005)***	(0.210)	(0.213)	(0.206)	(0.077)***	(0.028)***	(0.031)***	(0.018)***
P-value (F-etst)	*0*	*0.627*	*0.77*	*0.75*	*0*	*0.61*	*0.52*	*0.68*
Obs.	11646	11380	10922	10551	11646	11380	10922	10551
CONTROLS								
A	**n**	**y**	**y**	**y**	**n**	**y**	**y**	**y**
B	**n**	**y**	**y**	**y**	**n**	**y**	**y**	**y**
C	**n**	**n**	**y**	**y**	**n**	**n**	**y**	**y**
Educ.	**n**	**n**	**n**	**y**	**n**	**n**	**n**	**y**

Table 5: Five Years Cohort-specific EM Effects, by Outcome and Model

Note: cohort, region, nationality effects and confoundings (A); SES controls (B); mother working status (C). *P-value* of F-test on multiple linear restriction for nulle effects ia reported in italics.
* significant at 10%; ** significant at 5%; *** significant at 1%.
Robust standard errors in parentheses.

Work	Educ.	Sib>2	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			lnW year							
EM dummy			0.023	0.032	0.026	0.027	0.030	0.027	0.025	0.023
			(0.018)	(0.019)*	(0.017)	(0.017)	(0.017)*	(0.017)	(0.018)	(0.028)
1	0	1	0.034							0.046
			(0.025)							(0.043)
0	0	1		-0.015						-0.007
				(0.021)						(0.025)
1	1	1			0.047					0.036
					(0.040)					(0.057)
0	1	1				-0.007				-0.032
						(0.035)				(0.043)
1	1	0					-0.042			-0.027
							(0.024)*			(0.044)
0	1	0						-0.006		-0.034
								(0.031)		(0.037)
1	0	0							0.012	0.023
									(0.021)	(0.038)
			Top 50%							
EM dummy			0.030	0.029	0.032	0.033	0.033	0.031	0.026	0.023
			(0.015)**	(0.016)*	(0.014)**	(0.014)**	(0.014)**	(0.014)**	(0.015)*	(0.022)
1	0	1	0.012							0.028
			(0.021)							(0.034)
0	0	1		0.008						0.010
				(0.018)						(0.021)
1	1	1			-0.037					-0.043
					(0.033)					(0.046)
0	1	1				-0.040				-0.059
						(0.033)				(0.039)
1	1	0					-0.022			-0.020
							(0.020)			(0.035)
0	1	0						0.015		-0.012
								(0.025)		(0.031)
1	0	0							0.025	0.032
									(0.017)	(0.030)

Table 6: EM Treatment Effects for SES Group: *log wage* and *Above Median Status*

Note: EM effects after controlling for A, B, C and Education. Groups are defined as: mother works, mother with high education and big family (# siblings > 2). EM is the effect for people not belongig to the groups in the figure.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Robust standard errors in parentheses.

Work	Educ.	Sib>2	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Lower 25%							
EM dummy			-0.008	-0.013	-0.010	-0.010	-0.012	-0.010	-0.010	-0.014
			(0.009)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.014)
1	0	1	-0.020							-0.010
			(0.012)*							(0.020)
0	0	1		0.008						0.003
				(0.011)						(0.013)
1	1	1			-0.017					0.002
					(0.017)					(0.027)
0	1	1				-0.005				0.008
						(0.017)				(0.022)
1	1	0					0.030			0.034
							(0.013)**			(0.024)
0	1	0						-0.008		0.009
								(0.014)		(0.019)
1	0	0							-0.002	0.006
									(0.010)	(0.019)
			Top 75%							
EM dummy			0.009	0.016	0.011	0.014	0.016	0.012	0.009	0.011
			(0.014)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.022)
1	0	1	0.032							0.038
			(0.023)							(0.037)
0	0	1		-0.011						-0.006
				(0.018)						(0.021)
1	1	1			0.066					0.043
					(0.034)*					(0.047)
0	1	1				-0.045				-0.065
						(0.028)				(0.033)**
1	1	0					-0.050			-0.040
							(0.019)***			(0.033)
0	1	0						0.017		-0.019
								(0.025)		(0.028)
1	0	0							0.015	0.019
									(0.018)	(0.031)

Table 7: EM Treatment Effects for SES Group: *Wage Above Top Quartile* and *Wage Below Lower Quartile*

Note: EM effects after controlling for A, B, C and Education. Groups are defined as: mother works, mother with high education and big family (# siblings > 2). EM is the effect for people not belongig to the groups in the figure.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Robust standard errors in parentheses.

	ln W, year		Top 50%		Prize	
	OLS		**PROBIT**		**PROBIT**	
	(1)	(2)	(3)	(4)	(5)	(6)
EM dummy	-2.361	-2.336	-0.942	-0.916	-0.133	-0.199
	(0.016)***	(0.093)***	(0.004)***	(0.006)***	(0.335)	(0.252)
EM < 1	2.355	2.381	0.453	0.424	0.081	0.193
	(0.046)***	(0.101)***	(0.007)***	(0.008)***	(0.329)	(0.217)
EM < 2	2.369	2.409	0.835	0.810	0.048	0.178
	(0.021)***	(0.094)***	(0.007)***	(0.009)***	(0.341)	(0.243)
EM < 3	2.258	2.408	0.935	0.928	0.025	0.165
	(0.018)***	(0.093)***	(0.004)***	(0.006)***	(0.345)	(0.252)
EM 3 or more	2.148	2.374	0.738	0.721	0.043	0.180
	(0.027)***	(0.095)***	(0.008)***	(0.009)***	(0.341)	(0.238)
R2	0.01	0.24	-	-	-	-
P-value (F-test)	*0*	*0.55*	*0.005*	*0.12*	*0.181*	*0.492*
Effect of EM duration (%)						
EM < 1		**4.5**		**-49.2**		**-0.6**
EM < 2		**7.3**		**-10.6**		**-2.1**
EM < 3		**7.2**		**1.2**		**-3.4**
EM 3 or more		**3.8**		**-19.5**		**-1.9**
CONTROLS						
Confoundings	**n**	**y**	**n**	**y**	**n**	**y**

Table 8: EM Treatment Effects of Duration

Note: Controls only for confounding; *percentual* returns are reported under column (2), (4) and (5), obtained as $\alpha + \alpha_{dur}$. Only three outcomes are anyzed. Models used are reported in the third row. *P-value* for F-test on equality of treatment across duration dummies is reported in italics.
* significant at 10%; ** significant at 5%; *** significant at 1%.
Robust standard errors in parentheses.

	ln W, year		Top 50%		Prize	
	OLS	**IV**	**PROBIT**	**IV**	**PROBIT**	**IV**
	(1)	(2)	(3)	(4)	(5)	(6)
EM dummy	0.003	-0.021	-0.017	0.105	-0.005	-1.503
	(0.040)	(0.177)	(0.044)	(0.146)	(0.035)	(3.130)
EM < 1	-	-	-	-	-	-
EM < 2	0.031	0.054	0.022	-0.139	0.002	1.644
	(0.041)	(0.192)	(0.044)	(0.157)	(0.035)	(3.129)
EM < 3	0.028	0.144	0.065	-0.057	-0.013	1.590
	(0.040)	(0.185)	(0.043)	(0.150)	(0.035)	(3.139)
EM 3 or more	-0.021	0.180	0.047	0.048	-0.008	1.664
	(0.044)	(0.193)	(0.044)	(0.156)	(0.037)	(3.122)
R2	0.38	0.38	-	0.31	-	0.10
P-value (F-test)	*0.25*	*0.6*	*0.23*	*0.2*	*0.71*	*0.36*
Effect of EM duration (%)						
EM < 2	**3.3**	**3.4**	**-3.4**	**0.5**	**14.1**	**-0.3**
EM < 3	**12.3**	**3.1**	**4.8**	**4.8**	**8.7**	**-1.8**
EM 3 or more	**15.9**	**-1.8**	**15.3**	**3**	**16.1**	**-1.3**
CONTROLS						
Confounding	**y**	**y**	**y**	**y**	**y**	**y**
SES	**y**	**y**	**y**	**y**	**y**	**y**
School	**y**	**y**	**y**	**y**	**y**	**y**

Table 9: EM Treatment Effects of Duration

Note: Full controls models, *percentual* returns are reported in bold case, obtained as $\alpha + \alpha_{dur}$. Only three outcomes are anyzed. Models used are reported in the third row. *P-value* for F-test on equality of treatment across duration dummies is reported in italics.
* significant at 10%; ** significant at 5%; *** significant at 1%.
Robust standard errors in parentheses.