Parental Human Capital Investment Responses to Children's Disabilities

Anastasia Terskaya
University of Barcelona and Institut d'Economia de Barcelona

This paper investigates whether parental decisions to invest in education of their disabled children are driven by equality or efficiency. Even if parents are inequality averse, they may still choose to invest more in nondisabled children than in disabled children if there are additional costs of parental inputs associated with disability. I show that variation in family size and children's disabilities can be used to infer whether parents are averse to inequality, exploiting the fact that parents of single children cannot possibly exhibit inequality aversion. Using Mexican cross-sectional data, I show that equality is important for parental investments in education.

I. Introduction

A growing body of empirical evidence indicates that people with disabilities are more likely to experience social exclusion and socioeconomic disadvantage than those without disabilities, especially in developing countries (see, e.g., OECD 2009; Mitra, Posarac, and Vick 2013).[1] One of the important channels through which a disability may lead to diminishing well-being and the likelihood of poverty is the lack of education experienced by people with disabilities. According to Filmer (2008), in

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[1] The World Health Organization describes disability as an umbrella term for impairments, activity limitations, and participation restrictions as part of a broader classification scheme covering three main domains: body functioning and structure, activities and participation, and environmental factors (https://www.who.int/topics/disabilities/en/).

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some developing countries, the school participation deficit associated with a disability is more than 50 percentage points. Despite one of the Sustainable Development Goals being to ensure equal access to education for all, the disability gap in educational attainment has increased in developing countries during the past few decades, and many children with disabilities are never enrolled in school (Male and Wodon 2017). Since the decision to enroll in school and to continue education are mainly made by parents, understanding how parents respond to children's disabilities might shed light on the primary causes of the disability schooling gap.

The aim of this work is to infer whether the disability schooling gap can be partially explained by parental responses to children's disabilities. In particular, parents might invest differently in the education of disabled and nondisabled children, depending on whether parental behavior is driven by efficiency or equality concerns. If parental decision-making is driven by efficiency concerns, then parents will allocate resources in order to maximize total expected earnings of their children (Behrman, Pollak, and Taubman 1982). In this case, parents may provide more resources to children with higher expected returns to education, and therefore they may invest less in disabled than in nondisabled children, reinforcing the disability schooling gap. On the other hand, if parental decisions are driven by equality concerns, then parents will allocate resources in order to reduce differences in endowments between their children.

To analyze parental responses to children's disabilities, I provide a simple parental preference model built on seminal contributions of Becker and Tomes (1976) and Behrman, Pollak, and Taubman (1982). The model allows the cost of parental inputs (e.g., education) to differ with children's endowment levels while also incorporating parental inequality aversion. It also incorporates the fact that parental investments in one child may affect the human capital formation of his/her sibling because of sibling spillover effects. The model predicts that if the cost of parental inputs is higher for disabled than for nondisabled individuals, even inequality-averse parents might provide more resources to nondisabled than to disabled children. Additionally, the model predicts that parental inequality aversion affects only multichild families, since parents in one-child families do not have other children to whom they can reallocate the resources. Therefore, the disability schooling gap of only children cannot be explained by parental preferences but, for example, by differences in the costs of education between disabled and nondisabled individuals. In contrast, the disability schooling gap of children from multichild families can be affected by both parental preferences and the costs of parental inputs.

I use these theoretical predictions to build an empirical strategy relying on the variation in the number of children and in the disability status. In particular, under the assumption that the number of children in a family unit and a child's disability status are independent, the presence of parental inequality aversion implies that the disability schooling gap is lower in multichild families than in one-child families. It also implies that having a disabled sibling is associated with worse educational outcomes since inequality-averse parents reallocate resources from nondisabled children toward disabled children. By contrast, if parents care more about efficiency than equality, the disability schooling gap will be lower in one-child families than in multichild families and having a disabled sibling would be associated with better educational outcomes.

Most empirical studies about parental response to differences in children's endowments use sibling or twin comparisons (Behrman, Pollak, and Taubman 1982; Datar, Kilburn, and Loughran 2010; Aizer and Cunha 2012; Hsin 2012; Rosales-Rueda 2014; Yi et al. 2015; Cabrera-Hernández and Orraca-Romano 2016; Grätz and Torche 2016; Garcia Hombrados 2017; Bharadwaj, Eberhard, and Neilson 2018).[2] That is, these studies compare parental investments between low- and high-endowed siblings, mostly using birth weight, health-related outcomes, or cognitive ability test scores as endowment measures. Behrman, Rosenzweig, and Taubman (1994) and Savelyev et al. (2022) instead rely on comparisons between the within-twin correlations of human capital outcomes of monozygotic and dizygotic twins. Berry, Dizon-Ross, and Jagnani (2020) conduct a lab-in-the-field experiment to identify parental preferences for equality versus efficiency. Their results suggest that parents have strong preferences for equality in inputs as well as for maximizing expected earnings of children.

While sibling or twin comparisons can indicate whether parents follow a reinforcing or a compensating strategy, these models generally cannot distinguish whether parental behavior is driven by parental preferences (i.e., equality vs. efficiency concerns) or by differences in the costs of investing in high- versus low-endowed children.[3] In turn, the difference in costs of education between disabled and nondisabled individuals could be substantial, rendering sibling comparison uninformative about parental preferences. Therefore, this paper contributes to the previous literature by providing an alternative empirical strategy, which, under certain assumptions, allows one to infer the presence or absence of parental inequality aversion while allowing the cost of parental inputs to vary with the levels of children's endowments.

I conduct an empirical analysis using Mexican census data. I match individual characteristics using the entropy balancing reweighting method (Hainmueller 2012) in order to achieve a balance of observable characteristics between one-child and multichild families with disabled and nondisabled children. The results suggest that the disability schooling gap is lower in multichild families than in one-child families and that

[2] See also Almond and Mazumder (2013) for a review of empirical studies on parental responses to endowments.

[3] I say that parents follow a reinforcing strategy if they devote more resources to increasing the quality of the better endowed child, a compensating strategy if they provide more resources to a child with a lower endowment, and a neutral strategy if they devote equal resources to their children.

siblings of disabled individuals have lower educational attainment than siblings of nondisabled individuals with similar characteristics. The totality of the evidence is consistent with parental inequality aversion. In particular, parental inequality aversion reduces the disability schooling gap by about 13% and induces a decrease in years of education of nondisabled individuals who have a disabled sibling by about 2%. I also explore alternative explanations of these results, such as an unobserved heterogeneity or a bias related to endogenous fertility decisions. Reassuringly, the results are not supportive of these alternative explanations. The heterogeneity analysis suggests that the effect is statistically distinguishable from zero only in males.

The remainder of the paper is organized as follows. Section II presents the parental preference model that guides my empirical analysis. Section III lays out the empirical strategy. Section IV describes the data, and section V discusses the results. Section VI concludes.

II. Theoretical Model

In this section, I provide a static parental preference model that motivates and guides my identification strategy. The aim is to show that variation in family size and endowments can be used to test whether parents are inequality averse. This follows from the model's implication that parental aversion to inequality does not affect families with just one child. Therefore, if parents exhibit inequality aversion, then the investments gap between low- and high-endowed children is lower in multichild families than in one-child families. The opposite is true if parents care more about efficiency than equality.

A. *Preference Model*

Preference models are models of constrained utility maximization where parental preferences—in particular, parental aversion to inequality in the distribution of wealth among their children—play a central role in determining the distribution of parental investments among siblings. The theoretical framework is built on the classical intrahousehold allocation models of Becker and Tomes (1976) and Behrman, Pollak, and Taubman (1982). I assume that parental preferences can be represented by the utility function $U_p = U_p(c, V_1, .., V_n)$, where c denotes parental consumption and V_i is the quality of child i. Following Behrman, Pollak, and Taubman (1982), I assume that parental preferences are separable from consumption; therefore, the problem of parental investment in children can be rewritten as the following utility maximization:

$$U = U(V_1, .., V_n).$$

I specify parental preferences using a constant elasticity of substitution utility function:

$$U = \left\{ \sum_{i=1}^{n} V_i^{\rho} \right\}^{1/\rho}. \tag{1}$$

The main advantage of this utility form is that ρ measures the degree of parental inequality aversion across children. When $0 < \rho < 1$, parents do not dislike inequality and instead care about efficiency. In this case, parents follow a reinforcement strategy. When $\rho < 0$, parents dislike inequality and hence are more concerned about equality than efficiency. In this case, parents may compensate the less endowed child if marginal returns to education are positively correlated with endowments. When $\rho = 0$, parents trade off equality and efficiency.

Following Behrman, Pollak, and Taubman (1982), I assume that a child's quality function has the following form:

$$V(e_i, \mathrm{PI}_i) = e_i^{\alpha_e} \mathrm{PI}_i^{\alpha_s}, \tag{2}$$

where e_i denotes the endowment of child i and PI_i denotes parental investments in i. Note that in general, child's quality may depend on both knowledge (e.g., education) and health (Cunha, Heckman, and Schennach 2010; Ehrlich and Yin 2013; Yi et al. 2015), and therefore PI is a composite of parental investments in both components.

Diminishing returns to parental investments require $0 < \alpha_s < 1$, and positive returns to endowments imply that $\alpha_e > 0$. Note that with this function, the marginal returns to parental investments are positively related to endowments. That is, a child's endowments and his or her parental inputs are assumed to be complements in the human capital production function, which introduces an efficiency versus equality trade-off in parental investment decisions. This assumption is motivated by strong empirical evidence of complementarities between skills and investments during the later stages of childhood (Cameron and Heckman 2001; Cunha et al. 2006; Cunha, Heckman, and Schennach 2010; Heckman and Mosso 2014). Further evidence also indicates that there is static complementarity between ability and investments (Aizer and Cunha 2012; Attanasio et al. 2020) and that there is complementarity between genetic endowments measured using genetic data and parental investments proxied by childhood family socioeconomic status (Muslimova et al. 2020; Papageorge and Thom 2020; Ronda et al. 2022).

Finally, parental budget constraint has the following form:

$$\sum_{i=1}^{n} p_i \mathrm{PI}_i = I, \tag{3}$$

where p_i denotes the cost of parental inputs in child i and I denotes total investments in children.[4] Furthermore, I allow the cost of parental inputs to differ with children's initial endowments e, assuming that $p(e)$ is not

[4] Note that both the cost of parental investment and total investment include monetary and nonpecuniary expenditures, such as time.

increasing in e.[5] In other words, I assume that parental inputs are not more costly for children with higher initial endowments. For example, it might be more difficult to choose an appropriate school for an unhealthy child than for a healthy child.

The household's optimization problem yields the following optimality condition:

$$\frac{\partial U}{\partial \mathrm{PI}_i} \bigg/ \frac{\partial U}{\partial \mathrm{PI}_j} = \frac{p_i}{p_j}, \tag{4}$$

which implies that in the families where all children have the same initial endowment levels ($e_i = e_j \Rightarrow p_i = p_j \; \forall \; i, j \in 1, \ldots n$), all children will get the same level of parental investments.

B. *Children's Endowments and Resource Reallocation*

Consider a population that consists of two types of children—low endowed ($T = L$) and high endowed ($T = H$)—such that initial endowments satisfy $e^H > e^L$ and costs of parental inputs are such that $p^H \leq p^L$. For simplicity I assume that there can be only one low-endowed child in a family. Therefore, I consider one-child families with a low-endowed child ($F = L1$), one-child families with a high-endowed child ($F = H1$), k-child families with all high-endowed children ($F = Hk$), and k-child families with one low-endowed child and $k - 1$ high-endowed children ($F = Lk$). PI_F^T denotes the parental inputs in children of type T from family F.

In this model, the intrahousehold resource allocation depends on family size, distribution of siblings' initial endowments, parental preferences, costs of parental inputs, and returns to parental investments. One of the limitations of the model is that it takes family size as given. Note, however, that if fertility is endogenous, multichild families may differ from one-child families in terms of characteristics and preferences (Ehrlich and Lui 1991). I address this concern empirically in section V.B.1.

The optimal level of parental investment for low- and high-endowed children from one-child families satisfies the following condition:

$$\frac{\mathrm{PI}_{H1}^H}{\mathrm{PI}_{L1}^L} = \frac{p_L}{p_H} \geq 1. \tag{5}$$

Therefore, if the price of parental inputs is the same for low- and high-endowed children, then only children should get the same amount of inputs. However, if the costs of inputs depend on initial endowments, the level of parental investment will vary between low- and high-endowed children.

Solving the utility maximization problem for families with k highly endowed children yields the following optimal amount of parental inputs for each child:

[5] This assumption is weaker than the assumption in Behrman, Pollak, and Taubman (1982) that prices are independent from initial endowment.

$$\mathrm{PI}_{Hk}^{H} = \frac{I}{kp_H}. \tag{6}$$

Equations (5) and (6) show that parental investment levels of only children and of equally endowed siblings (PI_{Hk}^{H}) are not affected by parental inequality aversion (ρ).

Parental investment of children from multichild families where one child has a low level of endowments satisfies

$$\frac{\mathrm{PI}_{Lk}^{L}}{\mathrm{PI}_{Lk}^{H}} = \gamma, \tag{7}$$

where $\gamma = \{(p_L/p_H)(e_H/e_L)^{\rho\alpha_e}\}^{1/\alpha_s\rho-1)}$.

If $\gamma < 1$, we say that parents follow a reinforcing strategy and provide more inputs to the child with higher endowments. In contrast, when $\gamma > 1$, parents follow a compensating strategy; that is, they provide more inputs to a child with a low level of endowments than to a highly endowed child. If parents care more about efficiency ($0 < \rho < 1$), they will always follow a reinforcing strategy ($\gamma < 1$) since $\alpha_e > 0$, $0 < \alpha_s < 1$, $p_L/p_H \geq 1$, and $e_H/e_L > 1$. When parents are inequality averse ($\rho < 0$), we have that $(e_H/e_L)^{\rho\alpha_e} < 1$, and γ can be greater or smaller than 1, depending on the relative price of investment and on the degree of inequality aversion. Then, even inequality-averse parents may follow a reinforcing strategy if the cost of investing in low-endowed children is significantly higher than the cost of investing in high-endowed children. This implies that the comparison of parental investments between siblings with different endowments does not lead to unambiguous conclusions about parental inequality aversion if the cost of one unit of investment depends on a child's initial endowment.

A comparison of the investment gap between low- and high-endowed children in one-child and multichild families yields

$$\frac{\mathrm{PI}_{Hk}^{H}/\mathrm{PI}_{Lk}^{L}}{\mathrm{PI}_{H1}^{H}/\mathrm{PI}_{L1}^{L}} = \frac{1}{k} + \frac{(k-1)p_H}{k\gamma p_L}. \tag{8}$$

Proposition 1. In this framework, the following conditions hold:

i. $\mathrm{PI}_{Hk}^{H}/\mathrm{PI}_{Lk}^{L} > \mathrm{PI}_{H1}^{H}/\mathrm{PI}_{L1}^{L}$ if and only if $\rho > 0$.
ii. $\mathrm{PI}_{Hk}^{H}/\mathrm{PI}_{Lk}^{L} < \mathrm{PI}_{H1}^{H}/\mathrm{PI}_{L1}^{L}$ if and only if $\rho < 0$.
iii. $\mathrm{PI}_{Hk}^{H}/\mathrm{PI}_{Lk}^{L} = \mathrm{PI}_{H1}^{H}/\mathrm{PI}_{L1}^{L}$ if and only if $\rho = 0$.

Proof. Let me start by proving item i. Note that from equation (8), it follows that

$$\frac{\mathrm{PI}_{Hk}^{H}}{\mathrm{PI}_{Lk}^{L}} > \frac{\mathrm{PI}_{H1}^{H}}{\mathrm{PI}_{L1}^{L}} \Leftrightarrow \frac{p_H}{\gamma p_L} = \left\{ \left(\frac{p_L}{p_H}\right)^{\rho\alpha_s} \left(\frac{e_H}{e_L}\right)^{\rho\alpha_e} \right\}^{1/(1-\alpha_s\rho)} > 1.$$

Since $e_H > e_L$, the assumption that prices are not increasing in initial endowments implies that $p_L/p_H \geq 1$. Moreover, returns to initial endowments are positive ($\alpha_e > 0$), and returns to parental inputs are diminishing ($0 < \alpha_s < 1$). Therefore, if I assume that $p_H/\gamma p_L > 1$, then it must be the case that $\rho \in (0; 1/\alpha_s)$. However, since $\rho < 1 < 1/\alpha_S$, the last condition is satisfied whenever $\rho > 0$. On the other hand, if $\rho > 0$, then $(\mathrm{PI}_{Hk}^{H}/\mathrm{PI}_{Lk}^{L})/(\mathrm{PI}_{H1}^{H}/\mathrm{PI}_{L1}^{L}) > 1$ is trivially satisfied.

The same argument is applied to items ii and iii. QED

Proposition 1 demonstrates that the investment gap is greater in one-child families than in multichild families when parents are inequality averse. This is due to the fact that inequality-averse parents provide some extra inputs to low-endowed children who have high-endowed siblings, while low-endowed only children cannot be possibly affected by inequality aversion. On the other hand, when parents care more about efficiency ($\rho > 0$), the investment gap is greater in multichild families than in one-child families since, in this case, parents reallocate resources from low-endowed to high-endowed children in order to increase the total expected earnings of their children.

C. *Sibling Spillover Effect*

One potential issue is that parental investments in one child may affect his/her siblings' human capital because of sibling spillover effects. To address this issue, I extend the theoretical model in order to incorporate the direct sibling spillover effect. That is, I allow the quality of child i, V_i, to be directly affected by parental inputs in his/her sibling. For simplicity, in this section I consider only two-child families with low-endowed and high-endowed children. Note that sibling spillover cannot possibly affect one-child families; therefore, the optimal amount of inputs in low- and high-endowed children from one-child families is given by (5).

As in section II.A, I assume that the parental utility function is given by (1) and the parental budget constraint is given by (3). I assume that the quality function is similar to the one provided in (2), but in addition it can be directly affected by sibling's parental inputs. The quality function is given by

$$V(e_i, \mathrm{PI}_i, \mathrm{PI}_{-i}) = e_i^{\alpha_e}\mathrm{PI}_i^{\alpha_s}\mathrm{PI}_{-i}^{\beta}\ \forall\ i = 1, 2, \tag{9}$$

where e_i denotes the endowment of child i, PI_i denotes parental inputs in i, and PI_{-i} denotes parental inputs in i's sibling.

Solving the first-order condition of this problem yields the following expression:

$$\frac{V_1^{\rho-1}e_1^{\alpha_e}\mathrm{PI}_2^{\beta}\alpha_s\mathrm{PI}_1^{\alpha_s-1} + V_2^{\rho-1}e_2^{\alpha_e}\beta\mathrm{PI}_1^{\beta-1}\mathrm{PI}_2^{\alpha_s}}{V_1^{\rho-1}e_1^{\alpha_e}\beta\mathrm{PI}_2^{\beta-1}\mathrm{PI}_1^{\alpha_s} + V_2^{\rho-1}e_2^{\alpha_e}\mathrm{PI}_1^{\beta}\alpha_s\mathrm{PI}_2^{\alpha_s-1}} = \frac{\mathrm{PI}_2(\alpha_s V_1^{\rho} + \beta V_2^{\rho})}{\mathrm{PI}_1(\beta V_1^{\rho} + \alpha_s V_2^{\rho})} = \frac{p_1}{p_2}, \tag{10}$$

which can be rewritten as

$$\left(\frac{e_1}{e_2}\right)^{\alpha_e\rho}\left(\frac{\mathrm{PI}_2}{\mathrm{PI}_1}\right)^{1-\rho(\alpha_s-\beta)} = \frac{p_1\alpha_s - p_2\beta(\mathrm{PI}_2/\mathrm{PI}_1)}{p_2\alpha_s - p_1\beta(\mathrm{PI}_1/\mathrm{PI}_2)}. \tag{11}$$

First, note that when both siblings have the same level of endowments ($e_1 = e_2$ and $p_1 = p_2$), the solution to the problem is $\mathrm{PI}_1 = \mathrm{PI}_2$, as in the baseline model. Therefore, in a family with two high-endowed children ($F = H2$), $\mathrm{PI}^H_{H2} = I/2p_H$.

Now consider a family with one low-endowed child and one high-endowed child ($F = L2$). In appendix A, I show that proposition 1 holds in this extended model when the sibling spillover effect (β) is smaller than the effect of i's own parental inputs on i's quality (α_s).

Proposition 2. In this framework, when a child's quality is given by equation (9), and when $\alpha_s > \beta$, proposition 1 holds.

The proof of proposition 2 is provided in appendix A.

While presence of externalities across siblings does not affect the main theoretical conclusions provided in proposition 1, I show that the magnitude of the effect of the presence/absence of parental inequality aversion on the investment gap changes depending on the size of these spillovers. Particularly, in appendix A, I provide results of a numerical simulation exercise, where I show how the investment gap in multichild families changes depending on parental inequality aversion ρ and on the direct sibling spillover effect (β). Figure A1 plots $\mathrm{PI}^H_{H2}/\mathrm{PI}^L_{L2}$ as a function of ρ for different values of β. The results suggest that parental compensation/reinforcement is attenuated in the presence of the sibling spillover effect. The intuition for this result is that when parents are inequality averse and there is a direct sibling spillover effect, parents have an additional gain from investing in the higher-endowed child since his/her quality also increases the quality of the lower-endowed child. As a result, parents end up compensating less. On the other hand, when parents care more about efficiency, the sibling spillover effect is associated with additional gains from investing in the less well-endowed child and therefore with a lower degree of reinforcement.

For subsequent analyses, let me define the effect of parental inequality aversion on parental inputs in low-endowed children who have high-endowed siblings as

$$\theta_k = [\log(\mathrm{PI}^L_{Lk}) - \log(\mathrm{PI}^H_{Hk})] - [\log(\mathrm{PI}^L_{L1}) - \log(\mathrm{PI}^H_{H1})]. \tag{12}$$

Propositions 1 and 2 imply that this effect is positive if parents are inequality averse and that it is negative when parents care more about efficiency than equality.

Another intuitive implication of the model is that when parents are inequality averse, high-endowed children in multichild families who have a low-endowed sibling get less inputs than high-endowed children in multichild families who have only high-endowed siblings ($\mathrm{PI}^H_{Hk}/\mathrm{PI}^H_{Lk} > 1$ iff $\rho < 0$). Let me define the effect of parental inequality aversion on parental investment of high-endowed children who have low-endowed siblings versus those who have only high-endowed siblings as

$$\psi_k = \log(\mathrm{PI}^H_{Lk}) - \log(\mathrm{PI}^H_{Hk}). \tag{13}$$

It can be shown that $\psi_k > 0$ if and only if $\rho > 0$ (parents value more efficiency than equality) and $\psi_k < 0$ if and only if $\rho < 0$ (parents are inequality averse).

I use the predictions of the theoretical model to guide the empirical strategy aiming to infer whether parents are inequality averse or care more about efficiency. In the empirical estimation, I focus on five groups of individuals: (1) nondisabled only children, (2) disabled only children, (3) nondisabled children from multichild families, (4) disabled children from multichild families, and (5) nondisabled children who have disabled siblings. The discussion above implies that the interaction between the effect of disability and the multichild family on parental investments is indicative of the presence/absence of parental inequality aversion (a positive effect of the interaction implies that parents are inequality averse, and a negative effect implies the opposite). Similarly, a negative effect of having a disabled sibling implies that parents are inequality averse, and a positive effect implies the opposite.

III. Empirical Model

In this section, I conduct an empirical analysis of parental responses to children's disabilities, guided and motivated by the theoretical model described above. In particular, I want to test whether the effect of disability on parental inputs differs between only children and children who have siblings.

The framework I propose is particularly useful to study parental responses to children's disabilities since it takes into account that the cost of parental investments in human capital (i.e., education) might be higher for disabled than for nondisabled children. Since access to education for people with disabilities is still limited, especially in developing countries, this may result in higher costs of education for people with disabilities than for nondisabled people. Therefore, frequently used empirical models based on sibling comparison would not be able to identify whether parental responses to children's disabilities are driven by efficiency or equality concerns since differences in parental investments (i.e., education) between disabled and nondisabled siblings depend on both parental preferences for equality and the price effect.

To measure parental inputs, I use educational attainment. Note, however, that in general, parental inputs may include both education and health (Ehrlich and Yin 2013) and parents may have different responses to children's endowments in terms of their investments in either of these components (Yi et al. 2015). Therefore, the empirical analysis is focused on one of the dimensions of human capital: education.[6]

As in the theoretical model, education depends on disability and family size. I limit my analysis to families with no more than one disabled child.

I specify the model as

$$\log(S_{i,T,f}) = \alpha_0 + \alpha_1 D_T + \alpha_2 Multi_f + \tilde{\theta}(Multi_f \times D_T) + \tilde{\psi}(Multi_f \times \mathrm{DS}_T) + v_{i,T,f}, \tag{14}$$

where $S_{i,T,f}$ denotes individual i of type $T = \{D, N, \mathrm{DS}\}$ (with D denoting disabled, N denoting nondisabled, and DS denoting nondisabled with a disabled sibling) from family type $f = \{1, k\}$ (with 1 denoting a one-child family and k denoting a multichild family). D_T is a dummy variable that takes a value of 1 if the child is disabled, and DS_T is a dummy variable that takes a value of 1 if the child has a disabled sibling. $Multi_f$ is a multichild family indicator.

In this model, α_1 captures the effect of disability on schooling of only children, and α_2 captures the effect of the family size on nondisabled individuals. The main coefficient of interest is $\tilde{\theta}$, which captures the difference in the disability schooling gap between one-child families and multichild families. $\tilde{\psi}$ captures the effect of having a disabled sibling on the schooling of nondisabled children relative to nondisabled children who have nondisabled siblings.

Positive $\tilde{\theta}$ together with negative $\tilde{\psi}$ indicates that parents are inequality averse and provide additional education to disabled children when they also have nondisabled children. Since inequality-averse parents redistribute resources from nondisabled children to children with disabilities, parental inequality aversion has a negative impact on the education of nondisabled children who have siblings with disabilities ($\tilde{\psi} < 0$).

If $v_{i,T,f}$ is uncorrelated with disability status and family size, the ordinary least squares (OLS) estimator of $\tilde{\theta}$ is unbiased:

$$\mathbb{E}[\log(S_{i,D,k}) - \log(S_{i,N,k})] - \mathbb{E}[\log(S_{i,D,1}) - \log(S_{i,N,1})] = \tilde{\theta}, \tag{15}$$

which is the empirical equivalent of θ_k, as defined in equation (12).

Let me allow $v_{i,T,f}$ to be correlated with disability status and with family size and specify this relationship as follows:

$$v_{i,T,f} = \gamma_T + \delta_f + \omega_{T,f} + \epsilon_i, \tag{16}$$

[6] Yi et al. (2015) study how parents respond to early health shocks of children and find that parents compensate in terms of health investments and reinforce in terms of educational investments. Note that eq. (6) suggests that reinforcement can be consistent with both presence and absence of parental inequality aversion, while compensation is consistent only with inequality aversion. Therefore, the estimates of Yi et al. (2015) suggest that parents are averse to inequality in the distribution of children's health but inconclusive about parental aversion to inequality in the distribution of children's education.

where γ_T denotes the omitted variables that are correlated only with the disability status, δ_f denotes omitted variables that are correlated only with the family size, $\omega_{T,f}$ denotes omitted variables that are correlated with both the family size and the disability status, and ϵ_i denotes a disturbance term. Then, the OLS estimator of $\tilde{\theta}$ may be biased since

$$\begin{aligned}&\mathbb{E}[\log(S_{i,D,k}) - \log(S_{i,N,k})] - \mathbb{E}[\log(S_{i,D,1}) - \log(S_{i,N,1})]\\&\quad = \tilde{\theta} + \mathbb{E}(\omega_{D,k} - \omega_{N,k}) - \mathbb{E}(\omega_{D,1} - \omega_{N,1}).\end{aligned} \tag{17}$$

Note that omitted variables that are correlated only with the disability status (γ_T) and only with the family size (δ_f) cannot possibly bias the estimate of $\tilde{\theta}$ since equation (16) controls for the disability dummy and for the multichild family indicator. To estimate $\tilde{\theta}$ consistently, I need to assume that the unobservable factors affecting schooling may differ for disabled and nondisabled children, but this difference cannot depend on family size. For example, disabled individuals may exert less effective effort than nondisabled individuals, and effort levels may also depend on family size. However, the difference between average effective effort exerted by disabled and nondisabled individuals has to be the same in one-child families and in multichild families. The main identification assumption can be specified as

$$\mathbb{E}(\omega_{D,k} - \omega_{N,k}) - \mathbb{E}(\omega_{D,1} - \omega_{N,1}) = 0. \tag{18}$$

This assumption is similar to a parallel path assumption in difference-in-difference models. Note, however, that this assumption does not ensure that the effect of disability in one-child families (captured by α_1) and the effect of multichild family (captured by α_2) will be consistently estimated from equation (14). Therefore, α_1 and α_2 should be interpreted with caution.

Another coefficient in which I am interested is $\tilde{\psi}$ (the effect of inequality aversion on nondisabled individuals with disabled siblings). Note that if $v_{i,T,f}$ is uncorrelated with the disability status and the family size, then $\tilde{\psi} = \mathbb{E}(\log(S_{i,\mathrm{DS},k})) - \mathbb{E}(\log(S_{i,N,k}))$. However, if $v_{i,T,f}$ is defined as in (16), then

$$\mathbb{E}[\log(S_{i,\mathrm{DS},k}) - \log(S_{i,N,k})] = \tilde{\psi} + \mathbb{E}(\gamma_{\mathrm{DS}} - \gamma_N) + \mathbb{E}(\omega_{\mathrm{DS},k} - \omega_{N,k}). \tag{19}$$

Hence, to consistently identify $\tilde{\psi}$, I need to assume that nondisabled individuals who have disabled siblings do not differ in individual or family characteristics from nondisabled individuals who have nondisabled siblings. This assumption may not appear plausible, and hence one should be cautious about giving $\tilde{\psi}$ a causal interpretation of the effect of parental inequality aversion on the education of nondisabled individuals. Therefore, I will be mainly discussing the effect of parental inequality aversion on the schooling level of disabled individuals who have nondisabled siblings ($\tilde{\theta}$).

IV. Data Description and Sample Construction

I use individual- and household-level data for Mexico from Integrated Public Use Microdata Series International (IPUMS-I) for 2010. The data were originally produced by the Mexican National Institute of Statistics, Geography, and Informatics. The data set contains information on a wide range of characteristics, including family interrelationships, education, and disability.

A. Estimation Sample

For this analysis, I select households with children, and the sample of children (those who report to be sons or daughters of the head of household) includes 5,174,463 individuals. About 2.4% of individuals report to have some form of disability. I define the number of children in the family as the number of children reported by the mother. Alternatively, I could have defined the number of children as the number of children who live in the household. However, older children are likely to live separately from their parents, and therefore the number of children would be underestimated. In section V.B.3, I test whether the results are robust to this alternative definition of the number of children. Since the number of siblings is one of the key variables in this analysis, I eliminate 152,823 observations with missing information on the number of children. I also eliminate 1,447 observations for which age was missing and 537,646 observations for which educational attainment was missing. Since the main outcome is years of schooling, I restrict the sample to individuals older than 8 years and younger than 30 years, which leaves me with 3,238,833 observations.[7] Next, I restrict the sample to households with both parents residing in the family dwelling and with no more than four children, which leaves me with 1,289,545 observations.[8] I select only disabled individuals whose disabilities are of congenial origin in order to address the potential endogeneity of disability and to control for the timing of the disability occurrence. Specifically, while disabilities caused by accidents or diseases might be affected by parental investments in education, congenial disabilities cannot possibly be affected by parental postnatal investments. This restriction leaves me with 1,278,845 observations.

[7] Primary school in Mexico starts when the child is age 6 or 7. Therefore, I do not consider individuals younger than age 7. Note that usually when analyzing years of schooling, researchers consider only individuals who have supposedly finished secondary school (older than age 15; Angrist and Krueger 1991; Acemoglu and Angrist 1999; Maccini and Yang 2009). Instead, I also consider younger individuals, since family structure might also have an effect on lower levels of educational attainment. In fact, there are a considerable proportion of disabled individuals in the sample who have never attended school, although secondary school (grade 9) education is compulsory by law in Mexico.

[8] In the main analysis, I compare one-child families with two-child families, and I also test whether the results hold when I compare one-child families with three- and four-child families.

Finally, I consider only households with no more than one disabled child, leaving 1,275,502 remaining observations from 697,977 households.

Table C1 reports summary statistics for the final sample. Individuals are, on average, 14.2 years old with 6.7 years of schooling. Most individuals are literate (97.1%), 43.2% have completed primary education, 10.9% have completed secondary education, and 2.8% have tertiary education. Females are 47.2% of the sample. Disabled individuals constitute 1.1% of the sample. Most disabled individuals have a mental disability. Most families in my sample are multichild families (93.3%) with 2.8 children on average.

In the main analysis, I compare one-child families with two-child families, so that the main estimation sample consists of 380,648 observations, of which 4,472 correspond to disabled individuals.

Table 1 reports the average years of education by sibsize and disability status. It shows that individuals between 8 and 30 years old with congenial disabilities have, on average, 4 years of education, while nondisabled individuals have an average of 6.5 years of education. The disability schooling gap constitutes 5.4 years of schooling for 16–30-year-olds.

Figure 1 shows the distribution of years of schooling for disabled and nondisabled individuals from one-child and two-child families aged 16–30. A considerable share of disabled individuals have never attended school. This may suggest that disabled children in Mexico face many barriers to accessing education. The distribution of years of schooling is more right skewed for disabled only children than for disabled children with siblings. In contrast, for nondisabled individuals, the distribution of years of schooling is similar for one-child and multichild families.

In sum, this descriptive evidence suggests that the education gap between nondisabled and disabled individuals is lower in multichild families, which is consistent with parental inequality aversion. However, these differences may be driven by differences in family and individual characteristics, which I take into account in the subsequent analysis.

TABLE 1
Years of Schooling

	Ages 8–30			Ages 16–30		
	N (1)	Mean (2)	SD (3)	*N* (4)	Mean (5)	SD (6)
Disabled	4,472	3.987	3.683	1,479	5.732	4.938
Disabled one-child family	789	3.503	3.597	285	4.754	4.742
Disabled two-child family	3,683	4.091	3.693	1,194	5.965	4.957
Nondisabled	376,176	6.495	4.063	117,261	11.160	3.028
Nondisabled one-child family	48,341	6.275	4.041	15,276	10.703	3.331
Nondisabled two-child family	327,835	6.527	4.065	101,985	11.229	2.974

Note.—All statistics are for individuals from one-child and two-child families.

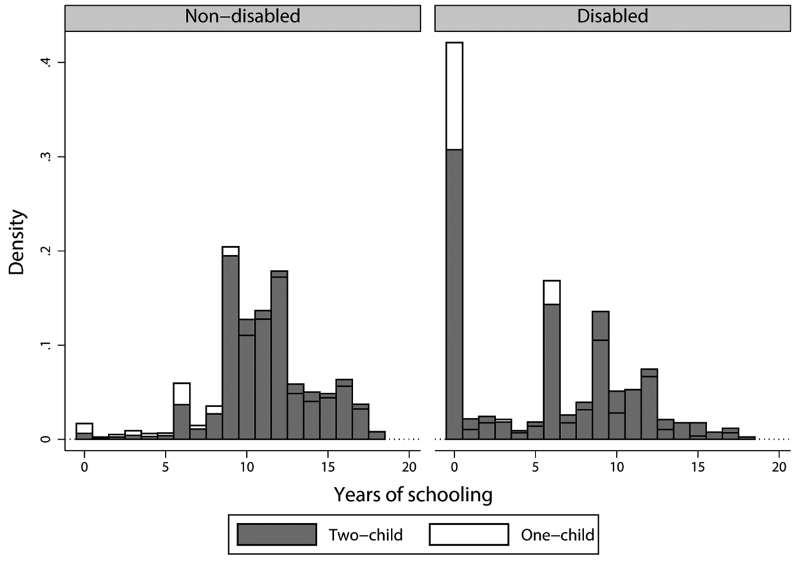

Figure 1.—Years of education by disability status and family size. The figure reports the average years of education in one-child and two-child families by disability status.

B. *Balancing of Observable Characteristics*

To check whether the main identification assumption holds for the observable characteristics, I conduct a set of balancing tests. Specifically, I regress each control variable on the disability dummy, the two-child family indicator, and on their interaction. Then I test whether the interaction coefficient is statistically distinguishable from zero.

In order to achieve a better balance between observations from different groups, I reweight observations, such that the covariate distribution of the control group is similar to the covariates distribution of the treatment group.[9] Since some of the covariates vary at the individual level rather than at the family level, the reweighting is conducted at the individual level.

To generate weights, I use the entropy balancing (EB) method developed by Hainmueller (2012), which produces a set of observation-level weights that balance covariate distributions across groups. One of the advantages of EB over the popular propensity score weighting (PS) is that EB guarantees that all the covariate moments included in the reweighting are equally balanced. By contrast, PS can lead to a greater imbalance on

[9] In the context of my identification strategy, the control groups are those unaffected by parental inequality aversion: one-child families and multichild families in which all children have similar initial endowments. Disabled individuals who have nondisabled siblings are potentially affected by parental inequality aversion, and hence they are from the treatment group.

some covariate moments while improving the balance on others (Iacus, King, and Porr 2012). Besides, EB allows one to directly incorporate covariate balance, so there is no need to check covariate balance iteratively to avoid model misspecification, as occurs with PS (Diamond and Sekhon 2013; Zhao and Percival 2016). The details on weight construction are provided in appendix B.

Table C2 reports the means and the standard deviations of observable characteristics by the disability status in one- and two-child families and the difference-in difference coefficient before and after EB reweighting. Column 5 reports the differences before EB reweighting, suggesting that there are statistically significant differences in age, age of mother and father, and parental disability status. In particular, disabled individuals from one-child families are older than those from two-child families. Column 6 reports the differences after EB reweighting, showing that EB balances the distributions of observable characteristics appropriately, as not all the differences are statistically distinguishable from zero.

My identifying assumption (18) also requires that disabled individuals from two-child families are similar to disabled individuals from one-child families.

Table C3 compares the distributions of the type of disability in one- and two-child families. It is crucial to control for the type of disability because the cost of education is likely to depend on it. For example, disabled individuals from one-child families may have fewer years of schooling than those from two-child families simply because their disability is more severe. I reweight observations for disabled individuals from one-child families, so that the distribution of their type of disability is similar to the distribution of the type of disability of disabled individuals from two-child families. The differences after the EB reweighting are reported in columns 3 and 4 of table C3 and, reassuringly, they are no longer significant. In section V.A.4, I conduct separate analyses for different types of disability.

V. Results

Table 2 provides the estimated coefficients from equation (14) before and after the EB reweighting. Column 1 of table 2 provides the estimated results without including additional controls and before EB. Column 2 provides the results after including controls, and columns 3 and 4 report the estimates after the EB reweighting with and without controls, respectively. The main coefficient of interest is the coefficient of the interaction term two-child family × disabled, which indicates that the gap in schooling between disabled and nondisabled children is, on average, 13.5% smaller in two-child families than in one-child families. The estimates of the coefficient for two-child family × disabled reduce after observations are reweighted with EB weights. On the other hand, the inclusion of controls does not affect the magnitude of the estimates.

TABLE 2
Effect of Disability and Family Size on Schooling

	OLS		OLS with EB Weights	
	(1)	(2)	(3)	(4)
Two-child family × disabled	.165***	.163***	.131***	.135***
	(.044)	(.041)	(.049)	(.042)
Disabled sibling	−.000	−.012**	−.015	−.019***
	(.010)	(.005)	(.011)	(.006)
Disabled	−.836***	−.302***	−.797***	−.338***
	(.040)	(.054)	(.045)	(.063)
Two-child family	.053***	.022***	.012***	.011***
	(.004)	(.002)	(.004)	(.002)
Blind		.079*		.118**
		(.040)		(.055)
Deaf		−.002		.044
		(.049)		(.071)
Mute		−.561***		−.485***
		(.039)		(.051)
Lower extremities		−.229***		−.217***
		(.040)		(.054)
Mental		−.909***		−.840***
		(.040)		(.053)
Covariates included	No	Yes	No	Yes
State fixed effects	No	Yes	No	Yes
Observations	380,648	380,648	380,648	380,648
R^2	.011	.795	.140	.539
Oster ratios and adjusted coefficients:				
Two-child family × disabled	232.262 (.162)		−26.812 (.145)	
Disabled sibling	−3.925 (−.012)		−4.350 (−.028)	

Note.—The reported estimates correspond to regressions of the inverse hyperbolic sine one transformation of years of schooling (used to approximate log of schooling) on the disability dummy, the indicator of two-child family, the indicator of disabled sibling, and the interaction between the disability dummy and the two-child family dummy. Columns 2 and 4 also include the set of controls listed in table C2, age fixed effects, and state fixed effects. In cols. 2 and 3, observations are weighted using EB weights. Standard errors clustered at the household level are in parentheses. Oster ratios are relative degrees of selection under proportional selection of observable and unobservable factors computed as proposed by Oster (2019). Oster adjusted coefficients (in parentheses) are treatment effect estimates, based on the assumption that the degree of selection on unobservables is twice as large as the degree of selection on observables, computed as proposed by Oster (2019).
* $p < .10$.
** $p < .05$.
*** $p < .01$.

In order to assess the degree to which differences in unobservable characteristics may drive the results, I follow the methodology proposed by Oster (2019) and compute the relative degree of selection on unobservables (with respect to the degree of selection on observables). The Oster ratios are reported in table 2. I find that the influence of unobserved factors would have to be at least 26.8 times stronger than the influence of observed factors (listed in tables C2 and C3) in order to explain away the interaction coefficient two-child family × disabled. Following Oster (2019), I compute the bias-adjusted treatment effect, assuming that the relative degree of selection on unobservable variables is twice as large as the selection on observables. The estimated bias-adjusted effect is 0.145, which indicates that the disability schooling gap is 14.4% smaller in two-child families than in one-child families.

The estimated effect of having a disabled sibling on the schooling of nondisabled individuals (row 2 of table 2) is negative and implies that nondisabled children who have disabled siblings receive on average 1.9% less education than nondisabled children who have no disabled siblings. The Oster ratio for the disabled sibling indicator is −4.4, suggesting that the influence of unobserved factors would have to be at least 4.4 times stronger than the influence of observed factors in order to explain away the effect of having a disabled sibling. The bias-adjusted treatment effect is −0.028, suggesting that if the degree of selection on unobservables were twice as large as the degree of selection on observables, having a disabled sibling would be associated with a 2.8% reduction in years of education. Overall, the results are consistent with parents being inequality averse.

Table 2 also indicates that there is a large negative effect of disability on years of schooling in one-child families even when EB reweighting is applied (the coefficient of the disability dummy reported in row 3 captures the effect of disability in one-child families). Specifically, column 3 suggests that children with disabilities have, on average, 80% fewer years of education than those with no disability. Column 4 indicates that this effect is stronger among individuals with mental disabilities.[10] In order to analyze whether this effect may be solely due to unobserved heterogeneity, I estimate the bias-adjusted effect of disability in one-child families following Oster (2019), assuming that the degree of selection on unobservables is twice as large as the degree of selection on observables. The estimated bias-adjusted effect is −0.901 for all types of disability (corresponding to the effect of disabled from cols. 1 and 3) and −0.284 for an unknown type of disability (corresponding to the effect of disabled from cols. 2 and 4). This suggests that the effect of disability is unlikely to be explained by differences in unobserved characteristics between families with and without disabled children.

Finally, table 2 indicates that there is a positive association between years of schooling and family size in nondisabled children (corresponds to the coefficient of two-child family). Specifically, the results from column 4 suggest that nondisabled children from two-child families have, on average, 1.1% more years of schooling that those from one-child families. I test whether this effect can be attributed to unobserved heterogeneity by computing bias-adjusted coefficients as above. The bias-adjusted effect of a two-child family is not statistically distinguishable from zero,

[10] Note that the coefficient for Disabled in col. 4 of table 2 is interpreted as the effect of disability of unknown type.

being very small in magnitude (0.006, or approximately 2 weeks of education).

A. Heterogeneous Parental Response

1. By Gender

In this section, I analyze whether the effect of parental inequality aversion differs by children's gender. The effect may vary by gender if parents are not gender neutral. For instance, Dahl and Moretti (2008) have found evidence supporting the notion that parents in the United States favor boys over girls. In contrast, Baccara et al. (2014) have identified significant preferences favoring girls. Behrman, Pollak, and Taubman (1986) have shown that parental preferences either slightly favor girls or are neutral.

On the other hand, the effect of parental inequality aversion on schooling may depend on children's gender when there are gender differences in the returns to parental inputs, even if parental preferences are gender neutral. In fact, there is evidence that cognitive and noncognitive development of boys is more responsive to parental inputs than that of girls (Leibowitz 1974; Hill and Duncan 1987; Brooks-Gunn, Han, and Waldfogel 2002; Moore et al. 2004; Bertrand and Pan 2013). For example, Bertrand and Pan (2013) show that a substantial part of the gender gap in disruptive behaviors can be explained by gender differences in returns to parental inputs.

In addition, the evidence on gender differences in emotionality and sociability in children with disabilities suggests that females with autism display better social skills than males with autism (Lai et al. 2011; Head, McGillivray, and Stokes 2014). Therefore, for males with disability, any kind of social interaction—such as those involved with school attendance—can be costlier than for females. This can be incorporated into the model by allowing the nonpecuniary costs of education for disabled females to be lower than those for disabled males. Then, the model predicts a greater effect of parental inequality aversion on educational attainment for males than for females, since the effect of inequality aversion ($\tilde{\theta}$) on the cost of education for disabled children increases when parents exhibit inequality aversion (see eq. [12]).

I report the estimates of equation (14) in males and females separately in columns 1 and 2 of table 3. The results suggest that the interaction coefficient (two-child family × disabled) is large and statistically significant in males but not statistically distinguishable from zero for females.

2. By Birth Order

A number of empirical studies predict a negative relationship between birth order and parental investments (Behrman and Taubman 1986; Iacovou 2001; Black, Devereux, and Salvanes 2005; Price 2008; Lehmann,

TABLE 3
Heterogeneous Effect of Disability and Family Size on Schooling

	By Gender		By Birth Order		By Family Size	
	Females (1)	Males (2)	Firstborn (3)	Second Born (4)	Three-Child Family (5)	Four-Child Family (6)
Multichild × disabled	.094	.176**	.187***	.092*	.146***	.106**
	(.066)	(.070)	(.052)	(.050)	(.046)	(.051)
Disabled sibling	−.018*	−.018**	−.016**	−.028***	−.015***	−.019***
	(.010)	(.008)	(.007)	(.010)	(.005)	(.006)
Disabled	−.420***	−.296***	−.363***	−.239***	−.390***	−.494***
	(.099)	(.095)	(.075)	(.077)	(.068)	(.076)
Multichild	.015***	.008***	.011***	.001	.010***	.003
	(.004)	(.003)	(.003)	(.003)	(.003)	(.004)
Blind	.240***	−.058	.150**	.034	.135**	.154**
	(.078)	(.089)	(.062)	(.073)	(.061)	(.069)
Deaf	.197*	−.022	−.048	.060	−.036	.093
	(.117)	(.085)	(.099)	(.086)	(.084)	(.087)
Mute	−.562***	−.459***	−.407***	−.462***	−.487***	−.523***
	(.084)	(.069)	(.062)	(.062)	(.053)	(.057)
Lower extremities	−.152*	−.183**	−.247***	−.302***	−.245***	−.196***
	(.082)	(.080)	(.065)	(.070)	(.056)	(.062)
Mental	−.826***	−.851***	−.831***	−.733***	−.865***	−.918***
	(.082)	(.078)	(.062)	(.067)	(.056)	(.060)
Observations	178,668	201,977	238,886	151,035	573,005	420,109
R^2	.549	.573	.551	.542	.526	.514

Note.—The reported estimates correspond to regressions of the inverse hyperbolic sine one transformation of years of schooling (used to approximate log of schooling) on the disability dummy, the indicator of multichild families, the indicator of disabled siblings, and the interaction between the disability dummy and the multichild family dummy. All specifications include the set of controls listed in table C2, age fixed effects, and state fixed effects. Observations are weighted using EB weights. Standard errors clustered at the household level are in parentheses.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

Nuevo-Chiquero, and Vidal-Fernandez 2018). I explore whether the effect of parental inequality aversion differs by birth order by reestimating equation (14) for firstborn and second-born children separately. The results are reported in columns 3 and 4 of table 3. The interaction coefficient appears to be statistically significant and positive for both firstborn and later-born children. However, the effect of parental inequality aversion on the educational attainment of firstborn children is greater than the effect on their second-born counterparts. Specifically, the estimates imply that the disability schooling gap is 18.7% smaller for firstborns who have nondisabled siblings than for only children. For later-born children, this difference constitutes 9.2%. These results are in line with findings in the economic literature showing a negative correlation between birth order and children's human capital outcomes (Black, Devereux, and Salvanes 2005; Rosales-Rueda 2014; Lehmann, Nuevo-Chiquero, and Vidal-Fernandez 2018).

3. By Family Size

Up to now, this paper has compared the educational gap between nondisabled and disabled in one-child families and two-child families. To analyze whether the effect of parental inequality aversion varies with family size, I estimate equation (14) for three- and four-child families. The resulting estimates are reported in columns 1 and 2 of table 3, suggesting that the disability schooling gap is 14.6% smaller for children from three-child families than for only children and 10.6% smaller for children from four-child families than for only children. The results are consistent with parental inequality aversion.

4. By Type of Disability

In order to analyze whether parents respond differently to different types of disabilities, I reestimate equation (14) separately for mental and nonmental types of disability and for blind, mute, or deaf individuals.

The results reported in table 4 suggest that the interaction term is not statistically distinguishable from zero when only mental disabilities are considered. This might be due to the fact that we do not observe the severity of mental disability but only the presence or absence of such disability. While, for instance, blindness does not vary in severity, the severity of mental disability might vary substantially. In fact, if children with a mental disability from multichild families have, on average, more severe conditions than those from one-child families, this would introduce a negative bias on the estimate of the interaction term. On the other hand,

TABLE 4
Effect of Disability and Family Size on Schooling by Type of Disability

	Mental (1)	Nonmental (2)	Blind, Mute, or Deaf (3)
Two-child family × disabled	.056	.138***	.106*
	(.087)	(.051)	(.060)
Disabled sibling	−.023***	−.017***	−.019***
	(.008)	(.006)	(.006)
Disabled	−1.226***	−.505***	−.459***
	(.083)	(.048)	(.058)
Two-child family	.014***	.008***	.007**
	(.003)	(.003)	(.003)
Observations	377,881	378,934	377,989
R^2	.522	.520	.538

Note.—The reported estimates correspond to regressions of the inverse hyperbolic sine one transformation of years of schooling (used to approximate log of schooling) on the disability dummy, the indicator of two-child family, the indicator of disabled sibling, and the interaction between the disability dummy and the two-child family dummy. All specifications include the set of controls listed in table C2, age fixed effects, and state fixed effects. Observations are weighted using EB weights. Standard errors clustered at the household level are in parentheses.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

if disabled only children have more severe conditions, this would introduce a positive bias on the estimate of the interaction term.

Column 2 of table 4 reports the results for children with nonmental disabilities, suggesting that in this subsample, the interaction term is positive and statistically different from zero.

Finally, column 3 of table 4 reports the results for blind, mute, or deaf individuals. I analyze this group separately because these disability types do not vary in severity, and therefore the cost of education for individuals with these types of disabilities cannot vary with the family size. The estimated interaction term is positive and significant at the 10% level of confidence.[11]

5. By Parental Characteristics

In table 5, I provide an additional heterogeneity analysis according to parental characteristics, such as total parental income, maternal education, and paternal education. The results reported in columns 1 and 2 suggest that the effect of the interaction term (two-child family × disabled) is positive and statistically distinguishable from zero in upper-middle income families and positive but not statistically distinguishable from zero in lower-middle income families. The estimates in columns 3 and 4 suggest that the effect of the interaction term is similar in families where maternal education lies at the median or above and where it is below the median. On the other hand, the effect of the interaction term is, on average, 70% larger in families where paternal education is at the median or above than in families where paternal education is below the median (cols. 5, 6).

These results may suggest that less advantaged parents may have financial constraints that prevent them from redistributing the resources toward their less well-endowed children. These findings are in line with the strategic investment argument of Conley and Lareau (2008) that suggests that advantaged families may adopt a compensatory strategy in their resource allocation decision simply because they can afford it while providing at least a minimum-level investment to all their children. By contrast, disadvantaged families may want to maximize expected returns to investments in their children and therefore may end up compensating less. This may suggest that policies aimed at increasing resources of disadvantaged families may also induce parents to compensate the differences between their children and decrease the long-term effect of health-related inequality.

B. Robustness Checks

1. Endogenous Fertility

One of the potential identification challenges is that a child's disability may partially affect subsequent parental fertility behavior. In this case,

[11] In all regressions, I control for the type of disability using the EB weights so the shares of mute, blind, and deaf individuals are constrained to be equal in one-child and multichild families.

TABLE 5
EFFECT OF DISABILITY AND FAMILY SIZE ON SCHOOLING BY TYPE OF DISABILITY

	By Family Income		By Maternal Education		By Paternal Education	
	≥Median (1)	<Median (2)	≥Median (3)	<Median (4)	≥Median (5)	<Median (6)
Two-child family × disabled	.139**	.086	.125**	.150**	.178***	.109*
	(.066)	(.055)	(.059)	(.061)	(.062)	(.061)
Disabled sibling	−.013	−.025***	−.015**	−.022**	−.015**	−.024**
	(.008)	(.009)	(.007)	(.010)	(.007)	(.012)
Disabled	−.254***	−.326***	−.157*	−.471***	−.293***	−.331***
	(.095)	(.087)	(.081)	(.093)	(.092)	(.084)
Two-child family	.008**	.013***	.008***	.012***	.007**	.014***
	(.003)	(.004)	(.003)	(.004)	(.003)	(.004)
Blind	.144*	.077	.032	.179**	.137*	.012
	(.081)	(.078)	(.064)	(.083)	(.073)	(.082)
Deaf	.050	−.012	−.040	.075	.049	.002
	(.097)	(.096)	(.079)	(.111)	(.099)	(.109)
Mute	−.511***	−.472***	−.412***	−.528***	−.465***	−.604***
	(.087)	(.066)	(.073)	(.070)	(.081)	(.076)
Lower extremities	−.227***	−.209***	−.272***	−.199***	−.141*	−.282***
	(.085)	(.073)	(.070)	(.075)	(.074)	(.074)
Mental	−.867***	−.863***	−.878***	−.919***	−.866***	−.906***
	(.088)	(.069)	(.070)	(.077)	(.078)	(.076)
Observations	192,492	188,156	225,647	155,000	228,303	152,342
R^2	.579	.527	.602	.525	.596	.508

Note.—The reported estimates correspond to regressions of the inverse hyperbolic sine one transformation of years of schooling (used to approximate log of schooling) on the disability dummy, the indicator of two-child family, the indicator of disabled sibling, and the interaction between the disability dummy and the two-child family dummy. All specifications include the set of controls listed in table C2, age fixed effects, and state fixed effects. Observations are weighted using EB weights. Maternal and paternal education is measured using years of schooling (median is 9 years for both mothers and fathers). Standard errors clustered at the household level are in parentheses.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

fertility and a child's disability are not independent, which may bias the estimated effect of the interaction between the disability status and the multichild family identifier. On the one hand, child disability may impose constraints on household resources and, as a result, may reduce fertility. On the other hand, it may also increase subsequent fertility if parents have preferences for healthy children.

The empirical evidence on the impact of adverse child health conditions on parental fertility choices is limited. To the best of my knowledge, the only paper that attempts to estimate a causal effect of child health conditions on subsequent maternal fertility is Wehby and Hockenberry (2017). The paper uses a mother fixed effect duration model for maternal fertility over time to estimate the effect of the share of previously born children with disabilities/adverse health conditions in order to account for time-invariant unobservable characteristics, using the US data. Wehby and Hockenberry (2017) find no evidence that having disabled children significantly affects subsequent fertility.

I replicate Wehby and Hockenberry's (2017) analysis for the Mexican data. To do so, I extend the sample to include all children in families with at least one disabled child. Then, I employ a mother fixed effect duration hazard model to estimate the effect of the share of disabled children among previously born children in the household on having a subsequent live birth. As in my main analysis, I restrict the sample to families with disabled children with congenial origin. The time to next birth or potential censoring time is modeled using the Cox proportional hazard regression model stratified by family, so the estimated impacts are based on within-family rather than between-family variation. Following Wehby and Hockenberry (2017), I control for maternal age at previous birth, for the number of previous births, and for the share of female children among previously born children.

The results of Cox's proportional hazard model are reported in table C4. Consistent with Wehby and Hockenberry (2017), the results suggest that having a disabled child is not associated with a significant change in subsequent live births in specification for any disability (hazard ratio, 0.977; $p = .256$). Next, I estimate the effect of the share of previously born children with mental and nonmental types of disability. The results suggest that only mental disabilities are associated with a marginally significant decline in subsequent births (hazard ratio, 0.943; $p = .098$). As for nonmental disabilities, the association is not statistically significant (hazard ratio, 0.995; $p = .825$). Note that the effect of parental inequality aversion is statistically distinguishable from zero only in the sample of children with nonmental disabilities. This suggests that the results are unlikely to be driven by a bias associated with endogenous fertility decisions.

Another way to address this issue is to fix the fertility decision by considering as multichild families only those families with twins. This method is frequently used in the economic literature in order to achieve exogenous variation in family size (see, e.g., Black, Devereux, and Salvanes 2005; Rosenzweig and Zhang 2009; Yi et al. 2015). Therefore, I consider the following groups: nondisabled individuals from one-child families, disabled individuals from one-child families, nondisabled individuals who have nondisabled twins, nondisabled individuals who have disabled twins, and disabled individuals who have no disabled twins. The problem with this approach is that the size of the treatment group (disabled individuals who do not have a disabled twin) consists of only 30 observations. I provide the estimation results for this sample in column 3 of table C5. The point estimates in the sample of twins have a similar sign and magnitude as the estimates in the full sample, which suggests that the main results are unlikely to be fully explained by a bias associated with endogenous fertility. However, the estimated effect of parental inequality aversion is not statistically significant, given the limited number of twins with disabilities.

2. Parental Disability

Column 5 of table C2 shows that the maternal and paternal disability indicators failed the balancing test before applying EB weights. In this section, I test whether the results hold if I drop families with disabled parents from the analysis. Column 1 of table C5 reports the estimated coefficients for the sample of families with nondisabled parents. The estimated coefficients are similar to the baseline estimates from table 2.

3. Sibsize Definition

In the main analysis, I define the number of children as the number of children reported by their mother. However, if some children do not live with their parents, parents might not react to the differences between these children the same way as they react to the differences between children who live together. To address this point, I reestimate equation (14) for families with all children residing in the household. The results are reported in column 2 of table C5, and these estimates are similar to the estimates based on the full sample.

4. Age-Adjusted Outcomes

Table C2 shows that there are some statistically significant differences in age between disabled and nondisabled children in one-child and multichild families. Because age puts a cap on years of education, older children, ceteris paribus, will have higher educational attainment. I address this issue in the main analysis by applying the EB reweighting and by controlling for age dummies as well as for parental age. In addition, in table C6 I replicate the main analysis for years of schooling standardized by age.[12] The results are consistent with the baseline results reported in table 2 and suggest that the schooling gap between disabled and nondisabled children is, on average, 0.4 standard deviations smaller in two-child families than in one-child families.

Another way to address this issue is to use an outcome that is purged of age effects. While there is no such outcome in IPUMS data, there is a literacy indicator, which has a lower correlation with age than with years of schooling (0.07 vs. 0.78). Therefore, I conduct the analysis using the literacy indicator and the literacy indicator standardized by age as the outcome. The results reported in table C6 are in line with the baseline results. Column 2 of table C6 suggests that the disability-literacy gap is 4 percentage points lower in two-child families than in one-child families. The standardized results reported in column 3 of table C6 indicate that this effect corresponds to a 0.47 standard deviation increase in literacy.

[12] For each age, I compute the mean and the standard deviation of years of schooling. The standardized variable is obtained by removing the age-specific mean and dividing by the age-specific standard deviation.

5. Falsification Tests

Finally, I run a set of placebo tests to verify that the results I obtain are not driven by pure chance. To do so, I randomly assign to each child a placebo disability status using the observed probability of disability. Specifically, I generate a binomial random variable that takes a value of 1 with probability equal to the share of disabled individuals in the sample. Using this placebo variable, I estimate equation (14) including controls. I repeat this procedure 500 times.

The distribution of placebo *t*-values is illustrated in figure C1. The estimated interaction coefficient is statistically significant at the 5% level in fewer than 5% of draws, which suggests that the results are unlikely to be driven by chance.

VI. Conclusions

In this paper, I study how parents respond to their children's disabilities and, in particular, whether parents are averse to inequality of their children. I assess the impact of parental responses to their children's disability on the education of these children. By means of a preference model, I show that the variation in the disability status and in the family size can be used to infer the presence of parental inequality aversion. This is due to the fact that only parents from multichild families whose children have different endowments can possibly display inequality aversion.

My theoretical framework and identification strategy take into account that the cost of adding to quality may depend on children's initial endowments, an issue that cannot be addressed when considering siblings comparisons. When I apply this identification strategy to Mexican data, the results indicate that parental investments in education are subject to inequality aversion and, therefore, parents attenuate the negative effects of disability on their children's educational attainment. I also show that in Mexico, the education gap between disabled and nondisabled individuals constitutes about 5.4 years of schooling, which may have dramatic implications for the labor market outcomes of disabled individuals.

The findings of this paper inform policies aimed at reducing the negative effects of childhood disabilities on human capital formation and disability-related inequality. Particularly, the findings suggest that such interventions could be targeted toward families rather than toward individual children since families tend to redistribute their resources toward disadvantaged children. Moreover, in the presence of inequality aversion, compensatory education policies targeted at individual children may induce parents to act less as equalizing agents and to redistribute resources away from the child being compensated and toward themselves or other children, which may reduce the impact of compensatory programs. On the other hand, relying on families to distribute resources may be ineffective in one-child families that are not affected by parental inequality aversion.

One of the limitations of this analysis is that the results for Mexico cannot be generalized to other countries, since parental preferences for equality may differ across countries, depending on pension systems, culture, informal institutions, and other factors. However, the method of testing for parental inequality aversion proposed in this paper can be easily applied to other contexts, since it requires data that are generally easily available.

Appendix A

Model with Sibling Spillover

A1. Proof of Proposition 2

Proof. First, note that by substituting (1) $\mathrm{PI}^H_{H2} = I/(2p_H)$, (2) $\mathrm{PI}^H_{H1}/\mathrm{PI}^L_{L1} = p_L/p_H$, and (3) $p_H\mathrm{PI}^H_{L2} + p_L PI^L_{L2} = I$ in proposition 1, it can be rewritten only in terms of parental inputs in a family with one low-endowed child and one highly endowed child ($F = L2$) as follows:

i. $\mathrm{PI}^H_{H2}/\mathrm{PI}^L_{L2} > \mathrm{PI}^H_{H1}/\mathrm{PI}^L_{L1} \Leftrightarrow \mathrm{PI}^L_{L2}/\mathrm{PI}^H_{L2} < p_H/p_L$ if and only if $\rho > 0$.
ii. $\mathrm{PI}^H_{H2}/\mathrm{PI}^L_{L2} < \mathrm{PI}^H_{H1}/\mathrm{PI}^L_{L1} \Leftrightarrow \mathrm{PI}^L_{L2}/\mathrm{PI}^H_{L2} > p_H/p_L$ if and only if $\rho < 0$.
iii. $\mathrm{PI}^H_{Hk}/\mathrm{PI}^L_{Lk} = \mathrm{PI}^H_{H1}/\mathrm{PI}^L_{L1} \Leftrightarrow \mathrm{PI}^L_{L2}/\mathrm{PI}^H_{L2} = p_H/p_L$ if and only if $\rho = 0$.

Therefore, in the remaining part of the proof, I consider only family $F = L2$, and for simplicity, I skip family subscript $L2$ and denote PI^L_{L2} by PI_L and PI^H_{L2} by PI_H.

(i.1).—If $\mathrm{PI}_L/\mathrm{PI}_H < p_H/p_L$, then $\rho > 0$.

$\mathrm{PI}_L/\mathrm{PI}_H < p_H/p_L \Rightarrow p_L\alpha_s - p_H\beta(\mathrm{PI}_H/\mathrm{PI}_L) < p_L\alpha_s - p_H\beta(p_L/p_H) = p_L(\alpha_s - \beta)$ and $p_H\alpha_s - p_L\beta(\mathrm{PI}_L/\mathrm{PI}_H) > p_H(\alpha_s - \beta)$.

Substituting this into (11) implies that

$$\left(\frac{e_L}{e_H}\right)^{\alpha_e\rho}\left(\frac{\mathrm{PI}_H}{\mathrm{PI}_L}\right)^{1-\rho(\alpha_s-\beta)} < \frac{p_L}{p_H}.$$

I prove by contradiction that this is possible only if $\rho > 0$.

Case 1.—$\rho < 0 \Rightarrow (e_L/e_H)^{\alpha_e\rho} > 1$ and $1 - \rho(\alpha_s - \beta) > 0 \Rightarrow (e_L/e_H)^{\alpha_e\rho}(\mathrm{PI}_H/\mathrm{PI}_L)^{1-\rho(\alpha_s-\beta)} < p_L/p_H \Rightarrow (\mathrm{PI}_H/\mathrm{PI}_L)^{1-\rho(\alpha_s-\beta)} < p_L/p_H$.

Since $\mathrm{PI}_L/\mathrm{PI}_H < p_H/p_L \Rightarrow \mathrm{PI}_H/\mathrm{PI}_L > p_L/p_H > 1$, this inequality holds only if $1 - \rho(\alpha_s - \beta) < 1$, which contradicts $\rho < 0$.

Case 2.—If $\rho = 0$, (11) implies that $\mathrm{PI}_L/\mathrm{PI}_H = p_H/p_L$, which is also a contradiction.

Therefore, $\mathrm{PI}_L/\mathrm{PI}_H < p_H/p_L$ only if $0 < \rho < 1$.

(i.2).—If $\rho > 0$, then $\mathrm{PI}_L/\mathrm{PI}_H < p_H/p_L$.

$\rho > 0 \Rightarrow (e_L/e_H)^{\alpha_e\rho} < 1$. This transforms (11) into

$$\left(\frac{\mathrm{PI}_H}{\mathrm{PI}_L}\right)^{1-\rho(\alpha_s-\beta)} > \frac{p_L\alpha_s - p_H\beta(\mathrm{PI}_H/\mathrm{PI}_L)}{p_H\alpha_s - p_L\beta(\mathrm{PI}_L/\mathrm{PI}_H)}.$$

I prove by contradiction that this is possible only if $\mathrm{PI}_L/\mathrm{PI}_H < p_H/p_L$.

Case 1.—$\mathrm{PI}_L/\mathrm{PI}_H > p_H/p_L \Rightarrow [p_L\alpha_s - p_H\beta(\mathrm{PI}_H/\mathrm{PI}_L)]/[p_H\alpha_s - p_L\beta(\mathrm{PI}_L/\mathrm{PI}_H)] > p_L/p_H \Rightarrow (\mathrm{PI}_H/\mathrm{PI}_L)^{1-\rho(\alpha_s-\beta)} > p_L/p_H > 1$.

Given that $\rho > 0 \Rightarrow 1 - \rho(\alpha_s - \beta) < 1$, this inequality can be satisfied only if $\mathrm{PI}_L/\mathrm{PI}_H < p_H/p_L$, which is a contradiction.

Case 2.—If $\mathrm{PI}_L/\mathrm{PI}_H = p_H/p_L$, (11) transforms into $(p_L/p_H)^{\rho(\alpha_s-\beta)} = (e_L/e_H)^{\rho\alpha_e}$. Given that $\rho > 0$, this implies that $(p_L/p_H)^{(\alpha_s-\beta)} = (e_L/e_H)^{\alpha_e} \rightarrow p_L/p_H = (e_L/e_H)^{\alpha_e/(\alpha_s-\beta)} > 1$, which contradicts $e_L/e_H < 1$ and $\alpha_e/(\alpha_s - \beta) > 0$.

(ii.1).—If $\mathrm{PI}_L/\mathrm{PI}_H > p_H/p_L$, then $\rho < 0$.

$\mathrm{PI}_L/\mathrm{PI}_H > p_H/p_L \Rightarrow p_L\alpha_s - p_H\beta(\mathrm{PI}_H/\mathrm{PI}_L) > p_L\alpha_s - p_H\beta(p_L/p_H) = p_L(\alpha_s - \beta)$ and $p_H\alpha_s - p_L\beta(\mathrm{PI}_L/\mathrm{PI}_H) < p_H(\alpha_s - \beta)$.

This implies that

$$\left(\frac{e_L}{e_H}\right)^{\alpha_e\rho}\left(\frac{\mathrm{PI}_H}{\mathrm{PI}_L}\right)^{1-\rho(\alpha_s-\beta)} > \frac{p_L}{p_H}.$$

I prove by contradiction that this is possible only if $\rho < 0$.

Case 1.—$0 < \rho < 1 \Rightarrow 0 < 1 - \rho(\alpha_e - \beta) < 1$, $\mathrm{PI}_H/\mathrm{PI}_L > 1$, and $(e_L/e_H)^{\alpha_e\rho} < 1$. This implies $(e_L/e_H)^{\alpha_e\rho}(\mathrm{PI}_H/\mathrm{PI}_L)^{1-\rho(\alpha_s-\beta)} < (\mathrm{PI}_H/\mathrm{PI}_L)^{1-\rho(\alpha_s-\beta)} < \mathrm{PI}_H/\mathrm{PI}_L < p_L/p_H$, which is a contradiction.

Case 2.—If $\rho = 0$, (11) implies that $\mathrm{PI}_L/\mathrm{PI}_H = p_H/p_L$, which is also a contradiction.

Therefore, $\mathrm{PI}_L/\mathrm{PI}_H > p_H/p_L$ only if $\rho < 0$.

(ii.2).—If $\rho < 0$, then $\mathrm{PI}_L/\mathrm{PI}_H > p_H/p_L$.

Note that $\rho < 0 \Rightarrow (e_L/e_H)^{\alpha_e\rho} > 1$, which implies

$$\left(\frac{\mathrm{PI}_H}{\mathrm{PI}_L}\right)^{1-\rho(\alpha_s-\beta)} < \frac{p_L\alpha_s - p_H\beta(\mathrm{PI}_H/\mathrm{PI}_L)}{p_H\alpha_s - p_L\beta(\mathrm{PI}_L/\mathrm{PI}_H)}.$$

I prove by contradiction that this holds only if $\mathrm{PI}_L/\mathrm{PI}_H > p_H/p_L$.

Case 1.—$\mathrm{PI}_L/\mathrm{PI}_H < p_H/p_L \Rightarrow [p_L\alpha_s - p_H\beta(\mathrm{PI}_H/\mathrm{PI}_L)]/[p_H\alpha_s - p_L\beta(\mathrm{PI}_L/\mathrm{PI}_H)] < p_L/p_H \Rightarrow (\mathrm{PI}_H/\mathrm{PI}_L)^{1-\rho(\alpha_s-\beta)} < p_L/p_H \Rightarrow p_L/p_H < \mathrm{PI}_H/\mathrm{PI}_L < (p_L/p_H)^{1/[1-\rho(\alpha_s-\beta)]} < p_L/p_H$, which is a contradiction.

Case 2.—If $\mathrm{PI}_L/\mathrm{PI}_H = p_H/p_L$, (11) transforms into $(p_L/p_H)^{\rho(\alpha_s-\beta)} = (e_L/e_H)^{\rho\alpha_e}$. Given that $\rho < 0$, this implies that $(p_L/p_H)^{(\alpha_s-\beta)} = (e_L/e_H)^{\alpha_e} \rightarrow p_L/p_H = (e_L/e_H)^{\alpha_e/(\alpha_s-\beta)} > 1$, which contradicts $e_L/e_H < 1$ and $\alpha_e/(\alpha_s - \beta) > 0$.

(iii.1).—If $\mathrm{PI}_L/\mathrm{PI}_H = p_H/p_L$, then $\rho = 0$, which is proof by contradiction.

If $\rho \neq 0$ and $\mathrm{PI}_L/\mathrm{PI}_H = p_H/p_L$, (11) transforms into $(p_L/p_H)^{(\alpha_s-\beta)} = (e_L/e_H)^{\alpha_e} \Rightarrow p_L/p_H = (e_L/e_H)^{\alpha_e/(\alpha_s-\beta)} > 1$, which contradicts $e_L/e_H < 1$ and $\alpha_e/(\alpha_s - \beta) > 0$.

(iii.2).—If $\rho = 0$, then $\mathrm{PI}_L/\mathrm{PI}_H = p_H/p_L$.

$\rho = 0$ transforms (11) into

$$\frac{\mathrm{PI}_H}{\mathrm{PI}_L} = \frac{p_L\alpha_s - p_H\beta(\mathrm{PI}_H/\mathrm{PI}_L)}{p_H\alpha_s - p_L\beta(\mathrm{PI}_L/\mathrm{PI}_H)}.$$

The unique solution to this equation is $\mathrm{PI}_L/\mathrm{PI}_H = p_H/p_L$. QED

A2. Simulation

Next, I conduct a numerical simulation exercise, where I show how the disability investment gap in multichild families changes depending on parental inequality aversion ρ and on the direct sibling spillover effect β. I compute a numerical solution to (11) for a specific set of parameters.

Figure A1 depicts $\mathrm{PI}^H_{H2}/\mathrm{PI}^L_{L2}$ and $\mathrm{PI}^H_{H1}/\mathrm{PI}^L_{L1}$ as a function of ρ for different values of β. The simulated results suggest that the effect of parental inequality aversion is lower in the presence of sibling spillover effect. These results suggest that in the presence of sibling spillover effect, parents may not be able to fully compensate differences between their children, since investments in one child can affect quality of another child.

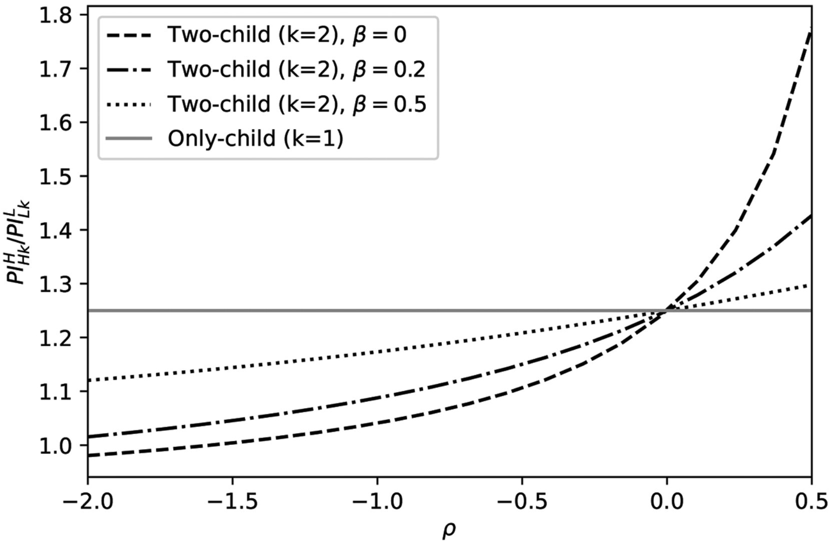

Figure A1.—Disability schooling gap, parental inequality aversion, and sibling spillover effect. This figure plots the solution to (11) for $\alpha_s = \alpha_e = 0.8$, $e_H = 1$, $e_L = 0.5$, $p_H = 0.8$, $p_L = 1$, and $I = 1$. $\mathrm{PI}^H_{Hk}/\mathrm{PI}^L_{Lk}$ (y-axis) is the disability investment gap in k-child families. ρ (x-axis) is the parental inequality aversion parameter. β is the sibling spillover effect.

Appendix B

Entropy Balancing Weights

In the EB, every control unit $(i|C)$ gets a weight that satisfies a set of balance constraints and each treated unit $(i|T)$ gets either a weight $\omega_i = 1$ or $\omega_i = s_i$, where s_i is a sampling weight associated with i. Specifically, the weights for each control unit are chosen in order to minimize the loss function:

$$\min_{\omega_i} H(\omega) = \sum_{i|C} h(\omega_i) = \sum_{i|C} \omega_i \log\left(\frac{\omega_i}{s_i}\right) \tag{B1}$$

subject to balance and normalizing constraints

$$\sum_{i|C} \omega_i c_{ri}(X_i) = m_r, \text{with } r \in 1, \ldots, R \text{ and} \tag{B2}$$

$$\sum_{i|C} \omega_i = 1, \tag{B3}$$

$$\omega_i \geq 0 \text{ for all } i\,|C, \tag{B4}$$

where $c_{ri}(X_i) = m_r$ describes a set of R balance constraints imposed on the covariate moments of the reweighted control group. For this analysis, a balance constraint is formulated with m_{rj} containing the rth order moment of a given variable x_j for the treatment group (group of disabled individuals from multichild families), whereas the moment functions $c_{ri}(X_i)$ are specified for the control group. Therefore, weights (ω_i) are chosen in a way that the weighted $1_{st}, \ldots, R_{th}$ moments of the covariates in the control group are equal to the corresponding moments of the covariates in the treatment group. The loss function $H(\omega)$ measures the distance between the distribution of estimated control weights $(\omega = [\omega_1, \ldots, \omega_n])$ and the distribution of the base weights $(S = [s_1, \ldots, s_n])$; it is nonnegative and it decreases the closer ω is to S (the unconstrained minimum would be achieved at zero if $\omega = S$). These properties of the loss function imply that while weights are adjusted as far as needed to fulfill the balance constraints (B2), they are maintained as close as possible to the base weights to sustain information about the control group. Therefore, another advantage of EB over PS is that with EB, extreme weights are less likely.

I apply the EB algorithm to generate four sets of weights for each of the following control groups: disabled individuals from one-child families (C_1), nondisabled individuals from multichild families who have no disabled siblings (C_2), nondisabled individuals from one-child families (C_3), and nondisabled individuals from multichild families who have disabled siblings (C_4). For the treatment group (T), I consider disabled individuals from multichild families. Then I use the obtained EB weights to estimate coefficients from equation (14) by OLS. I impose the EB constraints on the first and second moments of observable family and individual characteristics listed in table C2 and age fixed effects.

Appendix C

Tables and Figures

TABLE C1
Summary Statistics

	Mean	SD
	A. Individual-Level Statistics	
Years of schooling	6.675	3.977
Literate	.971	.166
Less than primary	.431	.495
Primary	.432	.495
Secondary	.109	.311
Tertiary	.028	.164
Age	14.260	4.930
Age 8–15	.647	.478
Age 16–30	.353	.478
Female	.472	.499
Disabled	.011	.103
Mental	.004	.064
Blind	.002	.044
Deaf	.001	.030
Mute	.003	.055
Lower extremities	.002	.050
Observations	1,275,502	

TABLE C1 (*Continued*)

	Mean	SD
	B. Household-Level Statistics	
Number of children	2.773	.874
One-child family	.067	.250
Two-child family	.320	.467
Three-child family	.386	.487
Four-child family	.227	.419
Rural	.410	.492
Maternal age	38.389	8.349
Paternal age	41.773	9.382
Years of schooling of mother	7.607	4.212
Years of schooling of father	7.932	4.481
Mother is employed	.300	.458
Father is employed	.896	.305
Mother's earnings (1,000 pesos)	1.249	4.259
Father's earnings (1,000 pesos)	4.779	8.964
Mother is disabled	.025	.156
Father is disabled	.042	.200
Dwelling ownership	.845	.361
Car ownership	.458	.498
Observations	697,977	

Note.—Sample means and standard deviations are reported.

TABLE C2
BALANCING TESTS

	Mean (SD)				Coefficient (SE)	
	Nondisabled		Disabled		Difference-in-Difference ((Col. 4 – Col. 3) – (Col. 2 – Col. 1))	
	One-Child Family (1)	Multichild Family (2)	One-Child Family (3)	Multichild Family (4)	Before EB (5)	After EB (6)
Age	13.791	13.752	14.701	14.096	−.566**	−.005
	(5.063)	(4.855)	(5.853)	(5.302)	(.227)	(.228)
Female	.467	.470	.417	.414	−.006	−.000
	(.499)	(.499)	(.493)	(.493)	(.019)	(.022)
Rural	.357	.335	.401	.390	.012	−.000
	(.479)	(.472)	(.490)	(.488)	(.019)	(.022)
Dwelling ownership	.818	.835	.803	.806	−.014	−.000
	(.385)	(.371)	(.398)	(.395)	(.016)	(.017)
Car ownership	.469	.511	.390	.433	.001	−.000
	(.498)	(.499)	(.487)	(.495)	(.019)	(.022)
Mother's age	39.126	38.236	41.510	38.843	−1.776***	−.013
	(9.492)	(7.710)	(11.132)	(8.406)	(.422)	(.333)
Mother's years of schooling	8.311	8.748	7.170	7.834	.227	−.003
	(4.700)	(4.277)	(4.789)	(4.094)	(.185)	(.166)
Mother's personal earnings	1.827	1.735	1.286	1.293	.099	−.000
	(4.977)	(5.155)	(4.006)	(4.396)	(.162)	(.213)
Mother is employed	.360	.357	.341	.321	−.016	−.000
	(.479)	(.478)	(.474)	(.466)	(.019)	(.020)

TABLE C2 (*Continued*)

	Mean (SD)				Coefficient (SE)	
	Nondisabled		Disabled		Difference-in-Difference ((Col. 4 – Col. 3) – (Col. 2 – Col. 1))	
	One-Child Family (1)	Multichild Family (2)	One-Child Family (3)	Multichild Family (4)	Before EB (5)	After EB (6)
Mother has no health insurance	.314	.283	.291	.269	.008	−.000
	(.463)	(.450)	(.453)	(.443)	(.018)	(.019)
Mother is disabled	.032	.021	.124	.072	−.041***	−.000
	(.177)	(.143)	(.330)	(.258)	(.013)	(.010)
Father's age	43.047	41.393	45.267	42.363	−1.250***	−.014
	(10.557)	(8.702)	(11.930)	(9.478)	(.455)	(.386)
Father's years of schooling	8.420	9.026	7.163	8.004	.235	−.003
	(4.843)	(4.544)	(4.816)	(4.403)	(.188)	(.186)
Father's personal earnings	5.251	5.826	3.945	4.576	.056	−.001
	(9.506)	(10.160)	(5.678)	(6.543)	(.234)	(.319)
Father is employed	.888	.912	.854	.892	.014	−.000
	(.314)	(.283)	(.352)	(.309)	(.014)	(.013)
Father has no health insurance	.337	.306	.338	.299	−.008	−.000
	(.472)	(.460)	(.472)	(.458)	(.019)	(.020)
Father is disabled	.047	.034	.145	.096	−.036***	−.000
	(.212)	(.180)	(.352)	(.294)	(.013)	(.012)
Observations	48,341	322,764	789	3,683		

Note.—Siblings of disabled individuals are excluded. Standard deviations (cols. 1–4) and standard errors (cols. 5, 6) clustered at the household level are in parentheses.
** $p < .05$.
*** $p < .01$.

TABLE C3
BALANCING TESTS: TYPE OF DISABILITY

	Mean (SD)		Beta (SE)	
			Difference (Col. 2 − Col. 1)	
Type of Disability	One-Child Family (1)	Multichild Family (2)	Before EB (3)	After EB (4)
Blind	.190	.201	.011	−.000
	(.393)	(.401)	(.015)	(.018)
Deaf	.077	.087	.010	.000
	(.267)	(.283)	(.011)	(.013)
Mute	.275	.260	−.015	−.000
	(.447)	(.439)	(.017)	(.019)
Lower extremities	.274	.240	−.033*	−.000
	(.446)	(.427)	(.017)	(.018)
Mental	.411	.376	−.035*	−.000
	(.492)	(.484)	(.019)	(.021)
Observations	789	3,683		

Note.—Standard deviations (cols. 1, 2) and standard errors (cols. 3, 4) clustered at the household level are in parentheses.
* $p < .10$.

TABLE C4
Effect of Child's Disability on Subsequent Fertility

	(1)	(2)
Share of disabled	.977	
	(.020)	
Share of disabled (nonmental)		.995
		(.025)
Share of disabled (mental)		.943*
		(.033)
Observations	89,489	89,489

Note.—The table reports the hazard ratios estimated using the stratified Cox proportional hazard model for the effect of the proportion of previously born children with disability (overall in col. 1 and mental and nonmental in col. 2) on subsequent live births. The model uses within-family variation. The models control for maternal age, the number of previous births, and the share of female children among previous births. Standard errors clustered by family are in parentheses.
* $p < .10$ for the null hypothesis that the hazard ratio is equal to 1.

TABLE C5
Robustness Checks

	No Disabled Parents (1)	All Children in Household (2)	Twins vs. One-Child Family (3)
Two-child family × disabled	.151***	.152***	.083
	(.047)	(.045)	(.141)
Disabled sibling	−.024***	−.023***	−.084
	(.007)	(.006)	(.093)
Disabled	−.383***	−.315***	−.289*
	(.068)	(.067)	(.152)
Two-child family	.013***	.007***	.024
	(.002)	(.002)	(.027)
Blind	.154***	.107*	−.156
	(.059)	(.058)	(.224)
Deaf	.023	−.020	.359**
	(.080)	(.084)	(.155)
Mute	−.454***	−.429***	−.774***
	(.054)	(.055)	(.159)
Lower extremities	−.255***	−.264***	−.157
	(.059)	(.058)	(.157)
Mental	−.807***	−.795***	−.884***
	(.057)	(.056)	(.172)
Observations	357,345	344,137	51,164
R^2	.538	.547	.596

Note.—The reported estimates correspond to regressions of the inverse hyperbolic sine one transformation of years of schooling (used to approximate log of schooling) on the disability dummy, the indicator of two-child family, the indicator of disabled sibling, and the interaction between the disability dummy and the two-child family dummy. All specifications include the set of controls listed in table C2, age fixed effects, and state fixed effects. The estimates in col. 1 are for a sample that excludes disabled parents. The estimates in col. 2 are for households where all children reside in the household. The estimates in col. 3 are for households with twins or with only children. Observations are weighted using EB weights. Standard errors clustered at the household level are in parentheses.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

TABLE C6
Age-Adjusted Outcomes

	Years of Schooling (Standardized by Age) (1)	Literacy (2)	Literacy (Standardized by Age) (3)
Two-child family × disabled	.402***	.042**	.469***
	(.120)	(.018)	(.170)
Disabled sibling	−.051***	−.002	−.014
	(.015)	(.002)	(.016)
Disabled	−.861***	−.120***	−1.054***
	(.183)	(.028)	(.272)
Two-child family	.024***	.000	.007
	(.006)	(.001)	(.007)
Blind	.284*	.051**	.415*
	(.164)	(.024)	(.236)
Deaf	.042	.022	.043
	(.216)	(.031)	(.335)
Mute	−1.381***	−.204***	−1.720***
	(.149)	(.023)	(.224)
Lower extremities	−.684***	−.055**	−.493**
	(.159)	(.024)	(.233)
Mental	−2.333***	−.379***	−3.174***
	(.154)	(.024)	(.235)
Observations	380,648	379,386	379,386
R^2	.367	.348	.320

Note.—The reported estimates correspond to regressions of years of schooling standardized by age (col. 1), the literacy indicator (col. 2), and the literacy indicator standardized by age (col. 3) on the disability dummy, the indicator of two-child families, the indicator of disabled sibling, and the interaction between the disability dummy and the two-child family dummy. All specifications include the set of controls listed in table C2, age fixed effects, and state fixed effects. Observations are weighted using EB weights. Standard errors clustered at the household level are in parentheses.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

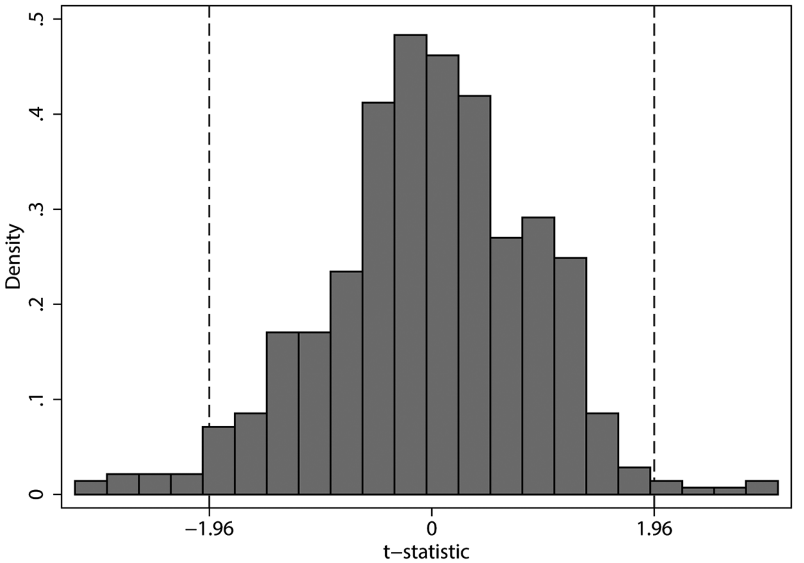

Figure C1.—Distribution of placebo t-values. The figure reports the distribution of the t-values of the test $\tilde{\theta} = 0$ obtained when estimating 500 placebo regression of equation (14). For placebo values of the disability status, the actual value is replaced by that from a randomly chosen individual.

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