Embracing Heterogeneity

Essays in Entrepreneurship and Human Capital

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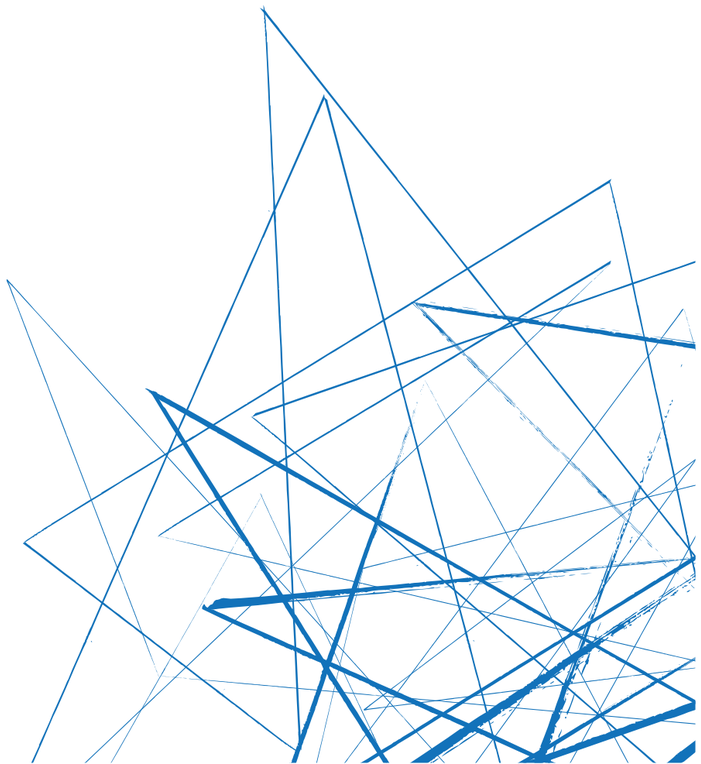

Theodor Lucian Vladasel

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The PhD School in Economics and Management is an active national and international research environment at CBS for research degree students who deal with economics and management at business, industry and country level in a theoretical and empirical manner.

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Summary

How does heterogeneity in individual and firm characteristics explain differences in occupational choice and organizational performance? This thesis considers variations in human capital profiles and organizational forms to provide insight into drivers of entrepreneurship entry, persistence, and income on the one hand, and the ability of social enterprises to motivate employee action on the other. Using econometric techniques and experimental approaches, the three essays that comprise this thesis provide novel insights into the determinants of different types of entrepreneurship and the human resource practices of social enterprises.

The first study examines the importance of family and community background as determinants of entrepreneurship, beyond a narrow focus on intergenerational associations, by estimating sibling correlations in unincorporated and incorporated business ownership. Background factors explain up to 45% of variation in entrepreneurship entry, persistence, and income and are more important for men and for incorporation – a more growth oriented type of entrepreneurship. Shared genes account for half of the observed similarities in siblings' outcomes, while parental self-employment and incorporation status explain up to 17% of sibling correlations. Parental income also matters, especially for women and incorporation; in contrast, parental education, immigrant status, family structure, and sibling peer effects contribute little to sibling similarities in entrepreneurship. Neighborhood factors, such as the local industrial structure and the share of entrepreneurs one is exposed to in adolescence, account for up to 8% of sibling correlations and explain the gap between brother and sister correlations in unincorporated self-employment.

Following this line of inquiry, the second study causally investigates the differential effects of birth order, family size, and sibling sex composition on unincorporated and incorporated entrepreneurship. While later born men are more likely to become unincorporated self-employed, this is due to their lower education and poorer labor market prospects, pointing to the subsistence nature of this type of entrepreneurship. Evidence of causal family size effects in linear and non-linear (multiple birth and sibling gender) instrumental variable approaches is limited, although children with more than four siblings are less likely to become incorporated business owners. There is also a small negative effect of having a brother on the father-daughter association in unincorporated entrepreneurship. Finally, jointly accounting for these differences increases previously estimated sibling correlations by little, confirming the role of families in generating sibling similarities, rather than differences in occupational choice.

The third essay studies how social enterprises – hybrid organizations combining features of commercial for-profits and charitable non-profits – can elicit effort from employees that differ in their social motivation. Social enterprises often exhibit revenue drift, i.e. an excessive focus on purpose at the expense of profits. Despite the threats this poses for organizational performance, social entrepreneurs are reluctant to use performance-based pay due to the perceived incongruence between incentives and social impact, as well as the risk of mission drift (the opposite of revenue drift) through the attraction of less motivated agents. In an online, real-effort experiment varying incentive strength and whether individuals can select their preferred contract, monetary rewards generate a balanced effort allocation by redirecting worker attention to commercial tasks. While strong incentives lead to a small decrease in workers' compassion, modest incentives do not affect social enterprise workforce composition and exhibit little risk of mission drift. Social enterprises that combine mission and monetary rewards not only attract more workers, but also succeed in mitigating adverse specialization (i.e. revenue drift) by directing worker attention to both commercial and social tasks.

Resumé

Hvordan kan forskelle i organisationers ydeevne forklares af heterogenitet blandt karakteristika på virksomheds- og individniveau? Denne afhandling undersøger variationer af individuelle profiler med hensyn til menneskelig kapital og organisatoriske former. Formålet er på den ene side at skabe et indblik i hvad der driver individer til at blive entreprenører samt disse individers vedholdenhed og indkomst, og på den anden side hvad der driver sociale entreprenørvirksomheders evne til at motivere sine medarbejdere. Ved brug af økonometriske teknikker og eksperimenter bidrager afhandlingens tre essays med ny indsigt omkring de afgørende faktorer i forhold til forskellige typer af entreprenørskab, og omkring tilgange i forhold til menneskelige ressourcer inden for sociale entreprenørskabsvirksomheder.

Det første studie kigger på vigtigheden af familie- og samfundsmæssig baggrund som afgørende faktorer for entreprenørskab. Studiet går udover det smalle fokus på generationsmæssige associationer ved at estimere søskendekorrelationer i forbindelse med ikke-inkorporerede og inkorporerede entreprenørskab. Baggrundsfaktorer forklarer op mod 45% af variationen i individers sandsynlighed for at blive entreprenører, hvor lang tid de forbliver entreprenører, samt hvad de tjener. Disse faktorer er mere vigtige for mænd og for den inkorporerede virksomhed – en virksomhedstype som er en mere vækstorienteret form for entreprenørskab. Fælles gener står for halvdelen af den observerede lighed mellem søskendes udfald, imens forældrenes status som selvstændige og som ejer af inkorporerede virksomheder forklarer op imod 17% af korrelationerne mellem søskende. Forældrenes indkomst har også noget at sige specielt for kvinder og for inkorporerede

virksomheder. I kontrast hertil betyder forældrenes uddannelse, immigrantstatus, familiestruktur, og den såkaldte peer-effekt blandt søskende meget lidt for søskendeligheder, når det kommer til entreprenørskab. Nabolagsfaktorer såsom lokal industristruktur og andelen af entreprenører, som individet har været eksponeret for i løbet af ungdommen forklarer op imod 8% af søskendekorrelationerne og forklarer gabet mellem bror og søster korrelationer i ikke-inkorporeret selvstændig virksomhed.

I et lignende spor er det næste studie en kausal undersøgelse af forskellige effekter af rækkefølgen i søskendeflokken, familiestørrelse, sammensætning af køn i søskendeflokken, samt ikke-inkorporeret og inkorporeret entreprenørskab. Mænd født senere i søskendeflokken har større sandsynlighed for at blive ikke-inkorporerede selvstændige. Dette kan forklares af deres lavere uddannelsesniveau og dårligere arbejdsmarkedsudsigter, hvilket er i tråd med netop denne form for entreprenørskab. Evidens fra kausale familiestørrelse-effekter i lineære og ikke-lineære (flere fødsler og søskendekøn) instrumental variable metoder er begrænset, dog har børn med flere end fire søskende mindre sandsynlighed for at blive ejere af en inkorporeret virksomhed. Der er en svag negativ effekt af at have en bror i forhold til far-datter relationen i ikke-inkorporeret entreprenørskab. Når disse forskelligheder betragtes i fællesskab øges tidligere estimerede søskendekorrelationer ikke nævntligt, hvilket bekræfter familiens rolle i at skabe ligheder blandt søskende frem for forskelligheder i valg af beskæftigelse.

Det tredje studie undersøger hvor sociale virksomheder – hybridorganisationer der kombinerer funktioner fra kommercielle profitdrævede virksomheder og velgørenheds non-profit virksomheder – kan fremkalde indsats blandt medarbejdere som er forskellige i forhold til deres sociale motivation. Sociale virksomheder har ofte et overdrevet fokus på formål og går dermed på kompromis med profit. På trods af at dette truer virksomhedens præstations, er sociale entreprenører tøvende med at bruge præstationsbaseret løn på grund af den opfattede uoverensstemmelse mellem incitamenter med social indvirkning og risikoen for at bevæge sig væk fra missionen (det modsatte

problem af at bevæge sig væk fra profitten), fordi mindre motiverede individer tiltrækkes ved denne form for lønstruktur. Gennem et online real-indsats eksperiment med varierende incitamentsyrke og variation i hvorvidt individer kan vælge deres foretrukne lønkontrakt skaber incitamenter en balanceret indsatsallokering ved at dirigere medarbejderes opmærksomhed hen mod kommercielle opgaver. Stærke incitamenter leder til at lille fald i medarbejderes medfølelse. Imens har moderate incitamenter ikke nogen effekt på medarbejdersammensætningen hos sociale virksomheder og giver kun lille risiko for tab af mission. Sociale virksomheder som kombinerer mission og monetærer belønninger tiltrækker ikke bare flere medarbejdere, men har også succes med at mindske ugunstig specialisering ved at lede medarbejdernes opmærksomhed hen mod både kommercielle og sociale opgaver.

Rezumat

În ce măsură explică eterogenitatea caracteristicilor individuale și ale firmelor diferențele în ceea ce privește alegerea ocupației profesionale și performanța organizațională? Această teză de doctorat analizează variațiile întâlnite în profilurile capitalului uman și în formele organizaționale pentru a arăta, pe de o parte, care sunt motoarele începerii unei activități antreprenoriale, ale persistenței și veniturilor antrepreneuriale, iar pe de altă parte, abilitatea întreprinderilor sociale de a-și motiva angajații să acționeze conform scopurilor întreprinderii. Folosind tehnici econometrice și abordări experimentale, cele trei eseuri care formează prezenta lucrare oferă o perspectivă inedită asupra factorilor determinanți ai diferitelor tipuri de antreprenoriat și asupra practicilor din domeniul resurselor umane puse în aplicare în întreprinderile sociale.

Primul studiu analizează importanța familiei și a comunității în calitate de factori determinanți ai antreprenoriatului. Dincolo de corelații intergenerationale între ocupațiile părinților și copiilor, acest studiu estimează corelații între rezultatele antreprenoriale – deschiderea unei afaceri, persistența și veniturile antreprenoriale, în societăți comerciale cu sau fără personalitate juridică – ale copiilor din aceeași familie (corelații intrafamiliale). Mediul familial de proveniență explică până la 45% din variațiile întâlnite în începerea unei activități antreprenoriale, persistență și venit, fiind mult mai semnificativ pentru bărbați și pentru societățile cu personalitate juridică – o formă de antreprenoriat mai orientată spre creștere, cu necesități de capital uman și financiar ridicate. Genele comune explică jumătate din asemănările observate în rezultatele antreprenoriale ale fraților și surorilor, în timp ce faptul că părinții au o activitate antreprenorială (și statutul de societate cu

personalitate juridică al acestei activități) explică până la 17% din corelațiile intrafamiliale. Venitul părinților este de asemenea important, mai ales pentru femei și pentru societățile cu personalitate juridică; în schimb, educația părinţilor, statutul de imigrant, structura familială și influențele reciproce ale fraților și surorilor contribuie într-o mică măsură la asemănările dintre copiii din aceeași familie în domeniul antreprenoriatului. Diferențele din compoziția industriei locale și numărul de antreprenori la care persoanele studiate au fost expuse în adolescență reprezintă până la 8% din corelațiile intrafamiliale și explică diferenţa dintre corelațiile bărbaților și surorilor în activitatea antreprenorială fără personalitate juridică.

Continuând această linie de cercetare, cel de-al doilea studiu examinează efectele pe care le au ordinea nașterii, mărimea familiei și sexul copiilor asupra antreprenoriatului cu și fără personalitate juridică. Deși este mai probabil ca bărbații născuți mai târziu să înceapă o activitate independentă fără personalitate juridică, acest lucru este cauzat de nivelul mai scăzut de educație și de perspectivele mai slabe pe piața muncii, ceea ce demonstrează caracterul de subzistență asociat acestui tip de antreprenoriat. Există dovezi limitate în ceea ce privește efectele cauzale ale mărimii familiei în modele liniare și non-liniare de regresie cu variabile instrumentale (folosind nașteri multiple și sexul copiilor drept instrumente), deși este puțin probabil ca persoanele cu mai mult de patru fraţi și surori să înființeze o societate cu personalitate juridică, posibil datorită constrângerilor cu privire la capitalul uman și financiar întâlnite în aceste familii. De asemenea, într-o familie în care tatăl deține o afacere, este mai puțin probabil ca fiica să devină antreprenor dacă aceasta are un frate. În ultimul rând, aceste diferențe luate împreună duc la o creștere ușoară a corelațiilor intrafamiliale estimate anterior, confirmând rolul familiilor în generarea unor asemănări, mai degrabă decât a unor diferențe între copiii din aceeași familie în ceea ce privește alegerea unei ocupații profesionale.

Cel de-al treilea eseu analizează întreprinderile sociale (organizații hibride ce îmbină caracteristicile întreprinderilor comerciale lucrative și cele ale organizațiilor caritabile non-profit) și modul în care acestea determină angajații cu motivații sociale diferite să acționeze în interesele firmei.

Întreprinderile sociale înregistrează adesea o deviere de la profit, adică o concentrare excesivă asupra scopurilor caritabile, neglijând profiturile. Deși acest lucru le periclitează performanţa organizațională și supraviețuirea, antreprenorii sociali ezită să folosească o remunerație în funcţie de performanță din cauza a ceea ce ei consideră a fi o incompatibilitate între stimulente financiare și impact social, precum și a riscului de a devia de la misiunea întreprinderii (care se opune riscului de a devia de la profit), prin atragerea unor angajați mai puțin motivați. Într-un experiment ce variază nivelul stimulentelor bănești și posibilitatea indivizilor de a-și alege tipul de contract, stimulentele financiare duc la o alocare echilibrată a efortului, redirecționând atenția angajaţilor către sarcinile comerciale. În timp ce stimulentele ridicate duc la o ușoară diminuare a compasiunii angajaţilor, stimulentele moderate nu afectează compoziția forței de muncă dintr-o întreprindere socială și prezintă un risc scăzut de deviere de la misiunea acesteia. Întreprinderile sociale care oferă atât recompense financiare, cât și recompense legate de misiunea lor socială nu numai că atrag mai mulți angajați, dar și reușesc să atenueze specializarea adversă, orientând atenția angajaților către sarcinile comerciale și sociale în egală măsură.

Contents

Chapter 1

Introduction

Motivation

Individual and firm-level heterogeneity are core concepts in economics and management, helping to explain differences in labor market outcomes, occupational choice, and organizational performance (Barney, 1991; Murphy et al., 1991; Becker, 1994; Teece et al., 1997; Syverson, 2011; Benabou and Tirole, 2016). The link between differences at the individual and firm level is particularly strong in (social) entrepreneurship, where founder ability and motivation are closely tied to organizational outcomes (Shane and Venkataraman, 2000; Zahra et al., 2009; Hurst and Pugsley, 2011; Battilana et al., 2015; Stevens et al., 2015). Entrepreneurship – both commercially and socially oriented – is the principal factor behind business dynamism (Decker et al., 2014), creating jobs, generating innovations, and ultimately spurring economic growth and societal welfare (van Praag and Versloot, 2007; Haltiwanger et al., 2013; Battilana and Lee, 2014). Inquiries into the effects of human capital profiles on (social) entrepreneurship entry and performance are therefore paramount in the quest to generate not only more, but also better entrepreneurs.

Building on classic studies positing that more able, less risk averse, or richer individuals select into entrepreneurship (Lucas Jr., 1978; Kihlstrom and Laffont, 1979; Carroll and Mosakowski, 1987; Evans and Jovanovic, 1989; Evans and Leighton, 1989; Bates, 1990; Gimeno et al., 1997), the past

decade has acknowledged that entrepreneurs have substantially different backgrounds and that their start-up activities represent distinct phenomena (Hurst and Pugsley, 2011; Henrekson and Sanandaji, 2014, 2019). Intuitively, individuals whose human capital and financial resources allow them to earn more from their new ventures than from paid employment may prefer to become entrepreneurs; conversely, individuals whose (limited) skills are less valued in the labor market are more likely to be self-employed. As a consequence, individuals from both tails of the ability distribution – with ability understood as a complex bundle of human and financial capital – have a higher likelihood of entrepreneurial entry, thus generating 'stars and misfits' or 'hobos and high-fliers' (Åstebro et al., 2011; Åstebro and Thompson, 2011; Andersson-Joona and Wadensjö, 2013; Ng and Stuart, 2016; Levine and Rubinstein, 2017, 2018).

More pragmatically, recent research has focused on distinguishing entrepreneurial motives and how these are reflected in organizational choices (Åstebro and Thompson, 2011; Roach and Sauermann, 2015). For example, Hurst and Pugsley (2011, 2015) argue that most business owners enjoy non-pecuniary benefits, without expressing growth ambitions. Schoar (2010), Henrekson and Sanandaji (2014, 2019), and Acs et al. (2016) have persuasively argued that self-employment is a poor proxy for innovative, Schumpeterian entrepreneurship. Instead, firm incorporation may provide a more accurate proxy for entrepreneurs' ambitions (Levine and Rubinstein, 2017). Incorporation is a strategic decision that offers founders the benefits of limited liability and tax advantages, allowing them to pursue riskier, but more rewarding ideas, at the cost of minimum capital requirements and increased regulatory oversight. As a result, only entrepreneurs who anticipate higher performance are expected to incorporate their business.

Empirically, incorporated enterprises have higher ability founders, larger revenue, and higher innovation, growth, job creation, and IPO rates relative to unincorporated firms (Guzman and Stern, 2016; Tåg et al., 2016; Åstebro and Tåg, 2017; Levine and Rubinstein, 2017; van Praag and Raknerud, 2017). As a result, the success of incorporated entrepreneurs contributes to top

income inequality (Halvarsson et al., 2018). I therefore consider incorporation a marker of growth oriented entrepreneurship, compared to small-scale, unincorporated self-employment, a dichotomy that loosely parallels the often used distinction between necessity and opportunity entrepreneurship (Levine and Rubinstein, 2018; Dencker et al., 2019). Understanding how the determinants of these types of entrepreneurship differ and whether they create obstacles to entry and growth is therefore an important task, with implications for both policy and individual human capital decisions.

Beyond innate traits, contexts shape individual preferences for entrepreneurship. Organizations are often regarded as 'fonts of entrepreneurship' (Sørensen and Fassiotto, 2011), spawning successful, competitive new ventures (Gompers et al., 2005; Klepper and Sleeper, 2005; Franco and Filson, 2006; Chatterji, 2009; Campbell et al., 2012).[1] Contextual effects are found in academic environments (Stuart and Ding, 2006; Roach and Sauermann, 2015; Kacperczyk, 2013; Lerner and Malmendier, 2013); in smaller, more entrepreneurial, less bureaucratic, and less hierarchic organizations (Dobrev and Barnett, 2005; Sørensen, 2007a; Özcan and Reichstein, 2009; Elfenbein et al., 2010; Nanda and Sørensen, 2010; Kacperczyk, 2012; Sørensen and Sharkey, 2014; Tåg et al., 2016); and in regional environments (Sorenson and Audia, 2000; Stuart and Sorenson, 2003; Giannetti and Simonov, 2009; Dahl and Sorenson, 2012; Guiso et al., 2015).

One particularly salient influence is provided by family and community background (Dunn and Holtz-Eakin, 2000; Aldrich and Cliff, 2003). Although research in this area has emphasized parental entrepreneurship as the main determinant of entrepreneurship (Blanchflower and Oswald, 1998; Hout and Rosen, 2000; Sørensen, 2007b; Lindquist et al., 2015), parents pass on genetic endowments, provide financial resources, a home environment, an extended family, and a social context, including neighborhoods and schools. Therefore, a broader inquiry into the role of families and communities could prove fruitful in illuminating the early life investments and interactions that shape entrepreneurship.

[1] Individual-firm educational or skill mismatches and disagreements over the value of inventions may also drive entrepreneurship entry (Gambardella et al., 2015; Stenard and Sauermann, 2016).

Organizations also exhibit heterogeneity in their mission. Besides primarily commercial or for-profit firms, entrepreneurs may found charitable, non-profit organizations (Rose-Ackerman, 1996; Glaeser and Shleifer, 2001) and social enterprises – that is, companies that combine commercial and social goals, tackling societal challenges with market-based business models (Austin et al., 2006; Dacin et al., 2011; Pache and Santos, 2013; Besley and Ghatak, 2017).[2] Due to their hybrid nature, social enterprises operate in a contested field, where commercial and social imperatives prescribe competing courses of action; these tensions make achieving the 'double bottom line' difficult, and many social enterprises struggle to achieve balance (Pache and Santos, 2010). To effectively serve their beneficiaries, social enterprises must allocate scarce employee effort to commercial and social tasks (Battilana and Dorado, 2010; Battilana and Lee, 2014; Besharov, 2014). Deviations towards profit to the detriment of purpose – i.e. mission drift – are often regarded by social entrepreneurs and potential employees as undesirable and contrary to the essence of social enterprises (Jones, 2007; Ebrahim et al., 2014; Ramus and Vaccaro, 2017; Grimes et al., 2018).

In practice, due to their embeddedness in a social logic espousing an emphasis on social impact, a reluctance to employ practices stemming from a commercial logic, and high levels of other-regarding preferences for both founders and employees (Miller et al., 2012; Santos, 2012; Stevens et al., 2015; Besley and Ghatak, 2017), social enterprises often pay insufficient attention to revenue generation. As purpose takes priority over profits, revenue drift hinders social enterprises' ability to deliver on their social mission and threatens their survival (Smith et al., 2013; Tracey et al., 2011; Battilana et al., 2015; Davies and Doherty, 2018; Staessens et al., 2018). Nonetheless, social entrepreneurs are reluctant to use pay for performance – a tool often used by commercial enterprises to motivate effort (Austin et al., 2006; Dees, 2012). Investigating whether social enterprises can tackle these internal tensions through the use of pay for performance is therefore important

[2] Social enterprises are an increasingly important phenomenon across the world, in both developing and developed economies. Short et al. (2009) notes that 6.6% of workers in the United Kingdom (UK) were involved in social enterprise activities at the time, with an upward trend. Most countries have an established network of social ventures (e.g., Social Enterprise UK) and several high-profile organizations – such as Ashoka or the Skoll Foundation – actively encourage innovative social ventures through contests, prizes, and fellowships.

for organizational theory, providing insights into the hiring and socialization practices adopted by hybrid organizations (Battilana and Dorado, 2010; Pache and Santos, 2013; Boone and Özcan, 2016; Smith and Besharov, 2019), the combination of monetary and mission incentives for workers who differ in their social motivation (Besley and Ghatak, 2005; Burbano, 2016; Cassar, 2019) in a multitasking framework (Kerr, 1975; Holmström and Milgrom, 1991), and the effects of incentives on attention allocation (Ocasio, 1997; Kaplan and Henderson, 2005; Ethiraj and Levinthal, 2009) relative to employee self-selection (Lazear, 2000; Cadsby et al., 2007). Moreover, such an investigation carries practical implications, where pay for performance, although seemingly incompatible with social impact (Bacchiega and Borzaga, 2001; Dees, 2012), may ultimately help social enterprises avoid revenue drift.

Thesis Structure

This thesis consists of three independent chapters, summarized in Table 1.1. Although the core focus in answering each research question is empirical, I draw on theories of human capital, occupational choice, and multitasking to build theoretical predictions and identify the relevant mechanisms behind the findings. Methodologically, I provide both descriptive and causal evidence, relying on econometric techniques – including variance decompositions, (correlated) random effects, fixed effects, and (linear and non-linear) instrumental variables, applied to detailed register data from Sweden – and experimental approaches.

Chapter 2, co-authored with Matthew J. Lindquist (SOFI, Stockholm University), Joeri Sol (University of Amsterdam), and Mirjam van Praag (CBS and VU Amsterdam), estimates sibling correlations to quantify the total importance of family and community background for unincorporated and incorporated entrepreneurship entry, persistence, and income. We then unpack the channels through which family and community background exert their influence on sibling similarity, including neighborhoods, parental income, entrepreneurship, and immigration status, family structure, sibling peer effects, shared genes, and (non)cognitive ability.

Table 1.1: **Thesis Summary**

Chapter and Title	Research question	Heterogeneity	Method and Data
2. On the Origins of Entrepreneurship: Evidence from Sibling Correlations	How much variation in entrepreneurship outcomes do family and community background explain?	Individual: between families gender Firm level: unincorporated and incorporated	Descriptive Variance decomposition (random effects) Swedish register $N \approx 700,000$
3. Same, but Different? Birth Order, Family Size, and Sibling Sex Composition Effects in Entrepreneurship	Do families generate sibling similarities or differences (by birth order, family size, and gender) in entrepreneurship?	Individual: within families gender Firm level: unincorporated and incorporated	Descriptive/causal Fixed effects Instrumental variables (linear/non-linear) Swedish register $N \approx 700,000$
4. Striking a Balance: Revenue Drift, Incentives, and Effort Allocation in Social Enterprises	Can social enterprises use monetary rewards to mitigate adverse specialization and elicit balanced effort?	Individual: social motivation Firm level: organizational forms	Causal Online experiment (real effort) Prolific.ac.uk $N = 708$

In Chapter 3, I shift the focus from siblings' shared background towards within-household heterogeneity, focusing on the differential effects of birth order, family size, and sibling sex composition on unincorporated and incorporated entrepreneurship entry in a set of causal exercises. To provide a unifying framework, I then jointly account for these differences in the sibling correlations estimated in the previous chapter, completing the picture on the economic and social family relationships that shape the next generation of entrepreneurs.

Finally, Chapter 4, joint with Simon C. Parker (Ivey Business School), Randolph Sloof (University of Amsterdam), and Mirjam van Praag (CBS and VU Amsterdam), studies how monetary incentives affect effort allocation in social enterprises – where socially motivated agents may place purpose ahead of profits and thereby endanger organizational performance and survival. We use an online experiment to test our predictions regarding the intensive and extensive margin effects of monetary rewards on effort balance.

Chapter 2: On the Origins of Entrepreneurship: Evidence from Sibling Correlations

The origins of (successful) entrepreneurial behavior are not yet fully understood. Beyond factors affecting individual predisposition (Parker, 2009), contextual influences – from communities, universities, or organizations – matter. Despite their strength, these influences stem partly from individual selection into such environments, based on ability and preferences (Özcan and Reichstein, 2009; Elfenbein et al., 2010; Roach and Sauermann, 2015; Tåg et al., 2016). The source of entrepreneurial behavior should therefore be investigated at an earlier stage in individuals' lives. A natural starting point for such an inquiry is provided by family and community background, a prominent context during individuals' formative years.

The pervasive and long-lasting impact of childhood environment on economic outcomes is widely recognized in economics (Becker, 1988; Solon, 1999; Chetty et al., 2016). This environment is a strong determinant of many entrepreneurial antecedents, such as (non)cognitive ability and education (Grönqvist et al., 2017; Levine and Rubinstein, 2017), job values and preferences (Halaby, 2003; Roach and Sauermann, 2015), and the availability of resources and learning opportunities (Sørensen, 2007b; Guiso et al., 2015; Lindquist et al., 2015). Regardless of the proximate predispositional and contextual pathways, family and community background are inextricably linked to entrepreneurship (Hout and Rosen, 2000; Aldrich and Cliff, 2003).

Chapter 2 conducts a systematic investigation of the importance of family and community background in shaping entrepreneurship outcomes. While parental entrepreneurship has been highlighted as the main intergenerational link (Blanchflower and Oswald, 1998; Dunn and Holtz-Eakin, 2000; Lindquist et al., 2015), this represents an overly narrow focus, as families affect individual ability, values, resources, and economic outcomes in multiple ways. To quantify the importance of family background in entrepreneurship, we apply methods from labor economics to

population representative Swedish register data. Specifically, we estimate sibling correlations, or the extent to which siblings have similar entrepreneurship outcomes compared to random individuals from the population (Solon, 1999).[3] The study of sibling correlations allows us to highlight i) the importance of family and community for entrepreneurship, ii) the elements of background that matter, and iii) how the explanatory power of these factors differs between unincorporated and incorporated business owners.

In our data, incorporated entrepreneurs have higher cognitive and noncognitive ability, more balanced skills, higher education and lifetime incomes than unincorporated entrepreneurs, as well as more successful parents, and enter more capital-intensive industries, validating the entrepreneurial dichotomy we employ. While Sweden potentially represents a particular context – with a flexible labor market, generous welfare state, and compressed income distribution (Björklund and Jäntti, 1997) – the distinctions between unincorporated and incorporated businesses are similar to those in other countries, such as the United States (Levine and Rubinstein, 2017; Halvarsson et al., 2018).

We find that up to 45% of variation in entrepreneurship entry and success is explained by background, mainly through role models inside and outside the household, family resources, and genes. Sibling similarity in growth oriented entrepreneurship is explained by having incorporated parents, growing up in neighborhoods with more incorporated businesses, and having richer parents (especially for women, who face stronger capital constraints). Genes explain half of sibling correlations, similarly for brothers and sisters, and similarly for unincorporated and incorporated firms. When we focus on the traits that may be passed on genetically, leadership skills appear particularly important for incorporation, where individuals are expected to manage a larger enterprise. By contrast, we find a minor role for parental education, immigration status, and family structure, and evidence for sibling peer effects is limited to brothers' unincorporated self-employment.

[3] Sibling correlations represent a variance decomposition technique. Similar methods have been applied in both economics (Griliches, 1979; Solon et al., 1991) and management (McGahan and Porter, 1997; Mollick, 2012).

Chapter 3: Same, but Different? Birth Order, Family Size, and Sibling Sex Composition Effects in Entrepreneurship

While family background creates substantial similarities in siblings' entrepreneurship outcomes, families also generate sibling differences, which are not captured by sibling correlations (Conley, 2004; Björklund and Jäntti, 2012). For this reason, Chapter 3 examines variation between children in the same family with regards to becoming an entrepreneur. Human capital literature suggests that families may treat siblings differently with regards to birth order, gender composition, or their interplay with family size (Butcher and Case, 1994; Black et al., 2005). These closely related factors are economically and statistically important for human capital development and may thus prove relevant for occupational choice. However, research in this area has only considered a broad definition of self-employment (Han and Greene, 2016; Black et al., 2018; Mishkin, 2017), failing to distinguish entrepreneurial heterogeneity. In this chapter, I first causally assess each source of sibling differences in unincorporated and incorporated entrepreneurship in Sweden, then jointly account for them to produce revised estimates of the importance of family background.

First, using family fixed effects models (Black et al., 2005, 2018), I show that causal birth order effects are limited to a higher entry of later born men into unincorporated self-employment. This effect is driven by strong negative birth order effects in education, as later born men face poorer labor market prospects. I find no birth order effects for women and incorporation. Second, once the endogeneity of parents' fertility decision is addressed in instrumental variable models based on multiple births and the gender of the first two children (Angrist and Evans, 1998; Black et al., 2005; Angrist et al., 2010), I find little evidence of causal family size effects in entrepreneurship: only children with more than four siblings have a lower rate of incorporation.

Third, pure sibling sex composition effects are absent: a woman growing up with a sister, as opposed to a brother, is not more likely to become an entrepreneur. This result holds when I get

closer to a causal interpretation by focusing on first born children, assuming that the next child's gender is quasi-exogenous (Cools and Patacchini, 2019; Peter et al., 2018). However, the presence of a brother reduces the father-daughter association in unincorporated self-employment (although this relationship is weaker in Sweden than in the United States, Mishkin, 2017), and there are countervailing effects for incorporation and mothers' business ownership.

Quantitatively, accounting for within-family differences increases previously estimated sibling correlations by 1.2-2 percentage points (or 3.2%-6.7%), especially in families with both boys and girls, where all sources of within-family heterogeneity can be expected. In conclusion, families are mainly responsible for generating similarities rather than differences and the role of families in entrepreneurship is only marginally understated.

Chapter 4: Striking a Balance: Revenue Drift, Incentives, and Effort Allocation in Social Enterprises

As hybrid organizations combining commercial and social logics (Austin et al., 2006; Dacin et al., 2011), social enterprises must allocate scarce employee effort between commercial and social tasks in order to deliver on their dual objectives (Battilana and Dorado, 2010; Besharov, 2014). However, many social enterprise employees prioritize purpose over profits (Battilana et al., 2015; Stevens et al., 2015). This excessive focus on social impact to the detriment of revenue generation – or 'revenue drift' (Ebrahim et al., 2014) – may ultimately threaten social enterprises' financial sustainability and survival (Smith et al., 2013). For example, a social enterprise tackling homelessness endangered its own survival by focusing excessively on beneficiary needs at the expense of required operational investments (Tracey and Jarvis, 2006; Tracey et al., 2011); similarly, a fair trade social enterprise insufficiently heeding customer demands and prioritizing coffee producers instead suffered from falling sales and operational difficulties for several years, limiting its ability to serve beneficiaries effectively (Davies and Doherty, 2018).

While we might expect social enterprises to offer pecuniary rewards to induce employees to balance their effort between commercial and social tasks, social enterprises rarely employ monetary incentives (Battilana and Lee, 2014). On the one hand, potential employees may perceive a tight coupling of pay and commercial performance as incompatible to social enterprise values (Austin et al., 2006; Tracey et al., 2011; Besharov, 2014). Identity tension is especially likely if employees associate monetary rewards with the competitive 'bonus culture' and profit motive traditionally characterizing a commercial logic (Dees, 2012; Benabou and Tirole, 2016; Dimitriadis et al., 2017).[4] On the other hand, social entrepreneurs may be anxious about the danger of mission drift, where commercial imperatives overshadow social concerns (Ebrahim et al., 2014), thereby choosing to eschew monetary incentives (Bacchiega and Borzaga, 2001).

Chapter 4 questions whether these concerns are well-founded. Drawing on organizational theory and economics (Kaplan and Henderson, 2005), we argue that given the predominance of socially motivated employees and a mission emphasis in social enterprises (Miller et al., 2012; Smith et al., 2013; Besley and Ghatak, 2017), an absence of pay for performance leads to an unbalanced effort allocation, with employees favoring social impact over revenue generation. In a multitasking framework (Holmström and Milgrom, 1991), workers' high levels of intrinsic motivation induce adverse specialization (MacDonald and Marx, 2001), endangering social enterprises' performance. While modest incentives (i.e. small performance bonuses) can elicit balanced effort between commercial and social tasks, we expect that strong incentives (i.e. large bonuses) lead to mission drift by distorting effort too far in the direction of commercial imperatives.

We conduct an incentivized, real-effort, online experiment to test our theoretical predictions and find that monetary incentives elicit a more balanced effort allocation, regardless of their steepness, while we find a small, but significant downward shift in employees' social motivation when strong incentives are offered. These results suggest that for social enterprises to effectively

[4] More than two thirds of commercial firms use pay for performance at the individual level for a large share of employees (see, e.g., Lazear and Shaw, 2007; Gerhart and Fang, 2014).

motivate employees, they must use both mission and monetary rewards. Moreover, incentives work by focusing employee attention on tasks the organization finds valuable, thereby performing a normative function (Ocasio, 1997; Kaplan and Henderson, 2005; Ethiraj and Levinthal, 2009).

Contributions

This thesis contributes to our understanding of the effects of individual heterogeneity on the creation and performance of different types of organizations. Chapter 2 analyzes how differential exposure to childhood environments creates large *between-family differences* in entrepreneurship entry, persistence, and income. Chapter 3 focuses instead on *within-family heterogeneity*, where differential parental treatment and sibling interactions lead to distinct human capital profiles. Both chapters also assess *gender heterogeneity*, as family background matters in different ways for men and women. Moreover, the thesis analyzes *unincorporated and incorporated* business owners as distinct types of entrepreneurs, roughly equivalent to small scale self-employment and, respectively, growth oriented entrepreneurship. Chapter 4 considers *organizational form heterogeneity*, with a landscape including for profits, nonprofits, and social enterprises as employers. Finally, employees *differ in social motivation*, which not only affects their occupational choice (i.e. contract selection), but also their effort allocation. Below, I summarize the contributions the thesis makes to several strands of literature.

First, the thesis documents the importance of family and community background as a strong explanatory factor for entrepreneurship entry and performance, accounting for up to 45% of variation in entrepreneurship outcomes, split roughly equally between nature and nurture. These sibling correlations are up to five times larger than what previously estimated intergenerational associations suggest, implying that early exposure to family and community influences has a broad and long-lasting impact on individual entrepreneurship. In other words, exposure to different childhood environments – i.e. between-family heterogeneity – has large effects on individuals' long-run propensity to become business owners. Moreover, these correlations are higher than sibling corre-

lations in other outcomes, suggesting that occupational choice is a particularly fertile ground for family influences. In this spirit, it would be interesting for future research to delve further into the role of background for other labor market outcomes, such as becoming an inventor (Aghion et al., 2018; Bell et al., 2018) or a manager (Black et al., 2018; Custódio and Siegel, 2018; Campbell et al., 2019). More broadly, the large importance of family background for incorporated entrepreneurship in Sweden – an egalitarian country with substantial safety nets – may imply a striking dependency of individual growth-oriented entrepreneurship on parental and community influences in more unequal economies such as the U.S. (Black and Devereux, 2011; Björklund and Jäntti, 2011). As inequality in income (and entrepreneurial human capital) is expected to increase globally, programs providing young individuals with exposure to entrepreneurial role models and learning opportunities (Huber et al., 2014; Elert et al., 2015; Fairlie et al., 2015; Eesley and Wang, 2017; Lyons and Zhang, 2018) as well as access to finance (Lerner, 2009; Lelarge et al., 2010) may become even more important in the future.

Second, this thesis contributes to the literature on selection into entrepreneurship from the tails of the ability distribution (Åstebro et al., 2011; Andersson-Joona and Wadensjö, 2013; Levine and Rubinstein, 2017, 2018). Incorporated firms are started by individuals with i) higher and more balanced cognitive and noncognitive ability, ii) higher education and incomes, and iii) exposure to wealthier and more entrepreneurial parents. The strategic incorporation decision is thus associated with larger, more capital intensive, growth oriented firms. By contrast, unincorporated firms are started by individuals with i) lower ability and education, ii) more specialist, technical skills, and iii) poorer labor market prospects. Individuals in between these extremes are more likely to become wage employees. Thus, the tails of the ability distribution are associated with entry into different types of entrepreneurship, and policy makers aiming to foster new ventures should pay attention to this distinction (Shane, 2009; Acs et al., 2016). The disaggregation of business owners into unincorporated and incorporated also reveals that parental (and to some extent neighborhood)

role models are specific to a given type of entrepreneurship. Put simply, to encourage growth oriented ventures, we must expose individuals to growth oriented ventures (or the within family and neighborhood factors associated with such firms).

Third, family and community influences are different for men and women. Brother correlations are always larger than sister correlations, especially for unincorporated self-employment. This gap is explained by exposure to male-dominated, geographically concentrated professions that are often pursued through self-employment, as well as older brothers acting as role models for younger ones; that birth order effects are only visible for men also speaks to the importance of social mechanisms for boys. Unincorporated fathers may also find it easier or more desirable to pass on their skills to sons rather than daughters, potentially hindering the transfer of entrepreneurial human capital. The family's final resources are more important for women's decision to become incorporated, suggesting potential differences in perceived risk or difficulties in accessing finance for women relative to men. Disentangling these channels is an important avenue for future research, with important implications for reducing the gender gap in entrepreneurship.

Finally, this thesis speaks to a growing literature on social enterprises' human resource practices. Entrepreneurs who choose to pursue a blend of commercial and social goals (Dacin et al., 2011; Miller et al., 2012) must ensure that employees allocate roughly equal amounts of effort to commercial and social activities to avoid mission drift and revenue drift (Battilana and Dorado, 2010; Battilana et al., 2015; Tracey et al., 2011). The theoretical contribution of this thesis is to map this setting onto a multitasking framework with motivated workers (Holmström and Milgrom, 1991; MacDonald and Marx, 2001; Jones et al., 2018) and argue that pay for commercial performance mitigates adverse specialization; in other words, only the combination of mission and monetary rewards succeeds in making workers balance their effort between competing tasks. The empirical contribution is to show experimentally that monetary incentives – contrary to the usual expectation of alienating socially motivated workers and inducing mission drift – focus socially

motivated employees' attention on desirable tasks, thus performing a normative function (Ocasio, 1997; Kaplan and Henderson, 2005; Ethiraj and Levinthal, 2009). Insights from multitasking theory may further be used in social entrepreneurship to understand the adoption of complementary practices from commercial and social logics (Pache and Santos, 2013), as well as the business models prone to mission or revenue drift (Ebrahim et al., 2014).

In a broader perspective, the first two studies highlight the large importance of families for entrepreneurship. Parental influences – stemming from both nature and nurture, and operating through a variety of mechanisms – tower over community influences; and while differences between children in the same family are limited, disparities in exposure to entrepreneurial families may limit individuals' perception of entrepreneurship as a feasible career or their ability to become successful business owners. The final study emphasizes that social entrepreneurs could and should engage with tools characteristic of commercial ventures to address internal tensions in effort allocation. Whether and how pay for performance complements other hiring and socialization practices, governance mechanisms, and social impact rewards constitute research avenues that may improve social enterprise theory and practice in the future.

Chapter 2

On the Origins of Entrepreneurship: Evidence from Sibling Correlations

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2.1 Introduction

Entrepreneurship is often hailed as a driver of innovation, job creation, and growth. However, the origins of (successful) entrepreneurial behavior are not yet fully understood. Individual preferences, ability, education, and financial resources, all feature as potential dispositional determinants of entrepreneurship (Parker, 2009). More recently, the contextual influence of universities, organizations, or neighborhoods has also received attention.[1] While each of these contextual effects has been convincingly documented, they stem partly from the selection of individuals into such environments, based on ability and preferences (Özcan and Reichstein, 2009; Elfenbein et al., 2010; Roach and Sauermann, 2015; Tåg et al., 2016). This implies that the source of entrepreneurial behavior should be investigated at an earlier stage in individuals' lives. We argue that a natural starting point for such an inquiry is provided by an individual's family and community background. To this end, we conduct a systematic assessment of the importance of family and community background as determinants of entrepreneurship.

Studies of the role of family background for entrepreneurship usually emphasize parental entrepreneurship (Lentz and Laband, 1990; Blanchflower and Oswald, 1998; Dunn and Holtz-Eakin, 2000; Aldrich and Kim, 2007; Laspita et al., 2012; Hoffmann et al., 2015). While its impact on individual entrepreneurship has been convincingly documented, working through role-modeling and/or the transfer of entrepreneurship-specific human capital (Sørensen, 2007b; Lindquist et al., 2015), parents' entrepreneurship is only one of many ways through which they influence their children's entrepreneurial choices and outcomes. Parents pass on genetic endowments, provide income, a home environment, an extended family, and a social context in which children grow up, including

[1] Stuart and Ding (2006) and Roach and Sauermann (2015) study the effect of academic environments; Nanda and Sørensen (2010), Kacperczyk (2013) and Lerner and Malmendier (2013) focus on workplace and university peers; Dobrev and Barnett (2005), Sørensen (2007a), Özcan and Reichstein (2009), Elfenbein et al. (2010), Kacperczyk (2012), Sørensen and Sharkey (2014), Tåg et al. (2016) study how organizational bureaucracy, size, and hierarchy spawn entrepreneurs; Sorenson and Audia (2000), Stuart and Sorenson (2003), Giannetti and Simonov (2009), Dahl and Sorenson (2012), and Guiso et al. (2015) examine the effects of an entrepreneurial network, local embeddedness, and youth exposure to regional entrepreneurial density.

neighborhoods, schools, and churches. These factors combine in different ways to encourage or discourage an individual's choice to become an entrepreneur as an adult, beyond the influence of parental entrepreneurship.

In this paper, we argue that intergenerational correlations in entrepreneurship should be viewed as narrow and insufficient measures of the overall importance of family background for entrepreneurship. Instead, our methodological contribution is to estimate sibling correlations in entrepreneurship. Sibling correlations can be viewed as omnibus measures of the importance of family background and neighborhood effects in determining a given outcome (Solon, 1999; Björklund and Jäntti, 2011; Black and Devereux, 2011). Intuitively, sibling correlations measure the similarity in outcomes of siblings relative to the similarity in outcomes of individuals drawn randomly from the population: the larger the correlation, the more important shared background is for generating sibling similarities. Thus, sibling correlations measure the fraction of variation in the outcome variable that is due to shared family background and neighborhood effects.[2]

Our sibling approach allows us to make two contributions to the entrepreneurship literature. First, we quantify the *overall* importance of family and community background as determinants of business ownership, beyond simple intergenerational associations entrepreneurship. What share of the variation in entrepreneurial outcomes is driven by family background and neighborhood effects? Our results indicate these influences are substantial and (up to five times) larger than was assumed based on parent-child transmission studies. Second, we discuss the *relative* importance of various determinants discussed in the previous literature within a single, unified framework. What is it that parents do that makes their children so similar? Which background characteristics influence entrepreneurial outcomes the most?

[2] Although our paper is (to the best of our knowledge) the only one to focus its full attention on sibling correlations in entrepreneurship, some twin studies calculate twin correlations in order to estimate heritability in entrepreneurship. Nicolaou et al. (2008) do not report these correlations; Zhang et al. (2009) and Nicolaou and Shane (2010) do report raw correlations for Swedish and U.S. twins, but do not discuss them at length, as they focus on measuring heritability. Zunino (2016) studies gene-environment interaction effects using Italian twins in a more standard regression framework and, hence, does not report sibling correlations comparable to our own.

To compute sibling correlations in entrepreneurial outcomes, we use data from Sweden's Multigenerational Register on nearly 700,000 children born between 1960 and 1970. For the years 1993-2012 we have information from the Swedish tax authority concerning business ownership for all of these individuals and their parents. We also have data on individual and family socioeconomic variables, including information on education, income, family structure, immigration status and parish of residence. For most brothers in our sample, we have measures of cognitive and non-cognitive skills at age 18 taken from their military draft records.

We operationalize entrepreneurship using information on business ownership. We classify individuals as business owners in any given year if they receive the majority of their taxable labor earnings from a company they own in full or in part. We then define two different types of entrepreneurs: those who own and operate unincorporated firms, i.e. the self-employed, are distinguished from those who own and manage incorporated (non-listed, limited liability) firms.[3] We make this distinction for several reasons. Unincorporated firms are typically small, owner-operated firms with no employees; by contrast, incorporated firms have, on average, more employees (Hurst and Pugsley, 2011; Åstebro and Tåg, 2017), a higher likelihood of growth and reaching an IPO (Guzman and Stern, 2016), and create more income for the business owner (Levine and Rubinstein, 2017; Humphries, 2017; van Praag and Raknerud, 2017). In Sweden, incorporated firms contribute to top income inequality, whereas unincorporated firms increase inequality at the bottom of the distribution (Halvarsson et al., 2018). The entrepreneurship literature has therefore questioned whether self-employment is a good proxy for Schumpeterian entrepreneurship (Schoar, 2010; Henrekson and Sanandaji, 2014, 2019) and has suggested that incorporation is a better marker of growth orientation (Levine and Rubinstein, 2017).

Most studies show different antecedents and characteristics for incorporated relative to unincorporated entrepreneurs: their parents have higher education and incomes, they are more educated,

[3] The literature commonly refers to unincorporated business owners as 'self-employed' and to incorporated business owners as 'entrepreneurs' (Levine and Rubinstein, 2017, 2018).

and score higher on aptitude tests (Åstebro and Tåg, 2017; Levine and Rubinstein, 2017; Tåg et al., 2016). These differences can be substantial, consistent with entrepreneurs being drawn from the tails of the ability distribution (Åstebro et al., 2011; Levine and Rubinstein, 2018). In our data, incorporated business owners have higher cognitive and noncognitive ability, education, and lifetime incomes than unincorporated business owners, have more balanced skill distributions, and enter different industries, supporting the validity of this dichotomy. To enable comparisons with previous literature defining entrepreneurship as self-employment and to follow recent developments, we estimate sibling correlations for both unincorporated and incorporated business owners. For both categories, we create three different outcome variables: (1) entry, i.e. if a person was a business owner in at least one year, (2) persistence, i.e. years as business owner, and (3) income, i.e. income earned as a business owner.

We estimate quite large sibling correlations, suggesting that family background and community influences are important determinants of entrepreneurial outcomes, especially for men and for incorporated business ownership. Background explains between 30% and 45% of the variation in entry, persistence, and income for men and between 15% and 38% for women. These numbers generally exceed the estimated sibling correlations for education and earnings (Björklund and Jäntti, 2012), suggesting that family and community are particularly salient factors for entrepreneurial outcomes. We then explore three questions that arise from these large sibling correlations. First, which background characteristics contribute most to sibling similarity in entrepreneurial outcomes? Second, why are sibling correlations in incorporated business ownership larger than their unincorporated equivalents? Third, why are brother correlations larger than sister correlations, especially for unincorporated firms? To answer these questions, we use detailed data on parental and sibling characteristics (including twin data) to examine the roles played by (i) neighborhood effects, (ii) parental income, education, business ownership, immigration, and family structure, (iii) sibling peer effects, and (iv) shared genes. Finally, for the men who underwent the mandatory military

draft, we explore which shared traits can provide pathways for family influence by analyzing the role of cognitive and non-cognitive ability.

We begin by estimating neighborhood correlations that include corrections for parental sorting into neighborhoods (Page and Solon, 2003a), which can be viewed as upper bounds on neighborhood influences experienced at age 15. Neighborhood effects can explain 3% to 8% of variation in business ownership for men and 3% to 5% for women, suggesting that sibling correlations are driven mainly by family factors and not by shared community influences. However, neighborhoods do matter. Additional analyses yield two new results. First, the neighborhood correlation in unincorporated business ownership for men is large enough to explain most of the observed gender difference in sibling correlations in this outcome. We hypothesize that regional differences in male-dominated occupations, such as farming and construction, which (in Sweden) are typically organized as unincorporated firms, account for the majority of the gender difference in sibling correlations in entry and persistence in unincorporated self-employment. Second, there is a somewhat smaller neighborhood effect related to 'entrepreneurial spirit' or role modeling; that is, part of the neighborhood correlations is explained by the share of business owners in the neighborhood (Giannetti and Simonov, 2009; Guiso et al., 2015).

Comparing the entrepreneurship correlations of different sibling types, we find that shared genes account for up to 50% of the sibling correlations for men and potentially even more for women. In their Swedish twin study, Zhang et al. (2009) find a large genetic component for women, but no genetic component for men, and report a large shared environmental component for men, but no shared component for women in self-employment. Zunino (2016) finds a large genetic component for men, but not for women, and argues that the zero finding for women is most likely due to a female unfriendly entrepreneurial environment in Italy that dampens the expression of their genetic predisposition. In a sample of U.S. twins, Nicolaou and Shane (2010) find no substantive differences in heritability for men and women. In line with the latter, our results based on different

sibling types, and not just twins (Björklund et al., 2005), show equally large genetic and shared environmental components for both men and women.

Parents, however, do more than just pass on their genes to their children. The most important parental characteristic for explaining sibling correlations is parental business ownership. Interestingly, parents' unincorporated business ownership explains a large share of the sibling correlations in unincorporated, but not incorporated business ownership, and vice versa, parents' incorporation only explains sibling correlations in incorporation, hinting towards type-specific entrepreneurial role-models or the transfer of type-specific human capital. An additional share of sibling correlations in incorporated business ownership, i.e. the most capital-intensive form, is explained by parental income, especially for sisters. Parental education, family structure, and immigrant status account for little of sibling correlations in business ownership.

Siblings may also directly influence each other's choices, i.e. there may exist sibling peer effects in business ownership entry. To investigate this, we estimate a correlated random effects model (Altonji et al., 2017), which allows us to put an upper bound on sibling peer effects. For the most part, we find little evidence of such effects; we do, however, find evidence that an older brother's choice of becoming an unincorporated business owner has a positive effect on the probability of his younger brother making the same choice at a later date. This effect can explain up to 8% of the brother correlation in unincorporated entry.

In our final exercise, we examine the role played by similarities in brothers' cognitive and non-cognitive skills. While these scores explain only a small fraction of the sibling correlations in their entry decisions (at most 3%), similarities in cognitive and non-cognitive skills explain 8% of the correlations in their business incomes. This is mainly due to non-cognitive ability: a high leadership score strongly predicts incorporated business ownership entry and income.

The remainder of this paper unfolds as follows. Section 2.2 describes our data and empirical strategy, including a discussion of the analytical relationship between sibling correlations and inter-

generational correlations. We report sibling correlations in Section 2.3 and assess the mechanisms behind sibling similarity in Section 2.4. Section 2.5 summarizes our findings and concludes.

2.2 Data and Methods

2.2.1 Data

We use a 70% sample from Sweden's Multigenerational Register, which includes all persons born from 1932 onwards who have lived in Sweden at any time since 1961.[4] All family ties, both biological and adoptive, are recorded in this register. We define siblings, and hence families, as those sharing the same (adoptive) mother.[5] The Multigenerational Register also provides information on month and year of birth. Having month of birth allows us to accurately identify multiple births, although we do not have information on the zygosity of these children. Other variables taken from this register include gender, year of immigration or emigration, and year of death, as well as parish, municipality, and county of residence from 1968 onwards.

Individuals in our sample can be matched to various official data sources using unique personal identification numbers; we thus have no attrition issues when matching additional variables to our sample. This does not mean, however, that our data are free from measurement error. For example, parental business ownership will be miss-measured in some cases, as some parents in our sample are quite old (or even dead) during the years when our business ownership variables are available. We address these and other measurement issues below.

Our entrepreneurship variables are taken from the Swedish Tax Register. Consistent with the Swedish tax authority, we define individuals as business owners when they derive the majority of their taxable labor income from a business owned in full or in part. We differentiate between two types of business.[6] For the years 1993 to 2012, we know whether a person received the majority

[4] This register is held at Statistics Sweden, together with all the other registers we refer to.

[5] In rows (4)-(6) of Appendix Table A.1.2 we show that our results are robust to alternative family definitions.

[6] Tåg et al. (2016), Åstebro and Tåg (2017), Humphries (2017), and Halvarsson et al. (2018) distinguish between these types of business owners in Sweden; Berglann et al. (2011), and van Praag and Raknerud (2017) do so for Norway, while Hvide and Oyer (2018) focus on newly incorporated firms in Norway.

of their taxable labor income from a partially or fully owned unincorporated or incorporated firm (and possibly employing personnel).[7] In our data, an incorporated business is a privately owned, non-listed, limited liability stock company, subject to minimum capital requirements.[8]

Our extensive margin measures of business ownership, *Unincorporated* and *Incorporated*, are dichotomous variables equal to 1 if the individual is ever categorized as the owner of an unincorporated and, respectively, incorporated firm, and zero otherwise. In any given year, no individual is classified as both *Unincorporated* and *Incorporated*.[9] We use information on business ownership and income between 1993 and 2012 to define our intensive margin outcomes. Specifically, we count the number of years individuals have been business owners, *Years unincorporated* and *Years incorporated*,[10] as well as the income received while a business owner, *Income unincorporated* and *Income incorporated*. Our measure of income represents pre-tax total factor income, including earnings, taxable benefits (e.g. unemployment insurance, parental insurance, sick pay, etc.), and net capital gains (e.g. dividends, interest received or paid, etc.). We average business ownership income across the years individuals were business owners and then take the log of this average.[11]

Given the years for which business ownership data is available, we restrict our sample to

[7] Data on unincorporated business ownership is available from 1985 onwards and is used to calculate parental unincorporated business ownership. We only use data from 1993 onwards for children in order to make the results comparable to those for incorporation; results are robust to using all years (see row (2) of Appendix Table A.1.2).

[8] Many (male dominated) occupations, such as farmers and craftsmen, are over-represented among unincorporated firms, especially in rural areas, whereas incorporated firms are spread across industries more evenly. Statistics Sweden includes farmers in its business owner definition, since farms are run as companies (unincorporated or incorporated). In our sibling sample (yearly data, 1993-2010), the recorded industry for unincorporated firms is agriculture in 12.5% of cases; the equivalent number for incorporated firms is 2.4%. Sibling correlations are robust to excluding families where parents were farmers (see row (12) of Appendix Table A.1.2). Other occupations, such as lawyers, medical doctors, or accountants are represented across all categories, since they may operate as employees of other organizations, as well as unincorporated and incorporated entrepreneurs.

[9] The most likely entry mode is a new ventures, rather than a business take-over (Parker and van Praag, 2012). Moreover, in our data, 21.2% of unincorporated business owners have also been incorporated, and 32.3% of the incorporated have also been unincorporated. Of the 18,867 individuals with both types of experience, 78.7% have first been unincorporated (these spells need not be consecutive and may not capture the same venture). In principle, this is consistent with a conceptual model where individuals first experiment with entrepreneurship on a smaller scale, learn about their potential quality as entrepreneurs, and then decide whether to launch a growth oriented, incorporated firm (Folta et al., 2010; Manso, 2016). While our paper focuses on the differences between types of businesses, we acknowledge that one type (unincorporated) may also lead to the other (incorporation).

[10] Note that these measures do not capture firm survival (considered a poor measure of success, see Arora and Nandkumar, 2011), but individual persistence, in potentially different spells.

[11] Note that current business income may not accurately reflect current entrepreneurial productivity. Some owners may misreport their incomes to avoid paying taxes, while the incorporated may choose to take out lower wages today in order to build equity in their business (Hamilton, 2000; Åstebro and Chen, 2014; Hurst et al., 2014).

siblings born between 1960 and 1970. Thus, we follow the oldest cohort from ages 33 to 52, and the youngest cohort from ages 23 to 42. Those who died or emigrated from Sweden before 1993 are dropped from the sample. These restrictions imply that siblings are born at most 11 years apart and that some individuals have siblings who are not included in our sibling sample.[12]

We have also created a set of family-wide background variables. We define parental unincorporated and incorporated business ownership the same way we do for their children. Our parents, however, are quite old when we observe entrepreneurial outcomes. The median birth year of the mothers and fathers in our sample is 1939 and 1936, respectively. This implies some degree of measurement error due to censoring in our measures of parental entrepreneurship.[13] We address this measurement error in four distinct ways. First, we have data on unincorporated firm ownership starting in 1985. So for parents, we use this longer time span (1985-2012) to define unincorporated business ownership. Second, we focus on the extensive margin measure of parental business ownership (i.e. appearing at least once in our data as business owners). Third, as a robustness check, we 'pool' mothers and fathers to see if either has ever been a business owner and use this as an alternative measure of parental entrepreneurship. Fourth, we split the sample into younger and older parents, to see how this affects our results concerning the associations between parents' and their adult children's entrepreneurial outcomes.[14]

Parental education, taken from the National Education Register and the 1970 Census for some older parents, is measured in seven different levels spanning the old seven-year compulsory level

[12] We impose these restrictions so we can observe siblings' and parents' business ownership for the longest period possible. In Appendix Table A.1.2, row (9) we show similar results for a smaller sample of complete families.

[13] All our regressions include a control for children's birth year, and row (11) of Appendix Table A.1.2 shows that including parental and children's birth year dummies does not affect the sibling correlations. In the accounting exercise, we also include two additional dummy variables for parental death or emigration from Sweden. The first is an indicator if the parent died or emigrated from Sweden before 1985 (which is when our data for self-employment in an unincorporated business begins). The second is an indicator for parental death and emigration between 1985 and 1992 (given that our data for incorporated businesses starts in 1993).

[14] In addition, we extracted survey data on self-employment from the Swedish Level of Living Survey (LNU) for the years 1968, 1974, 1981, and 1991. We chose a representative sample of men and women born in the same years as the parents in our sample. We then calculated the share of these men and women who reported being self-employed in at least one of the four available survey years; 18% of men and 8% of women report being self-employed at least once. These numbers are somewhat lower than the rates that we report in Panel B of Table 2.1. Thus, we may actually have a relatively accurate measure of parental self-employment.

through to graduate school. This indicates the highest degree completed in Sweden, and as such, it is missing for older immigrants who have not attended school in Sweden.[15] We include a dummy for missing parental education in our empirical analysis. Parental income, taken from the Swedish Tax Register, is defined as the log of the average of a parent's pre-tax total factor income for all available years from 1968 to 2012. This is calculated separately for mothers and fathers, then summed. Total factor income captures both labor earnings and returns on capital (financial wealth and/or rental property and/or other rental assets), and is strongly correlated with wealth (Lefgren et al., 2012). In our empirical specifications, we introduce total factor income as a set of dummy variables for deciles of the distribution, as well as the top 5 and top 1 percent. These dummies capture the skewed nature of Sweden's income and wealth distributions, and are thus likely to be significant predictors of business ownership (Hurst and Lusardi, 2004).

Our family structure variable is based on information on parents' actual cohabitation (when the child is 15) from Sweden's Total Population Register. It contains six categories: missing, both parents present, single mother, single father, mother with new husband, father with new wife.[16] Finally, we define neighborhoods as the parish siblings live in at age 15.[17]

For most male Swedish citizens in our sample we have information from their military draft records concerning height, weight, and body mass index (BMI). We also have stanine (scale 1-9) test scores for logical, verbal, spatial, and technical ability, as well as a measure of leadership skills constructed from a structured interview with a psychologist, which was used to help select young men (around the age of 18) into officer training.[18]

[15] In some cases, their education is still included if it was recorded by the immigration authorities.

[16] Other family structure variables, such as i) mother's age at first birth, ii) the mother's partner count (number of children she has conceived children with), iii) father unknown, iv) the presence of both biological and adoptive children in the household, or v) family size, add little explanatory power.

[17] We estimate parish correlations; other neighborhood definitions (schools or statistical metropolitan areas) are unlikely to induce large changes in these correlations (Raaum et al., 2006; Lindahl, 2011). Moreover, in our data, correlations estimated for wider definitions (municipalities and counties) are lower than parish correlations.

[18] Military draft records were provided by the Swedish Recruitment Agency (*Rekryteringsmyndigheten*) and the Swedish War Archives (*Krigsarkivet*). See Lindqvist and Vestman (2011) and Grönqvist et al. (2017) for a complete description of the data and discussions on how these data correlate with various labor market outcomes.

2.2.2 Descriptive Statistics

Our sample consists of 696,231 individuals (356,847 men and 339,384 women) from 430,935 families, and Appendix Table A.1.1 shows the number of families with different sibship sizes. In our sample, nearly 33% of individuals are singletons (i.e. they have no siblings included in the sample – by contrast, we only have 6% 'true' singletons).[19]

Table 2.1 presents descriptive statistics. Panel A shows that 12.8% of the individuals in our sample have been *Unincorporated* at least once, while 8.4% have been *Incorporated* at least once. The average number of *Years unincorporated* and *Years incorporated* are 5.9 and 5.8, respectively. *Income unincorporated* and *Income incorporated* are higher than permanent income (i.e. averaged over the years 1993-2012), and display larger variability (see Panel A in Table 2.2 for measures of permanent income). As expected, income from incorporated businesses is, on average, larger than income from unincorporated businesses.

Descriptive statistics for parents are shown in panel B; 15% of mothers and 24% of fathers have been unincorporated at least once, while 3.1% of mothers and 6.3% of fathers have been incorporated. Mothers and fathers have similar education levels; fathers are slightly more likely to be Swedish natives and have somewhat higher income than mothers. Panel C shows that the average number of children is 2.8 per family, of which we capture 1.6 children per family on average in our sample. The majority of families consists of intact families – almost 70%. Single mothers represent the second most frequent family type (18.7%), followed by mothers with a new husband (5%), single fathers (3.7%), and fathers with a new wife (1.7%). This variable is missing for 1.3% of our sample. Panel D shows that our average parish, out of a total of 2,650 parishes, comprises 259 individuals, while the largest includes 5,286 individuals.[20]

In Panel A of Table 2.2, we examine differences in the observable characteristics of employees

[19] We include singletons to increase the precision of the estimate of between-family variation and odds ratios, although our results are not sensitive to their inclusion or exclusion, see row (1) of Appendix Table A.1.2.

[20] These numbers reflect the size of our sample in each parish, not true parish size. A Swedish parish is roughly similar in size to a U.S. Census tract, with a median parish size of just under 3,000 inhabitants (in 2000).

Table 2.1: **Descriptive Statistics**

	Mean	S.D.	*N*	Min	Max
A. Business ownership outcomes					
Unincorporated	0.128	(0.334)	696,231	0	1
Incorporated	0.084	(0.277)	696,231	0	1
Years unincorporated	5.892	(5.238)	89,061	1	20
Years incorporated	5.785	(4.605)	58,410	1	20
Unincorporated log income	13.703	(1.202)	89,061	0	19.7
Incorporated log income	14.088	(0.969)	58,410	9.1	19.1
B. Parental characteristics [a]					
Mother unincorporated	0.148	(0.355)	430,935	0	1
Father unincorporated	0.243	(0.429)	421,548	0	1
Mother incorporated	0.031	(0.174)	430,935	0	1
Father incorporated	0.063	(0.244)	421,548	0	1
Mother log income	11.603	(0.828)	429,550	0	17.1
Father log income	12.174	(0.670)	418,670	0	17.3
Mother years of schooling	10.023	(2.787)	423,737	7	19
Father years of schooling	9.985	(3.012)	406,914	7	19
Mother immigrant	0.103	(0.304)	430,935	0	1
Father immigrant	0.086	(0.280)	421,548	0	1
C. Demographics					
Male	0.513	(0.499)	696,231	0	1
Twins	0.021	(0.143)	696,231	0	1
Adopted	0.014	(0.119)	696,231	0	1
Family size, total [a]	2.803	(1.306)	430,935	1	18
Family size, in sample [a]	1.616	(0.767)	430,935	1	8
Family structure at age 15 [a]					
Both parents	69.54%		299,657		
Single mother	18.69%		80,548		
Single father	3.74%		16,118		
Mother with new husband	4.98%		21,454		
Father with new wife	1.73%		7,452		
Missing	1.32%		7,506		
D. Neighborhood characteristics					
Parish size	259.365	(475.779)	2,650	1	5,286
Ever unincorporated	0.162	(0.369)	2,650	0	1
Ever incorporated	0.074	(0.262)	2,650	0	1
% Other parents unincorporated	0.307	(0.138)	2,642	0	1
% Other parents incorporated	0.043	(0.036)	2,642	0	0.5

[a] Variables calculated at the family level to avoid overweighting large families.

(labor market participants who have never been business owners), unincorporated, and incorporated business owners. On average, the incorporated have higher lifetime incomes and more education than the other two groups, while the unincorporated have lower incomes and less education.

Table 2.2: **Descriptive Statistics by Business Ownership Status**

	Employee (1)	Uninc. (2)	Inc. (3)	(2)-(1) (4)	(3)-(1) (5)	(3)-(2) (6)
A. Individual characteristics						
Years of schooling	12.309	11.945	12.305	-0.364	-0.004	0.360
	(2.184)	(2.072)	(2.109)	***		***
Log income	11.864	11.731	12.222	-0.133	0.357	0.491
	(0.635)	(0.497)	(0.466)	***	***	***
Business income		13.591	14.070			0.479
		(1.212)	(1.003)			***
Logical ability [a]	4.905	4.752	5.338	-0.152	0.433	0.585
	(1.974)	(1.893)	(1.826)	***	***	***
Verbal ability [a]	4.797	4.672	5.076	-0.126	0.278	0.404
	(1.774)	(1.735)	(1.633)	***	***	***
Spatial ability [a]	5.057	5.076	5.461	0.018	0.404	0.385
	(1.933)	(1.886)	(1.817)		***	***
Technical ability [a]	4.897	4.932	5.356	0.035	0.459	0.424
	(1.897)	(1.814)	(1.767)	***	***	***
Leadership skills [a]	5.243	5.125	5.635	-0.118	0.392	0.510
	(1.502)	(1.527)	(1.422)	***	***	***
Skill dispersion [a]	0.260	0.262	0.233	0.002	-0.027	-0.029
	(0.135)	(0.134)	(0.118)	***	***	***
B. Family characteristics						
Mother unincorporated	0.137	0.230	0.202	0.093	0.065	-0.028
	(0.344)	(0.421)	(0.402)	***	***	***
Father unincorporated	0.231	0.355	0.319	0.123	0.088	-0.035
	(0.422)	(0.478)	(0.466)	***	***	***
Mother incorporated	0.024	0.032	0.120	0.008	0.095	0.087
	(0.154)	(0.177)	(0.325)	***	***	***
Father incorporated	0.053	0.065	0.195	0.012	0.142	0.131
	(0.224)	(0.246)	(0.396)	***	***	***
Mother log income	11.591	11.586	11.736	-0.005	0.145	0.150
	(0.819)	(0.822)	(0.686)		***	***
Father log income	12.166	12.126	12.355	-0.040	0.189	0.229
	(0.649)	(0.722)	(0.635)	***	***	***
Mother schooling	9.996	10.141	10.372	0.144	0.375	0.231
	(2.785)	(2.842)	(2.859)	***	***	***
Father schooling	9.940	10.039	10.341	0.099	0.400	0.301
	(3.031)	(3.067)	(3.120)	***	***	***
Mother immigrant	0.110	0.120	0.087	0.010	-0.023	-0.033
	(0.312)	(0.325)	(0.282)	***	***	***
Father immigrant	0.091	0.101	0.075	0.010	-0.016	-0.026
	(0.288)	(0.301)	(0.263)	***	***	***
Intact family	0.705	0.700	0.772	-0.006	0.066	0.072
	(0.456)	(0.458)	(0.420)	***	***	***

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard deviations in parentheses. Some individuals have been both unincorporated and incorporated at different times; they are omitted here, but the results are similar if they are counted as both *Unincorporated* and *Incorporated*. [a] Available for men only.

For the sample of men for whom military data is available, unincorporated business owners tend to have lower cognitive and non-cognitive ability than employees, with the exception of technical ability, often associated with the manual labor performed by the self-employed (Levine and Rubinstein, 2017). By contrast, the incorporated have higher ability than the other groups, especially with regards to leadership skills. The dispersion of these skills – measured by the intra-individual coefficient of variation (ratio of standard deviation to mean) of cognitive and noncognitive scores – is also lower for incorporated business owners than for either of the other two groups, which are highly similar.[21] This result is in line with the balanced skills or jack-of-all-trades theory of entrepreneurship (Lazear, 2004, 2005), in which individuals with a generalist profile are more likely to become entrepreneurs than specialists due to the broad set of tasks and functions entrepreneurs must perform. However, our result places an important limit on the generalization of this theory: only incorporated business owners benefit from balanced skills, and not all owners, as previously thought (Aldén et al., 2017).

In addition, the incorporated are more likely to enter industries such as the manufacturing of machinery, metal, electrical and optic tools, retail and wholesale trade, financial services, and computer and data services, whereas the *Unincorporated* are found more often in services and agriculture (see Appendix Figure A.1.1). These differences broadly reflect the different financial requirements of an industry (Hurst and Lusardi, 2004), with incorporated business owners more often encountered in capital intensive industries.

Panel B shows that employees and business owners also differ in their background. On average, the parents of the *Incorporated* have higher education and income levels than the parents of the other two groups. The *Unincorporated* are more likely to have parents who owned an unincorporated (but not incorporated) firm than those in the other two groups, whereas incorporated

[21] We compute the coefficient of variation using all non-missing observations; results are similar when we only use individuals with complete ability data. Results are also similar when we measure balance as the intra-individual standard deviation of skills (Aldén et al., 2017). However, we prefer the coefficient of variation as there are significant mean differences between groups.

entrepreneurs are more than three times as likely to have incorporated parents compared with the other two categories. Clearly, incorporated and unincorporated business owners differ in terms of their observable characteristics and family backgrounds, including the type of entrepreneurial experiences they were exposed to as children. These differences are in line with those noted by Levine and Rubinstein (2017), Åstebro and Tåg (2017), Tåg et al. (2016), and Humphries (2017). In addition, the differences between employees and the incorporated are larger than those between employees and the unincorporated, implying that employees and the unincorporated may be more substitutable than employees and the incorporated, which should translate into larger sibling correlations in being incorporated than in being unincorporated.

2.2.3 Methods

Entrepreneurship, E_{if}, for sibling i from family f can be modeled as:

$$E_{if} = X'_{if}\beta + \epsilon_{if}, \tag{2.1}$$

where X'_{if} includes individuals' birth year and a gender dummy for individual i from family f. The residual term, ϵ_{if}, is an individual-specific component representing a person's position in the overall distribution of entrepreneurship, whose population variance is given by σ^2_ϵ. Following Solon (1999), the individual variance component, ϵ_{if}, is assumed to be comprised of two linearly additive and independent variance components:

$$\epsilon_{if} = a_f + b_{if}. \tag{2.2}$$

The first part, a_f, is a permanent component shared by all siblings in family f. This is what makes siblings similar. The second component, b_{if}, is the permanent component unique to sibling i in family f. The variance of ϵ_{if} can be expressed as the sum of the stationary population variances of the permanent family and individual components:

$$\sigma^2_\epsilon = \sigma^2_a + \sigma^2_b. \tag{2.3}$$

The share of the variance in an individual's long-run propensity to choose business ownership over wage employment that can be attributed to family background effects is:

$$\rho = \frac{\sigma_a^2}{\sigma_a^2 + \sigma_b^2} \equiv \text{corr}(\epsilon_{if}, \epsilon_{i'f}). \tag{2.4}$$

This share coincides with the correlation in business ownership of randomly drawn pairs of siblings, which is why ρ is called a sibling correlation. This sibling correlation can be thought of as an omnibus measure of the importance of family and community effects. It includes family-wide influences that are shared by siblings, such as parental entrepreneurship, parental income, parental aspirations, cultural inheritance, genes, etc. However, it also includes shared influences that are not directly experienced in the home, such as school, church, and neighborhood effects. Genetic traits not shared by siblings, differential treatment of siblings, time-dependent changes in neighborhoods, schools, etc., are captured by the individual component b_{if}. If non-shared factors are relatively more important than shared factors for determining business ownership, the variance of family effects will be small relative to the variance of individual effects and the sibling correlation will be low; in other words, the more important the effects of factors that siblings share are, the larger the sibling correlation will be.[22]

An estimate of the sibling correlation in entrepreneurship entry, ρ, can be constructed using estimates of the between-family variation, σ_a^2, and the individual (within-family) variation, σ_b^2. These can be obtained by estimating the following latent linear response model:

$$E_{if}^* = \mathbf{X'}_{if}\beta + a_f + b_{if}, \tag{2.5}$$

[22] The existence of non-shared family factors, such as differential treatment by birth order, gender, or their interaction with family size, implies that sibling correlations should be viewed as lower bounds on the importance of family background and neighborhood effects. Björklund and Jäntti (2012) discuss this issue and examine the size of the advantage of first born children over their younger siblings in cognitive and non-cognitive skills, height, schooling, and earnings; they find only minor effects. Nonetheless, birth order effects could be important for self-employment (Black et al., 2018). Mishkin (2017) finds that in the U.S. the father-daughter association in self-employment is 80% lower if a brother is present. The next chapter in this thesis (working paper circulated as Vladasel, 2018) studies birth order, family size, and sibling sex composition effects in entrepreneurship in Sweden and concludes that they are quantitatively unimportant. In particular, I show that the mechanism described by Mishkin (2017) only applies to paternal unincorporated business ownership, and is much weaker in Sweden. Overall, sibling differences have only a minor effect on our estimates.

where we only observe $E_{if} = I(E^*_{if} > 0)$ (i.e. the dependent variable is dichotomous). We estimate equation (5) using Stata's *xtlogit* command under the assumption that the random effect a_f is a realization from a normal distribution with mean zero and constant variance, while the individual variance component, b_{if}, is drawn from the logistic distribution with mean zero and variance $\pi^2/3$. Stata's *xtlogit* command reports ρ (along with a 95% confidence interval) as part of its standard output. For the continuous intensive margin outcomes, we estimate a similar model using Stata's *mixed* command under the assumption that the two random components are independent realizations from a multivariate normal distribution with mean zero and constant variance. The variance components are estimated using restricted maximum likelihood. These models are estimated only conditional on entry into self-employment.

2.2.4 The Relationship between Sibling and Intergenerational Correlations

Solon's (1999) derivation of the sibling correlation nicely demonstrates the analytical relationship between the intergenerational (e.g. parent-offspring) correlation, which we will call γ, and the sibling correlation, ρ. Let the permanent family component, a_f, be defined as the sum of parental business ownership (times γ), $\gamma\epsilon_f$, and a set of other parental factors orthogonal to ϵ_f, z_f. We then obtain the following relationship:

$$a_f = \gamma\epsilon_f + z_f. \tag{2.6}$$

Taking the variance of both sides of equation (2.6) and dividing through by $\sigma^2_{\epsilon_{if}}$ gives us:

$$\frac{\sigma^2_{a_f}}{\sigma^2_{\epsilon_{if}}} = \rho = \frac{\gamma^2\sigma^2_{\epsilon_f}}{\sigma^2_{\epsilon_{if}}} + \frac{\sigma^2_{z_f}}{\sigma^2_{\epsilon_{if}}}. \tag{2.7}$$

If $\sigma^2_{\epsilon_f} \approxeq \sigma^2_{\epsilon_{if}}$, then we obtain the following relationship:

$$\rho = \gamma^2 + \frac{\sigma^2_{z_f}}{\sigma^2_{\epsilon_{if}}}. \tag{2.8}$$

The sibling correlation equals the intergenerational correlation in business ownership squared plus all parental factors uncorrelated with parental business ownership. In Section 2.4.2 we show

that the total effect of the latter dwarfs the importance of parental entrepreneurship when accounting for sibling correlations. Thus, focusing attention solely on intergenerational correlations results in a narrow measure of the overall importance of family for entrepreneurship.

2.3 Sibling Correlations in Entrepreneurial Outcomes

We report sibling correlations in entrepreneurial outcomes in Table 2.3. Column (1) shows sibling correlations for *Unincorporated*, our extensive margin measure for ever being an unincorporated business owner. The overall sibling correlation is 0.21: in other words, 21% of the total variation in *Unincorporated* is determined by family background and community influences. For brothers, the sibling correlation in *Unincorporated* is 0.29, whereas for sisters it is 0.21.[23] In column (2), the overall sibling correlation for ever being an incorporated business owner, *Incorporated*, is 0.34. For brothers the correlation is 0.40 and for sisters it is 0.35. These numbers suggest that family and community background are even more important for the decision to become *Incorporated* than for becoming *Unincorporated*. For men, 40% of the variation in this outcome is determined by the families and environments they are raised in.

Sibling correlations for our intensive margin outcomes, *Years unincorporated* and *Years incorporated*, are reported in columns (3) and (4). The overall correlations are 0.21 and 0.39, and similar to our extensive margin results. For brothers, background is responsible for 31% and 45% of the variation in persistence in unincorporated or incorporated firm ownership. For sisters, the equivalent numbers are 17% and 38%. In columns (5) and (6), we report sibling correlations in total factor income earned from business ownership, *Income unincorporated* and *Income incorporated*. Brother correlations are 0.30 and 0.42 for unincorporated and incorporated businesses ownership income, respectively, and sister correlations are 0.16 and 0.36.[24]

[23] The overall sibling correlations also include mixed gender sibships, which generally have the lowest sibling correlations. For brevity, we do not report mixed sibship results, except in Table 2.6 and Appendix Table A.1.3.

[24] While the extensive and intensive margin results are similar, one can argue that the latter are conditional on selection into entrepreneurship and may not capture much beyond selection. In an earlier working paper version of this chapter (Lindquist et al., 2017), we used a stricter definition of entrepreneurship, where only individuals who had spent at least the median number of years (4 for unincorporated, 5 for incorporated) as entrepreneurs are defined as such. The results for these outcomes are even stronger. The brother correlation in *Incorporated* $\geq 5y$ is 0.48,

Table 2.3: **Sibling Correlations in Business Ownership**

	Entry		Years		Income	
	Uninc. (1)	Inc. (2)	Uninc. (3)	Inc. (4)	Uninc. (5)	Inc. (6)
A. All children						
ρ	0.212	0.341	0.214	0.386	0.209	0.346
	(0.004)	(0.006)	(0.009)	(0.009)	(0.010)	(0.011)
Individuals	696,231	696,231	89,061	58,410	89,061	58,410
Families	430,935	430,935	80,551	53,157	80,551	53,157
B. Brothers						
ρ	0.292	0.404	0.310	0.447	0.296	0.416
	(0.007)	(0.007)	(0.013)	(0.011)	(0.014)	(0.013)
Individuals	356,847	356,847	55,606	42,650	55,606	42,650
Families	278,107	278,107	51,731	39,592	51,731	39,592
C. Sisters						
ρ	0.213	0.351	0.169	0.381	0.158	0.360
	(0.010)	(0.013)	(0.026)	(0.029)	(0.026)	(0.033)
Individuals	339,384	339,384	33,455	15,760	33,455	15,760
Families	267,894	267,894	31,155	15,274	32,155	15,274

Standard errors in parentheses. Columns (1) and (2) estimate maximum likelihood random effects logistic regressions with entry into entrepreneurship as the outcome variable; columns (3)-(6) estimate restricted maximum likelihood random effects linear regressions with years of and income from business ownership as the outcome variable, conditional on becoming an unincorporated or incorporated business owner.

To get a more complete picture of the importance of background for the number of years spent as a business owner, we create a set of dummies for individuals being unincorporated or incorporated for more than x years, where $x = 1, 2, ..., 10$. Figure 2.1 plots sibling correlations in these outcomes: as before, correlations in being *Incorporated* are higher than those in being *Unincorporated*, and brother correlations are higher than sister correlations. However, confidence intervals for brother and sister correlations in incorporated business ownership largely overlap, and gender differences are most noticeable for unincorporated self-employment. In addition, there is a positive relationship between years of business ownership and the influence of family and community background. At the median number of years as an unincorporated entrepreneur (4

larger than the 0.40 we obtain in Panel B of Table 2.3 (see also Figure 2.1 below, plotting correlations in being an entrepreneur for a given number of years). This suggests that our intensive margin outcomes do capture more than selection, and that family background further explains performance in entrepreneurship.

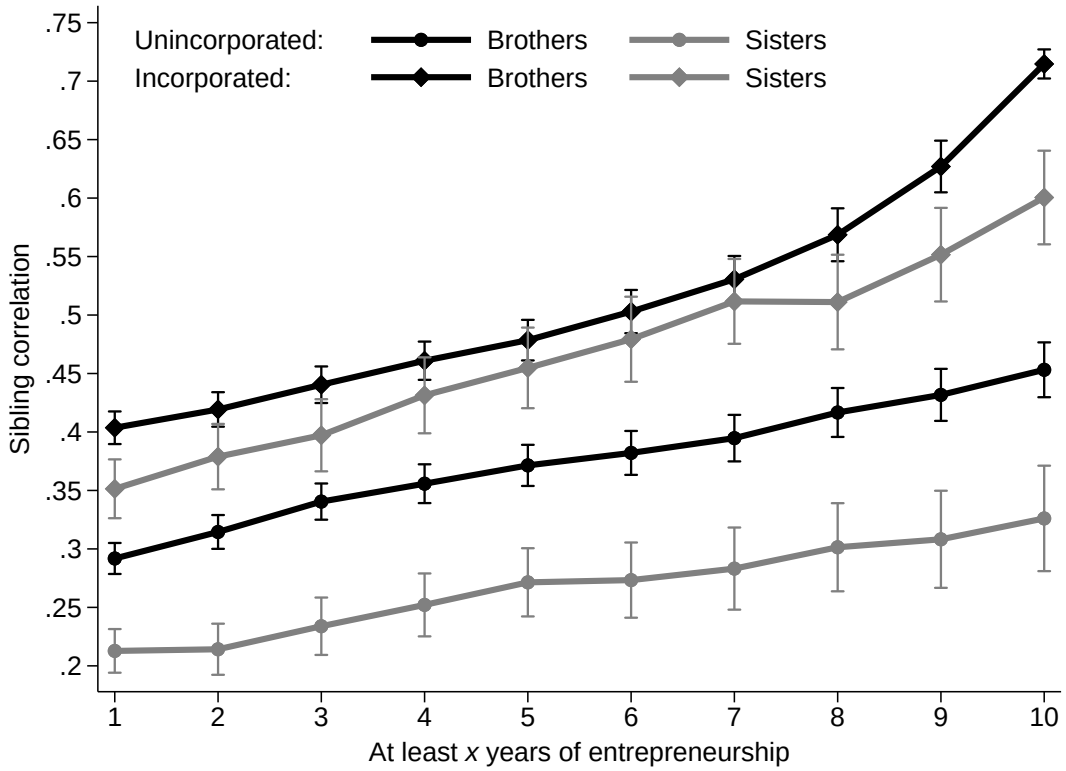

Figure 2.1: Sibling correlations in being a business owner for at least x years, separately by gender and type of business, with 95% confidence intervals.

years), correlations are 0.36 for brothers and 0.26 for sisters; at the median number of years as an incorporated entrepreneur (5 years), correlations are 0.48 for brothers and 0.46 for sisters. Correlations further increase for being a business owner for up to 10 years, where incorporated correlations reach 0.72.

Overall, the sibling correlations in Table 2.3 and Figure 2.1 suggest that family background and community influences are important determinants of entrepreneurial outcomes, especially for men and for incorporated business ownership. Background explains between 30% and 45% of the variation in entry, persistence, and income for men and between 16% and 38% for women. These numbers generally exceed the estimated sibling correlations for education and earnings (Björklund and Jäntti, 2012), as well as cognitive and non-cognitive ability for men (see Section 2.4.5), suggesting that family and community are particularly salient determinants of business ownership outcomes at both the extensive and intensive margin.

In Appendix Table A.1.2, we show that our sibling correlation estimates are robust to a set of different subsamples and outcome definitions. In particular, they are robust to (1) excluding singletons, (2) using data from 1985 onwards for unincorporated outcomes, (3) using data only on individual careers between ages 25 and 40, (4) defining the family through the father, (5) excluding families with an adoptive father, (6) excluding families with an adoptive mother, (7) restricting the sample to non-twin pairs, (8) restricting the sample to closely spaced non-twin pairs (born 12 to 24 months apart), (9) restricting the sample to families captured in their entirety, (10) families for which data on parental characteristics is complete, (11) including individual and parental birth year dummies, (12) excluding families with parents older than 65 in 1993, (13) excluding families with parents older than 65 or who left the sample before 1993, (14) excluding families where parents were farmers, and (15) a placebo test whereby we replicate the original cluster structure of our data and randomly assign individuals to these clusters (to show that sibling correlations pick up more than simple statistical noise).[25]

We also calculate sibling correlations for the different constellations of siblings, namely families with male children only, with female children only, and with mixed gender children. To perform this robustness check, we restrict the sample to sibships captured in their entirety, which row (9) of Appendix Table A.1.2 shows does not affect our main sibling correlations. Appendix Table A.1.3 shows similar patterns: correlations for male-only sibships are larger than in female-only sibships across all outcomes. Correlations in mixed gender sibships are slightly lower, in line with the broader differences between men and women's business ownership.

With these robust sibling correlations in hand, the remainder of the paper focuses on answering the following questions:

[25] These mixed-effects models do not have closed-form solutions and rely on numerical optimization techniques. In additional sensitivity analyses, we experimented with estimation commands using slightly different optimization procedures. For example, we estimated the extensive margin models using the user-written command *gllamm* (Rabe-Hesketh et al., 2005) and found very similar results.

1. What is it that parents give their children that make them so similar in terms of their business ownership outcomes?

2. Why are sibling correlations for incorporated business ownership larger than sibling correlations for unincorporated businesses ownership?

3. Why are brother correlations in unincorporated business ownership larger than the equivalent sister correlations?

To answer these questions, we focus on the extensive margin outcomes throughout the rest of the paper and show results for the intensive margin outcomes in the Appendix. When differences between the extensive and intensive margin results do arise, we highlight them in the main text.

2.4 Accounting for Sibling Similarities

What is it that makes the entrepreneurial outcomes of siblings so similar? In this section, we investigate the extent to which our sibling correlations can be accounted for by (i) neighborhoods, (ii) observable parental characteristics (including parental business ownership), (iii) sibling peer effects, and (iv) shared genes. We also examine one of the potential pathways (children's traits) through which parents may transmit entrepreneurship-relevant human capital: their levels of cognitive and non-cognitive abilities.

2.4.1 Neighborhoods

In his review of the determinants of entrepreneurship, Parker (2009) notes that "[a]ll major economies exhibit regional differences in rates of entrepreneurship" (p. 147). Indeed, Giannetti and Simonov (2009) show that between-municipality variance in Sweden is almost ten times the within-municipality variance in entrepreneurship, and that a standard deviation increase in the proportion of entrepreneurs in the local labor market is associated with 25% more entry into entrepreneurship. In Italy, Guiso et al. (2015) find a positive effect of local firm density in individuals' province of residence at age 18 on entrepreneurial entry. They also show that this density leads

to higher income in entrepreneurship and the adoption of better management practices, which suggests exposure to entrepreneurship when young aids learning.[26]

Our sibling correlations include such neighborhood and community effects. In this section we assess the share of the sibling correlation that can be accounted for by influences experienced outside of the home, but shared by siblings. To do so, we estimate neighborhood correlations in entrepreneurship, using data on the parish individuals resided in at age 15 (the smallest geographical unit we observe). These correlations place an upper bound on the impact of community-wide factors influencing business ownership choices. An estimate of the neighborhood correlation, ρ_n, can be constructed by using estimates of the between-neighborhood variation, σ_n^2, and the individual (within-neighborhood) variation, σ_b^2, which can be obtained by estimating the following latent linear response model for our extensive margin outcomes:

$$E_{in}^* = \mathbf{X'}_{in}\beta + c_n + b_{in}, \tag{2.9}$$

where c_n is a permanent community factor and we only observe $E_{in} = I(E_{in}^* > 0)$. An equivalent model can be estimated for our continuous intensive margin outcomes. The main difference from previously estimated sibling correlations is that we also include a set of parental characteristics in $\mathbf{X'}_{in}$ to correct for parental sorting into neighborhoods (Solon et al., 2000; Raaum et al., 2006). Correcting for sorting provides a tighter upper bound on neighborhood effects on business ownership outcomes. With these new variance components, the neighborhood correlation is:

$$\rho_n = \frac{\sigma_c^2}{\sigma_c^2 + \sigma_b^2}. \tag{2.10}$$

[26] Contemporaneous learning from local entrepreneurs, however, does not seem to play a role (Michelacci and Silva, 2007; Guiso et al., 2015). For example, Giannetti and Simonov (2009) argue that their contemporaneous effect reflects non-pecuniary benefits generated by social status concerns rather than learning, while Dahl and Sorenson (2012) show that entrepreneurs tend to locate close to 'home', exploiting regionally embedded social capital. Moreover, entrepreneurs display less geographical mobility than employees, and this effect is not produced mechanically by involvement in the family firm; these local entrepreneurs exploit advantages in access to finance to build larger, more capital intensive businesses (Michelacci and Silva, 2007). Additionally, Bell et al. (2018) show that exposure to inventors in the community where they grew up is a strong determinant of individuals becoming inventors, with role-modeling (rather than genetic transmission) as the main channel of influence.

Table 2.4: **Neighborhood Correlations in Business Ownership**

	Unincorporated		Incorporated	
	Brothers (1)	Sisters (2)	Brothers (3)	Sisters (4)
A. No controls				
	0.038	0.018	0.020	0.022
	(0.002)	(0.001)	(0.001)	(0.002)
	13.11%	8.40%	5.06%	6.24%
N	352,145	335,173	352,145	335,147
B. Parental controls (excl. entrepreneurship)				
	0.036	0.015	0.016	0.013
	(0.002)	(0.001)	(0.001)	(0.002)
	12.46%	7.04%	3.98%	3.75%
N	352,145	335,173	352,145	335,147
C. Parental controls (incl. entrepreneurship)				
	0.023	0.011	0.011	0.010
	(0.001)	(0.001)	(0.001)	(0.001)
	7.99%	5.21%	2.60%	2.75%
N	352,145	335,173	352,145	335,147
D. All controls (incl. share of entrepreneurs in neighborhood)				
	0.018	0.008	0.006	0.006
	(0.001)	(0.001)	(0.001)	(0.001)
	6.10%	3.81%	1.58%	1.62%
N	347,702	330,799	347,702	330,799
Odds ratios:				
% Other parents uninc.	3.581***	2.023***	1.315***	1.389***
% Other parents inc.	1.953**	10.508***	52.689***	50.824***

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses. Percentages indicate the contribution of the neighborhood correlation to the corresponding sibling correlations in columns (1) and (2) of Table 2.3. Panel D displays the odds ratios of the share of parents of individuals in the same parish who have been unincorporated or incorporated (leaving out the focal individual's parents).

Neighborhood correlations for *Unincorporated* and *Incorporated*, are reported in Table 2.4, separately for brothers and sisters. In panel A, we do not correct for selection into neighborhoods, i.e. we do not control for parental characteristics. The neighborhood correlation in being *Unincorporated* is 0.038 for brothers. This implies that neighborhood effects can account for *at most* 13% of the brother correlation in *Unincorporated* reported in Table 2.3. The neighborhood correlation in being *Unincorporated* is 0.018 for sisters, so neighborhood effects can account for *at most* 8.5% of the sister correlation in *Unincorporated*. Neighborhood correlations in *Incorporated*, which are

0.02 for both brothers and sisters. These correlations can account for *at most* 5-6% of the sibling correlations in *Incorporated* from Table 2.3. Thus, while neighborhoods do help explain business ownership, family influences are an order of magnitude larger.

In panel B, we address parental sorting into neighborhoods by controlling for a large set of parental characteristics such as income and education, but not parental business ownership. As these measures capture both direct and indirect neighborhood effects, we view them as our preferred baseline neighborhood correlations: they are somewhat smaller than those in panel A, especially for *Incorporated*, as parental incomes may explain both neighborhood sorting and children's incorporation outcomes. In panel C, we add parental business ownership. Parents clearly select into and/or are shaped by the local community environment, these control variables lower our estimated neighborhood correlations substantially. We interpret the remaining neighborhood correlations as tight upper bounds on causal neighborhood effects. Such effects account for between 5% and 8% of the *Unincorporated* sibling correlation and around 2.7% of the *Incorporated* sibling correlation. The takeaway of this exercise, so far, is that neighborhood influences do matter, but are a limited source of sibling similarity in business ownership.

We find similar results for our intensive margin outcomes (Appendix Table A.1.4), with one exception. The neighborhood correlation for men in *Years unincorporated* can account for over 18% of the brother correlation in *Years unincorporated* and is large enough to explain most of the observed gender difference in sibling correlations among unincorporated businesses owners. Our hypothesis is that regional differences in male-dominated occupations, such as farming and construction, typically organized as unincorporated firms (in Sweden), account for the lion's share of the gender differences in sibling correlations in unincorporated outcomes.[27] Indeed, Appendix

[27] Our argument mirrors that of Page and Solon (2003a,b), who argue that much of the neighborhood correlation in earnings seen in the U.S. is due to the persistence with which urban born boys (and their brothers) tend to live and work in urban areas as adults. This geographical persistence means that urban boys tend to live in areas with similar economic structures, price levels, and wage levels as adults (see also Løken et al., 2013). Thus, part of the brother correlation, and much of the neighborhood correlation, is generated by this geographical persistence. A related argument is provided by Chetty et al. (2016), who suggest that neighborhoods matter more for boys in the U.S. especially because of regional patterns of poverty and incarceration.

Figure A.1.2 shows that relative to women, unincorporated men are visibly more likely to be found in agriculture, forestry, and construction.

In panel D, we show that the share of business owners in one's parish (other than one's own parents) explains a large share of the residual neighborhood effect. The importance of growing up in a high entrepreneurship density neighborhood is reflected in the odds ratios reported in Table 2.4.[28] Remarkably, the share of adults in the neighborhood who have ever been incorporated business owners matters strongly for both brothers' and sisters' incorporation choices.

We draw three main conclusions from these exercises concerning the role of community wide influences in shaping business ownership. First, it is the family and not the neighborhood that determines the large sibling similarities in entrepreneurial outcomes that we observe in our data. Second, a large share of the gender difference in sibling correlations among unincorporated business owner is likely due to regional differences in the prevalence of male-dominated occupations. Third, a part of the neighborhood effect is related to the mechanisms typically discussed in the literature, such as the existence of a local entrepreneurial spirit and/or role modeling (see, e.g., Giannetti and Simonov, 2009; Guiso et al., 2015). While this effect does not explain much of our sibling similarities, the large odds ratios at the bottom of Table 2.4 indicate that this effect may be quite important for explaining the individual-level idiosyncratic part of the choice to become a business owner, particularly for incorporation.

2.4.2 Parental Characteristics

Which parental characteristics are mainly responsible for generating sibling similarities in business ownership? We study this question by including a set of family-wide variables suggested by the literature, either one at a time or simultaneously, as control variables in our logistic regressions. For example, consider the inclusion of mothers' and fathers' entrepreneurship in $\mathbf{X'}_{if}$. These two additional variables should reduce the residual variation in the outcome variable and produce

[28] For example, girls in a parish where all observed adults (other than their parents) have been incorporated are 50.8 times more likely to become incorporated than girls in a parish where no other parent has been incorporated.

a lower estimate of the between-family variation, σ_a^{2*}, than the estimate produced without the added controls. We can interpret the difference between these two estimates, $\sigma_a^2 - \sigma_a^{2*}$, as an upper bound on the amount of the variance in the family component that can be explained by parental entrepreneurship. It is viewed as an upper bound since it includes other factors affecting children's entrepreneurship that are correlated with parental entrepreneurship (for instance, education, occupation, or residence). This exercise also produces a new sibling correlation, ρ^*. From what we know about the relationship between parents' and children's entrepreneurship (Lindquist et al., 2015), we expect this new sibling correlation to be substantially lower.

The degree to which any particular control variable lowers the sibling correlation after being included provides a metric for judging its importance in explaining sibling similarities (Mazumder, 2008; Björklund et al., 2010) and the upper bound on its explanatory power, but does not allow for a strictly causal interpretation.[29] Specifically, we explore the potential roles played by parental (i) education, (ii) income, (iii) business ownership, and (iv) immigration status and family structure. We also report the odds ratios and coefficients associated with each of these control variables.[30] Our accounting exercise allows us to set an upper bound on different elements of family and community background above and beyond parental business ownership. We can thus gauge the relative importance of background characteristics, separately by gender, type of business, and along the extensive and intensive margins.

Previous research has suggested an important role of parental human capital and financial resources (Carroll and Mosakowski, 1987; Lentz and Laband, 1990; Aldrich et al., 1998; Blanchflower and Oswald, 1998; Dunn and Holtz-Eakin, 2000; Aldrich and Kim, 2007; Fairlie and Robb, 2007a;

[29] In addition, one could be concerned with a mechanical decrease in the sibling correlation as controls are added, similar to the mechanical increase in R^2. To ensure this is not the case, we generated a set of (20 or 100) noisy random variables at both the individual level and the family level, and included them as controls. Appendix Table A.1.5 shows that the change is not mechanical: these random variables increase (decrease) the sibling correlations by at most 1.59% (0.72%), and often explain nothing at all.

[30] Table 2.5 reports the results for *Unincorporated* and *Incorporated* business ownership, together with odds ratios. In Appendix Table A.1.6 we report the explanatory power of these variables for both the extensive and intensive margin outcomes, as well as their joint contribution to the sibling correlation (panel D).

Hvide and Oyer, 2018) for entrepreneurship entry and success.[31] Parental education often serves as a proxy for the transfer of general human capital, while a large role for parental income and wealth would be consistent with the existence of capital constraints (Holtz-Eakin et al., 1994a,b; Blanchflower and Oswald, 1998). While these human and financial capital transfers matter, it is unclear what their magnitude is in explaining entrepreneurial outcomes, as well as with regards to gender and firm type (unincorporated or incorporated).

Substantial attention has been devoted to parental entrepreneurship as the most likely influence on individuals' occupational choices. The mechanisms underlying the influence of parental entrepreneurship include shared genes, role-modeling, and the acquisition of general or specific business human or social capital (Lentz and Laband, 1990; Dunn and Holtz-Eakin, 2000; Fairlie and Robb, 2007b; Sørensen, 2007b; Colombier and Masclet, 2008; Laspita et al., 2012; Hoffmann et al., 2015; Lindquist et al., 2015), business inheritance, or occupational following.[32]

Ethnicity and parental immigration are likely to play a role in entrepreneurship decisions, in terms of the location of new immigrants and their subsequent choice of business (Dunn and Holtz-Eakin, 2000; Edin et al., 2003; Andersson and Hammarstedt, 2010, 2011; Kerr and Mandorff, 2015). Finally, although family structure is potentially associated with personality developments affecting entrepreneurial decisions, it has been understudied as a determinant of entrepreneurship, mainly given a lack of reliable data. Previous studies reveal a limited association of family structure with entrepreneurship or entrepreneurial values (De Wit and Van Winden, 1989; Dunn and Holtz-Eakin,

[31] Edelman et al. (2016) highlight the importance of parental social capital; unfortunately, we do not have data on social capital, although our neighborhood variable will capture such influences to a certain extent.

[32] Business inheritance is unlikely to play a large role. Using similar data to ours, Lindquist et al. (2015) find that only 2.2% of entrepreneurs in Sweden enter for the first time in the same industry and the same year as their parent exits entrepreneurship. Once they include offspring that become an entrepreneur one year before or after their parents' exit, the number rises to 4.4%. Sørensen (2007b) finds that almost 8% of children's entries into self-employment in Denmark occur at the same time and in the same industry as their parents' industry, while Dahl and Sorenson (2012) note that less than 5% of entrepreneurs (with at least 1 employee) enter an industry where their parents have experience. Other studies also show that a low share of children actually take over their parents' company, usually between 5.5 and 14% for U.S. and Canada (Lentz and Laband, 1990; Aldrich et al., 1998; Fairlie and Robb, 2007a). Note also that succession is less straightforward in the multiple-child families at the core of our analysis, since not all children inherit the family firm. In our data, siblings are observed as incorporated business owners at the same time and in the same industry in at most 3% of years, suggesting that co-ownership by siblings is limited, as is the contribution of business inheritance to sibling correlations.

Table 2.5: **Accounting Exercise: Parental Characteristics**

	Unincorporated		Incorporated	
	Brothers (1)	Sisters (2)	Brothers (3)	Sisters (4)
A. Parental education				
Mother's education level:				
9 years	1.074***	1.190***	1.344***	1.278***
11 years	1.077***	1.098***	1.241***	1.118***
12 years	1.195***	1.410***	1.591***	1.507***
14 years	1.545***	1.283***	1.536***	1.349***
15.5 years	1.205***	1.423***	1.555***	1.409***
19 years	1.579***	1.548***	1.658***	1.404***
Father's education level:				
9 years	1.087***	1.176***	1.203***	1.374***
11 years	1.050***	1.115***	1.044**	1.127***
12 years	0.865***	1.200***	1.284***	1.429***
14 years	0.842***	1.186***	1.121***	1.417***
15.5 years	0.838***	1.390***	1.143***	1.483***
19 years	0.834***	1.490***	1.133**	1.606***
$\rho*$	0.289	0.205	0.399	0.341
	(0.007)	(0.010)	(0.007)	(0.013)
	0.86%	3.66%	1.13%	2.98%
B. Parental income				
Pct. 10-20	0.890***	0.996	1.180***	1.190***
Pct. 20-30	0.752***	0.928**	1.322***	1.191***
Pct. 30-40	0.700***	0.929**	1.389***	1.399***
Pct. 40-50	0.635***	0.930**	1.488***	1.340***
Pct. 50-60	0.643***	0.895***	1.637***	1.441***
Pct. 60-70	0.632***	0.915***	1.809***	1.548***
Pct. 70-80	0.647***	1.024	2.182***	1.880***
Pct. 80-90	0.660***	1.180***	2.718***	2.388***
Pct. 90-95	0.662***	1.409***	3.219***	3.022***
Pct. 95-99	0.689***	1.663***	4.049***	4.232***
Pct. 99-100	0.770***	2.128***	6.354***	7.219***
$\rho*$	0.288	0.206	0.382	0.321
	(0.007)	(0.010)	(0.007)	(0.013)
	1.47%	3.14%	5.41%	8.67%

continued

2000; Hout and Rosen, 2000; Halaby, 2003; Hundley, 2006; Tervo, 2006), although the incorporated in Levine and Rubinstein (2017) are more likely to come from a two-parent family. Controlling for these observables one by one and then jointly, we can assess both their relative and their total contribution to variance in business ownership.

Table 5 (cont'd): **Accounting Exercise: Parental Characteristics**

	Unincorporated		Incorporated	
	Brothers (1)	Sisters (2)	Brothers (3)	Sisters (4)
C. Parental business ownership				
Mother unincorporated	1.643***	1.589***	1.286***	1.282***
Father unincorporated	1.963***	1.356***	1.342***	1.196***
Mother incorporated	0.911***	1.294***	2.837***	3.507***
Father incorporated	0.933***	1.231***	3.854***	2.032***
$\rho*$	0.267	0.199	0.348	0.309
	(0.007)	(0.010)	(0.008)	(0.014)
	8.66%	6.37%	13.75%	12.15%
D. Other family characteristics				
Mother immigrant	1.088***	1.010	0.845***	0.913**
Father immigrant	1.153***	1.044*	0.970***	0.945
Family structure:				
Single mother	1.046***	0.990	0.746***	0.763***
Single father	1.136***	1.102***	0.919***	0.882**
Mother, new husband	1.052*	1.044	0.784***	0.866***
Father, new wife	1.127***	1.031	0.587***	0.672***
$\rho*$	0.290	0.213	0.399	0.348
	(0.007)	(0.010)	(0.007)	(0.013)
	0.56%	0.05%	1.18%	1.06%
E. All family characteristics				
$\rho*$	0.257	0.190	0.334	0.287
	(0.007)	(0.010)	(0.008)	(0.014)
	11.63%	10.75%	17.23%	18.20%
Individuals	356,847	339,384	356,847	339,384
Families	278,107	267,894	278,107	267,894

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses. Percentages indicate the contribution of parental characteristics to the corresponding sibling correlations in columns (1) and (2) of Table 2.3. For parental education, the reference category is 7 years; for parental income, the reference category is the bottom decile of parental income distribution; for family structure, the reference category is the intact family. The models include dummies for missing parental education, income, business ownership, immigrant status, and family structure, when assessing the explanatory effect of that particular family variable. In Panel C, we also include dummy variables for the mother and father leaving the sample before 1985 (when data on unincorporated business ownership becomes available) and before 1993 (when data on incorporated business ownership becomes available).

In panel A of Table 2.5, parents' education explains a limited share of variance in business ownership. Odds ratios range from 0.83 to 1.66, and parental education lowers brother correlations in *Unincorporated* and *Incorporated* by (at most) 1.1%, and sister correlations by 3%. Despite strong intergenerational correlations in education in Sweden (Holmlund et al., 2011), the transfer

of general human capital from parents to sons does not explain why siblings are so similar in terms of their entrepreneurial outcomes; for daughters, higher education may allow them to overcome potential entry barriers, making parental education a more important determinant of business ownership. At the intensive margin, Panel A of Appendix Table A.1.6 shows that parental education explains between 3.6% and 9.2% of correlations in business income. This is consistent with an intergenerational correlation in ability, which allows children of more educated (higher ability) parents to perform better as business owners.

We control for parental income (the sum of mother's and father's total factor income) through a set of dummies for deciles of the income distribution, as well as the top 5 and 1 percent, in Panel B. High parental income lowers the odds of being *Unincorporated* for men but raises the odds for women, explaining 1.5% (3.1%) of the brother (sister) correlation. Parental income consistently raises the odds of being *Incorporated* for both brothers and sisters. This relationship is more pronounced at the top of the distribution (Hurst and Lusardi, 2004), with parental incomes in the top 1 percent increasing the odds of becoming *Incorporated* 6.4 times for brothers and 7.2 times for sisters. Our measure of parental financial resources accounts for 5.4% (8.7%) of the brother (sister) correlation in *Incorporated*, respectively. Thus, parental resources do matter, especially with regards to incorporation and sisters, as suggested by liquidity constraints models (Evans and Leighton, 1989; Blanchflower and Oswald, 1998; Dunn and Holtz-Eakin, 2000; Levine and Rubinstein, 2018). In Panel B of Appendix Table A.1.6, we find that while parental financial resources explain a small share of correlations in *Years Unincorporated* and *Years incorporated*, they do have a strong explanatory power (12%-15.9%) for *Income unincorporated*. Parental income also matters for *Income incorporated*, although the magnitude is smaller (around 5%), possibly because family resources are used mainly as collateral when the entry decision is made.[33]

[33] We also calculated parental income for the years 1968-1980, when most individuals in our sample are in their teens. While this variable is strongly correlated with parental lifetime income ($\rho = 0.61$), its contribution to explaining incorporated entrepreneurship is much smaller (i.e. 1.57% and 4.29%, relative to 5.14% and 8.67% in Panel B of Table 2.5 for men, and respectively, women). Parental income appears at least as important for potential investment in children's firms as in their human capital, in line with a capital constraints interpretation.

Following the large literature on intergenerational associations in entrepreneurship, we expect parental business ownership to be a strong predictor of individual business ownership and to explain a large share of sibling correlations. Indeed, Panel C of Table 2.5 suggests that having entrepreneurial parents raises the likelihood of individual business ownership. Moreover, same-sex associations are stronger, as are those for becoming *Incorporated* (consistent with Lindquist et al., 2015). This pattern is reflected in the contribution to sibling correlations: parental firm ownership explains 6.4%-8.7% of *Unincorporated* and around 13% of *Incorporated*. On the one hand, parental business ownership appears to be the strongest driver of sibling correlation, given that its explanatory power is larger than that of other family (and neighborhood) background factors; on the other hand, focusing attention solely on parental business ownership leads to an overly narrow approach, which leaves out a wide array of family background factors that impact entrepreneurship. Parental business ownership is less important when we look at the intensive margin outcomes in Panel C of Appendix Table A.1.6, and for income, it is no longer the most important parental variable to explain sibling correlations.[34]

Parental immigration and family structure have little bearing on the extensive margin sibling correlations, as shown in Panel D of Table 2.5, jointly explaining at most 1%. Inspection of the odds ratios suggests a concentration of immigrant entrepreneurship in unincorporated self-employment and that parental cohabitation reduces the odds of being *Unincorporated* while it increases those of being *Incorporated* (Levine and Rubinstein, 2017).

[34] To address potential measurement error in parental entrepreneurship, we perform two robustness checks. First, we split families into those with mothers born before and after 1939 (the median maternal birth year), with measurement error less problematic for the latter. Sibling correlations vary little between these groups, but parental entrepreneurship has a stronger explanatory power for incorporation. For instance, compared to an explained 13.75% of the brother correlation in *Incorporated* in Panel C of Table 2.5, in families with older and younger mothers the equivalent numbers are 11.13% and 16.09%. Our original estimates may thus understate the contribution of parental business ownership to sibling correlations in incorporation in older families; this effect is weaker for unincorporated outcomes, as we observe parental unincorporated firm ownership at an earlier date (i.e. since 1985). We obtain similar results if we split families by the father's birth year. Second, we replicate the exercise in Panel C of Table 2.5 by 'pooling' maternal and paternal business ownership. In this case, the explanatory power of parental business ownership is always lower, in particular for the intensive margin outcomes; indeed, combining unincorporated and incorporated firms results in even less explanatory power. The disaggregation of parental entrepreneurship by parent and type of firm appears particularly important for understanding the intergenerational transmission of occupational choice (outweighing the reduction in measurement error). These results are available upon request.

Finally, in panel E of Table 2.5 (and Appendix Table A.1.6), we estimate brother and sister correlations controlling for all the variables above. Their joint contribution to sibling correlations is 11% for *Unincorporated* and 18% for *Incorporated*, slightly smaller for *Years (un)incorporated*, and slightly larger for *Income (un)incorporated*.[35] As we noted earlier, parental characteristics (such as income and education) may be correlated, such that their individual explanatory power is an upper bound. This is confirmed by the joint explanatory power of all parental characteristics, which is slightly lower than the sum of individual contributions, especially for incorporation.

To sum up: parental education, family structure, and immigrant status account for minor shares of sibling correlations in business ownership; unincorporated parents explain a large share of sibling correlations in unincorporated business ownership (but not incorporation); parental incorporation explains a large share of sibling correlations in incorporation (but not in unincorporated firm ownership); parental business ownership matters most for entry and persistence rather than income; and finally, parental financial resources explain a larger share of correlations in incorporation, especially for sisters, and are most important in explaining business income.

2.4.3 Sibling Peer Effects

Sibling correlations also capture inter-sibling interactions; while these could be treated as a nuisance in estimating the impact of family background, we consider such sibling peer effects to be an integral part of shared environments. The entrepreneurship literature has convincingly identified peer effects within the workplace (Nanda and Sørensen, 2010) and universities (Lerner and Malmendier, 2013; Kacperczyk, 2013), based on the quasi-random assignment of employees to workplaces or

[35] The intergenerational association in business ownership may partly reflect occupational or industry following (Aina and Nicoletti, 2018; Hvide and Oyer, 2018); for instance, farmers are typically self-employed, and children who choose to become farmers will likely also be self-employed (especially for men). To address this, in the absence of detailed occupation data, we include the modal maternal and paternal industries in our accounting exercise (though these variables may also exhibit substantial measurement error). We find that they explain between 2.6% and 10.8%, with higher numbers for brothers and unincorporated business ownership. This is consistent with our argument regarding neighborhood effects and the regional concentration of male-dominated occupations often associated with self-employment. When we include parental industry in Panel E of Table 2.5, observable parental characteristics account for 12%-13% (18.5%-20.4%) of brother and sister correlations in becoming *Unincorporated* (*Incorporated*). It appears, therefore, that parental industry experience matters for sibling similarity in business ownership, both by itself and through its association with firm type.

students to classes. In addition, role-modeling may underpin intergenerational associations in entrepreneurship (Sørensen, 2007b; Lindquist et al., 2015; Hoffmann et al., 2015).

Here, we assess the potential role of sibling peer effects in generating sibling correlations. We first examine sibling correlations at different birth spacings based on month of birth data, from twins (zero spacing), through siblings born at least 12 months apart in rolling intervals of 12 months, and to sibling spacings of 108 months.[36] There are two competing expectations about sibling peer effects based on the relationship between spacing and sibling correlations (Eriksson et al., 2016). On the one hand, siblings born closer together interact more intensively and may share a more similar family environment while growing up, which should lead to higher sibling correlations at low birth spacings. On the other hand, much older siblings may act as stronger role models. Thus, depending on the relative strength and non-linearities of each effect, sibling correlations may increase or decrease with sibling spacing, may be non-linear, or even zero.

Results for unincorporated and incorporated outcomes in Figure 2.2 suggest that while twin correlations are higher than non-twin correlations, beyond twins there is no an evident relationship with birth spacing. This pattern is common across outcomes and gender, as Appendix Figure A.1.3 shows. This result is notable, given that in the bulk of the sibling correlation literature, the outcomes of closely spaced siblings are typically much more similar than those of widely spaced siblings (see, e.g., Eriksson et al., 2016). Before drawing any conclusion, we turn to a more systematic peer effects exercise.

While we lack a formal randomization process, we gain information on spillovers by exploiting differences in the timing of business ownership for sibling pairs. A method for exploring causal peer effects has been proposed by Altonji et al. (2017), who study illegal substance abuse, and has also been applied by Eriksson et al. (2016) to criminal activity. The method relies on the

[36] We omit spacings between 1 and 11 months, and larger than 108 months as these are quite rare. Labels in Figure 2.2 imply 12-month rolling intervals, i.e. the label 12 months covers spacings between 12 and 24 months. In addition, we restrict the non-twins to full siblings in families with 2 children in our sample. Sibling correlations for this sample, reported in row (7) of Appendix Table A.1.2 are the same as the baseline sibling correlations.

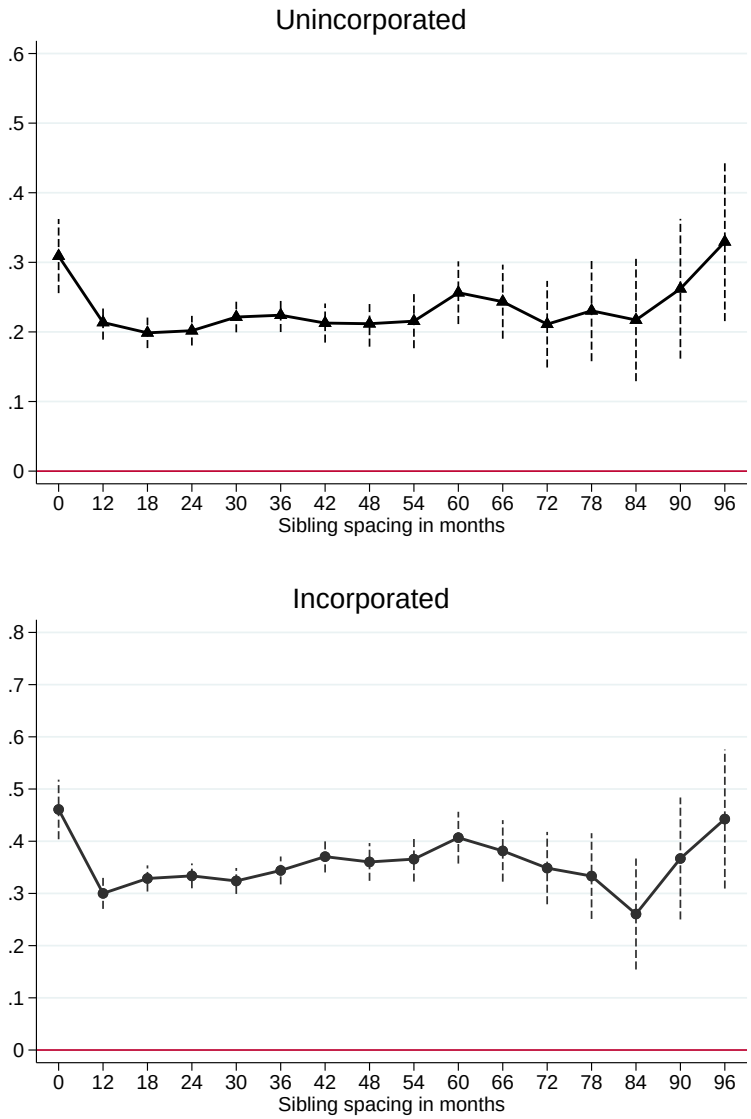

Figure 2.2: Twin and sibling correlations in business ownership by sibling spacing and type of entrepreneurship, with 95% confidence intervals.

relatively strong assumptions that only older siblings can influence younger siblings and that parental influences are not a mediating channel. While their method is intuitively applicable to situations where peer effects are likely to dominate other causes and where individuals are actively involved (in the outcome under study) when young, business ownership decisions are usually made after the individual has left the household. In addition, it is not clear that older siblings necessarily engage in business ownership earlier than younger siblings.[37]

[37] However, older siblings enter the labor market earlier, and are statistically more likely to become business owners before the younger siblings, especially at large birth spacings.

Table 2.6: **Upper Bounds on Peer Effects Contribution to Sibling Correlations**

	Effect on younger sibling		Effect on older sibling	
	(1)	(2)	(3)	(4)
A. Unincorporated				
All sibling types	5.11	5.05	-2.07	-3.71
Males	7.24**	7.82**	-1.19	-1.97
Females	10.29	9.78	4.04	-0.55
Mixed (younger brother)	-9.99	-8.36	-2.68	-3.70
Mixed (younger sister)	0.72	-1.84	-12.02	-14.52
B. Incorporated				
All sibling types	-2.78	-1.37	2.12	2.85*
Males	-0.61	0.86	0.20	1.45
Females	-8.93*	-7.12	1.61	1.92
Mixed (younger brother)	-5.86	-4.70	7.51	7.73
Mixed (younger sister)	-6.15	-4.36	7.76	7.33
Contemporaneous effect	No	Yes	No	Yes

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All numbers are in percentages, representing the share of the sibling correlation explained by the lagged entrepreneurship status of the older sibling, columns (1) and (2), and the younger sibling, columns (3) and (4), once controls are added and correlated random effects are accounted for. For the full set of results, see Appendix Tables A.2.1-A.2.5; the results in this table are based on columns (3), (4), (8) and (9) in those tables. Note that applying a Bonferroni correction for testing multiple hypotheses (i.e. given that we estimate 40 different models, requiring a p-value below $0.00125 = 0.05/40$) would render all estimates insignificant.

Our exercise focuses on explaining the variance of business ownership outcomes due to the influence of sibling peers rather than on identifying causal effects. We thus take an agnostic approach to applying the Altonji et al. (2017) model. Focusing on the subsample of sibling pairs, we estimate both the effect of the older sibling on the younger one, and of the younger sibling on the older one, subsequently converting the results into correlations to assess the potential contribution of peer effects to the sibling correlation (Bonett, 2007). A more detailed description of our empirical strategy is provided in Appendix A.2, together with a full set of results.

Table 2.6 summarizes the results, with panel A referring to being unincorporated and panel B to being incorporated. Column (1) shows how much the impact of the older sibling's business ownership at time $t-1$ on the younger sibling's business ownership at time t contributes (at most) to the sibling correlation, and column (2) does so while controlling for contemporaneous effects. Columns (3) and (4) do the same for the impact of the younger sibling on the older sibling.

The lagged effect of the older sibling's unincorporated status on the younger sibling represents at most 5% of the sibling correlation. Conversely, the effect of the younger sibling on the older one appears largely negative, implying that peer effects may generate sibling dissimilarities. However, only brother peer effects are significant, explaining 8% of the sibling correlation. For other sibling types and incorporation, most peer effects are not significant, further explaining why the brother correlations for unincorporated outcomes are larger than sister outcomes.

The results from our sibling peer effects analyses paint the following picture. The absence of a negative relationship between sibling spacing and sibling correlations suggests that i) time-varying, family-wide factors do not appear to be important, and ii) close (day-to-day) interactions between siblings may not be important. The largely non-significant peer effects estimated in our formal exercise imply that the lack of a negative relationship between spacing and sibling correlation may substantiate the claim of limited sibling peer effects we made based on the first exercise, rather than the alternative explanation of two potentially opposing effects. A notable exception are brothers, who experience sibling peer effects in unincorporated business ownership.

2.4.4 Shared Genes

Several studies have shown that entrepreneurship is influenced by genes (Nicolaou et al., 2008; Zhang et al., 2009; Nicolaou and Shane, 2010; Lindquist et al., 2015; Zunino, 2016; Nicolaou et al., 2017). Since most siblings share part of their genetic endowment, shared genes may be an important contributor to sibling correlations. In this section, we assess how much of sibling correlations in business ownership can be attributed to genes shared by siblings. We begin by positing an additive model of genetic and environmental influences (see, e.g., Björklund et al., 2005). Business ownership, E, is due to a genetic factor, G, a shared environmental influence, S, and an unshared, idiosyncratic environmental influence, U:

$$E = gG + sS + uU, \tag{2.11}$$

where g, s, and u are model parameters representing the relative influence of each of these three factors. Given this model, our sibling correlation, $\rho = corr(E, E')$ is equal to $g^2 + s^2$, i.e. the share of the total variation in our entrepreneurial outcomes that can be attributed to shared environmental and genetic factors.

In our data, we have information on four different sibling types with different degrees of genetic relatedness: twins, full siblings, half siblings, and adopted siblings. Assuming that twins share on average 75% of their genes – since we pool monozygotic (MZ) and dizygotic (DZ) twins –, the twin correlation, ρ_{twin}, equals $0.75 * g^2 + s^2$. We assume that the correct models for full, half and adopted siblings are $\rho_{full} = 0.5 * g^2 + s^2$, $\rho_{half} = 0.25 * g^2 + s^2$, and $\rho_{adopted} = s^2$, respectively. We also assume that the correlation in shared environmental factors, $corr(S, S')$, is equal to one for all sibling types.

We use a method of moments estimator to produce estimates of $\hat{g}^2$ and $\hat{s}^2$. This estimator chooses parameter estimates in order to match the four different sibling correlations observed in our data as best as possible (Björklund et al., 2005). We use weights to increase the precision of our estimates, where the weights are simply the sample size, N, that each sibling correlation is based on. This means that our estimator will work much harder to fit the model to the observed sibling correlation for full siblings than it will for the other three types of siblings.[38]

Table 2.7 reports the results. The estimated sibling correlations from our data, $\hat{\rho}$, the standard errors of these estimated correlation, and the sample size, N, are shown in the first two columns. The key estimated parameters $\hat{g}^2$ and $\hat{s}^2$ (and their standard errors) are reported in the top rows of each panel. The model based predictions for g^2 and s^2 are shown in columns (4) and (5), respectively. The model based prediction of the sibling correlation, ρ, for each sibling type is

[38] Conceptually, this approach is not very different from trying to fit a line through the different sibling correlations, ordered from pairs that share the least genetic material (adoptees, 0%) to pairs that share the most genetic material (MZ and DZ twins, 75% on average). This (weighted) least squares approach provides an estimate of the increase in sibling correlations due to a 1% increase in shared genes, which we can use to assess the importance of genetic effects for sibling correlations. We performed this exercise in our earlier working paper (Lindquist et al., 2017), with very similar results to the ones we present here.

Table 2.7: **Shared Genes and Sibling Correlations at the Extensive Margin**

			Model				Average
	N	$\hat{\rho}$	ρ	g^2	s^2	g^2/ρ	g^2/ρ
Sibling type	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Brothers, unincorporated				0.293	0.153		**47%**
				(0.134)	(0.073)		
MZ+DZ twins	4,416	0.363	0.373	0.220	0.153	59%	
		(0.043)					
Full siblings	37,772	0.306	0.300	0.147	0.153	49%	
		(0.014)					
Half siblings	5,036	0.157	0.227	0.073	0.153	32%	
		(0.041)					
Adopted siblings	1,010	0.304	0.153	0.000	0.153	0%	
		(0.087)					
B. Sisters, unincorporated				0.340	0.058		**72%**
				(0.147)	(0.069)		
MZ+DZ twins	4,438	0.399	0.312	0.255	0.058	82%	
		(0.048)					
Full siblings	33,288	0.206	0.227	0.170	0.058	75%	
		(0.020)					
Half siblings	4,746	0.201	0.143	0.085	0.058	60%	
		(0.056)					
Adopted siblings	1,126	0.105	0.058	0.000	0.058	0%	
		(0.118)					
C. Brothers, incorporated				0.391	0.196		**48%**
				(0.064)	(0.030)		
MZ+DZ twins	4,416	0.528	0.490	0.294	0.196	60%	
		(0.039)					
Full siblings	37,772	0.384	0.392	0.196	0.196	50%	
		(0.015)					
Half siblings	5,036	0.317	0.294	0.098	0.196	33%	
		(0.049)					
Adopted siblings	1,010	0.223	0.196	0.000	0.196	0%	
		(0.107)					
D. Sisters, incorporated				0.413	0.137		**58%**
				(0.195)	(0.107)		
MZ+DZ twins	4,438	0.429	0.447	0.309	0.137	69%	
		(0.072)					
Full siblings	33,288	0.355	0.344	0.206	0.137	60%	
		(0.026)					
Half siblings	4,746	0.130	0.240	0.103	0.137	43%	
		(0.122)					
Adopted siblings	1,126	0.335	0.137	0.000	0.137	0%	
		(0.142)					

Standard errors in parentheses. GMM estimation of genetic effects using different types of siblings (Björklund et al., 2005). The last column shows the weighted average contribution of shared genes.

reported in column (3). In general, the model estimates are much more precise for brothers than for sisters. The model for incorporated brothers (in Panel C), for example, fits the data surprisingly well, despite being based on a number of strong, simplifying assumptions.[39]

In column (6), we report the percentage of the model sibling correlation ρ due to shared genes, g^2. In Panel A, for example, this ranges from 0% for adopted brothers (by construction) to 59% for twin brothers. In column (7) of each panel we report the weighted average percent of the sibling correlation attributable to shared genes across all possible sibling pairs. For unincorporated brothers the share is 47%; for incorporated brothers the share is 48%. Thus, up to half of the brother correlation in business ownership along the extensive margin can be attributed to shared genes. For sisters, the share due to common genes is somewhat larger (up to 72% and 58% for unincorporated and incorporated business ownership, respectively), since sister correlations are smaller and our estimates of g^2 are roughly similar for men and women (though slightly noisier for the latter). Our results showing substantial genetic effects for both men and women are thus in line with those obtained by Nicolaou and Shane (2010) for U.S. twins.

2.4.5 Cognitive and Non-Cognitive Abilities

In the previous subsection, we argued that children inherit traits from their parents that are important for explaining entrepreneurial outcomes, but we were silent on what specific traits might matter. Here, we address the potential role of cognitive and non-cognitive skills, shown to be determined both by genes and by social and environmental factors (Polderman et al., 2015; Grönqvist et al., 2017). These heritable skills are important for labor market outcomes in general (Lindqvist and Vestman, 2011) and entrepreneurship in particular (Hartog et al., 2010; Levine and Rubinstein, 2017).[40] We thus turn to pathways (i.e. children's traits) for the influence of

[39] This exercise follows Björklund et al. (2005) as closely as possible. They, however, have data on nine different sibling types including some reared apart (i.e. whose environments are not perfectly correlated). This allows them to loosen and test some of the strict assumptions we make in our additive model, producing lower estimates of genetic effects. Thus, our results place an upper bound on the contribution of shared genes to sibling correlations. We are unable to run similar genetic exercises on our intensive margin outcomes, since the sample sizes for the different sibling types become too small to be informative after conditioning on being a business owner.

[40] Extroversion and neuroticism partially mediate genetic influences in entrepreneurship (Zhang et al., 2009).

family and community background, asking: To what extent can sibling similarities in cognitive and non-cognitive skills help explain sibling similarities in business ownership?

For most male Swedish citizens in our sample we have formal tests of cognitive and non-cognitive skills taken from their military draft records. Specifically, we have tests of logical, verbal, spatial, and technical ability, leadership skills, as well as height, weight, and BMI, for which we report correlations in Panel A of Table 2.8. Brother correlations in height, weight, and BMI at age 18 are 53%, 43%, and 40%, respectively, while those in cognitive test scores range from 25% for technical ability to 32% for leadership skills.[41] In addition, in columns (9)-(14) of Panel B, we reestimate the brother correlations in business ownership for this sample. These correlations range from 25% to 43%, only slightly lower than their counterparts in Table 2.3.

We add these controls to our models in Panel C. Although these test scores are significant predictors of business ownership entry in columns (9) and (10), they are not quantitatively important: they explain 3.9% and 1.8% of the correlations in becoming *Unincorporated* and *Incorporated*. The odds ratios are all quite close to 1, and both types of business owners have slightly lower verbal and logical scores and slightly higher spatial and technical abilities. These results do not change if we control instead for the nine levels of these test scores as dummies. In columns (11)-(14), cognitive ability is associated with lower persistence, but higher business income, especially for logical and verbal ability (and technical ability for unincorporated firms).

The odds ratios and coefficients on leadership skills provide a more nuanced story. Scoring well on this test is not correlated with becoming *Unincorporated*. In stark contrast, leadership skills are strong predictors of becoming *Incorporated*: scores above the median raise the propensity to become *Incorporated* by a factor of 3 to 5. These skills are also highly correlated across brothers (32%), which implies that they depend in part on a shared family origin and help us to understand why the sibling correlation in *Incorporated* is greater than the sibling correlation in *Unincorporated*.

[41] These correlations are similar to the ones estimated by Björklund and Jäntti (2012) for Sweden.

Table 2.8: **Accounting for Cognitive and Noncognitive Characteristics**

	Height (1)	Weight (2)	BMI (3)	Logical (4)	Verbal (5)	Spatial (6)	Technical (7)	Leadership (8)
	A. Sibling correlations in given outcomes							
ρ	0.533	0.428	0.400	0.290	0.298	0.261	0.248	0.315
	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
			Entry		Years		Income	
			Uninc. (9)	Inc. (10)	Uninc. (11)	Inc. (12)	Uninc. (13)	Inc. (14)
	B. Sibling correlations in business ownership, no controls							
ρ			0.246	0.347	0.301	0.428	0.277	0.416
			(0.014)	(0.014)	(0.027)	(0.021)	(0.029)	(0.023)
	C. Sibling correlations in business ownership, with controls							
$\rho*$			0.242	0.337	0.287	0.415	0.256	0.383
			(0.014)	(0.014)	(0.027)	(0.022)	(0.029)	(0.024)
			1.55%	2.72%	4.41%	2.94%	7.57%	7.97%
			Odds ratios:		Coefficients:			
Height			0.997**	1.044**	0.006	0.020**	0.003*	0.008**
Weight			1.005**	1.000	0.002	0.011**	-0.002**	-0.004**
Logical			0.919**	0.995	-0.145**	-0.084**	0.061**	0.053**
Verbal			0.942**	0.905	-0.235**	-0.189**	0.039**	0.065**
Spatial			1.027**	1.006	-0.020	-0.001	-0.003	-0.004**
Technical			1.031**	1.045**	0.038	0.021	0.015**	0.003
Leadership score:		2	1.263**	1.494**	0.257	-0.108	0.126	0.099
		3	1.111	1.700**	0.755**	0.657	0.098	0.023
		4	1.040	2.138**	1.244**	1.092**	0.117	-0.037
		5	1.024	2.805**	1.212**	1.212**	0.194**	0.057
		6	1.023	3.213**	0.613*	0.725	0.357**	0.181*
		7	1.025	4.152**	0.413	0.453	0.459**	0.300**
		8	1.092	4.412**	-0.108	0.247	0.653**	0.410**
		9	1.313**	5.818**	-0.530	0.183	0.761**	0.430**

** $p < 0.05$, * $p < 0.1$ (*** $p < 0.01$ not reported for brevity). Standard errors in parentheses and p-values for the joint significance of the leadership scores in brackets. The percentages in Panel C indicate the contribution of cognitive and noncognitive skills to the corresponding sibling correlations in entrepreneurship in Panel B. BMI is not included in the models in Panel C, as it is highly correlated with weight ($\rho \approx 0.87$). Leadership dummies are jointly significant, with $p<0.001$. At the extensive margin, the sample comprises 164,390 men in 144,306 families, given data availability on all non-cognitive characteristics. At the intensive margin, there are 25,370 men in 24,518 families for unincorporated outcomes, and 22,601 men in 21,709 families for incorporated outcomes.

Furthermore, above median leadership skills are positively associated with business income (Hartog et al., 2010). Together, cognitive and non-cognitive ability explain 8% of sibling correlations in income from business ownership.

2.5 Conclusion

We quantify the importance of family background and neighborhood effects as determinants of entrepreneurship by estimating sibling correlations in unincorporated and incorporated business ownership entry, persistence and income. For men, background factors determine 30% to 45% of the total variation in their business ownership outcomes; for women, background determines 15% to 38% of their outcomes. Many of our estimated sibling correlations in entrepreneurship exceed those estimated for education and earnings (Björklund and Jäntti, 2012), suggesting that family background is a particularly salient factor for determining entrepreneurial outcomes. Our results indicate that such influences are up to five times larger than what parent-offspring correlations in entrepreneurship suggest. In other words, while parental entrepreneurship matters, other elements of family background also have substantial explanatory power.

We then study the relative importance of potential mechanisms highlighted by previous studies. What is it that parents give children that makes them so similar in their business ownership outcomes? Shared genes account for up to 50% of the sibling correlations for men and slightly more for women. For brothers, we also examine the role of similarities in cognitive and non-cognitive skills: while these scores explain little of our sibling correlations, above median leadership skills are a strong predictor of income from incorporated business ownership.

The most important parental characteristic for explaining sibling correlations is parental business ownership. Interestingly, parents' unincorporated business ownership explains a large share of the sibling correlations in unincorporated, but not incorporated business ownership, and vice versa, parents' incorporation mostly explains sibling correlations in incorporation, hinting towards type-specific entrepreneurial human capital and role-models. An additional share of sibling correlations in incorporation, i.e. the more capital-intensive form of business ownership, is explained by parental income, especially for sisters, suggesting potential financial constraints for this type of entrepreneurship (Levine and Rubinstein, 2018).

Neighborhood effects explain at most 8% and 5% of variation in business ownership for men and women, respectively. While sibling correlations appear to be largely driven by family background effects and not by shared community influences, this does not mean that neighborhoods do not matter. The neighborhood correlation in unincorporated business ownership for men is large enough to explain most of the observed gender difference in sibling correlations in unincorporated business ownership. We hypothesize that regional differences in male-dominated occupations, such as farming and construction, which (in Sweden) are typically organized as unincorporated firms, account for the lion's share of the gender difference in sibling correlations in entry and persistence in unincorporated self-employment. We also observe evidence in favor of a somewhat smaller neighborhood effect related to 'entrepreneurial spirit' or role modeling, as part of the neighborhood correlations is explained by the share of business owners in the parish during formative years (Giannetti and Simonov, 2009; Guiso et al., 2015).

We find little evidence in favor of sibling peer effects, although an older brother's choice of becoming an unincorporated business owner may raise the probability that his younger brother becomes an unincorporated business owner at a later date. This effect explains up to 8% of the brother correlation in unincorporated entry and further narrows the gap between brother and sister correlations in this outcome. Furthermore, parental education, immigration status, and family structure contribute little to sibling similarities in business ownership outcomes.

The exercises presented in this paper are not without limitations. First and foremost, when 'explaining' the determinants of sibling similarities, we cannot claim that we have presented a set of precise causal estimates. Instead, we view our results as part of an exploratory accounting exercise that can point us towards those factors which can potentially explain the largest share of sibling similarities, including but not limited to parental entrepreneurship. Second, since we measure the degree to which siblings are similar, we cannot exclude the possibility that single-child families operate in a different manner and that lone children are influenced in different ways by family and

community-wide factors.[42] Third, our results pertain to a highly developed economy, with specific cultural and economic traits, and notably egalitarian policies. Our results may likely hold in the other Nordic countries, where we observe similar sibling correlations in other outcomes such as income and education (Solon, 1999; Björklund and Jäntti, 2011; Black and Devereux, 2011), but they may not apply in other countries. Tracking changes over time (Björklund et al., 2009) and across countries (Schnitzlein, 2014) in sibling correlations in entrepreneurship would help us to decide whether the sibling correlations that we have documented should be considered relatively large or relatively small, and whether these numbers are constant across time and space.

There may, of course, be factors other than those we address here that contribute to sibling similarities. These may include parents' managerial ability, risk and time preferences, or family values. Capturing such variation would be an interesting avenue for future research, although parts of these effects are arguably captured by the observable parental characteristics we account for (e.g. parental risk preferences may determine parental business ownership) and may have a genetic component as well. In addition, a future reconciliation of heritability and sibling correlations could shed more light on the importance of (shared) genes in generating sibling similarity.

More generally, future research could attempt to undercover the particular pathways of influence: how much do similarities in cognitive and non-cognitive ability (which we touch upon), educational achievement, choice of organizational hierarchies, obtaining a patent, etc., explain sibling correlations in entrepreneurship? This would offer a more nuanced understanding of the sources of similarity between siblings, as well as provide a way of synthesizing the literature on contextual antecedents of entrepreneurship in a unified framework. Future research could also assess the importance of background for other career outcomes, such as becoming a CEO or an inventor (Aghion et al., 2018; Bell et al., 2018; Black et al., 2018; Mérida, 2019).

[42] In unreported models, the intergenerational association between parents and children in unincorporated and incorporated entrepreneurship in single- and multiple- child families was similar (i.e. 0.068 vs. 0.072 in unincorporated self-employment, and 0.132 and 0.102 in incorporation, with similar explanatory power). Parental entrepreneurship is thus at least as important for children in single child families as in multiple child families.

We view our findings optimistically. The existence of substantial predetermined family-wide factors does not mean that policy is doomed to fail. The majority of variation in the business ownership outcomes we study remains individual-specific. Furthermore, several of our findings are consistent with the recent literature supporting the idea that adolescents appear to 'learn' about entrepreneurship through their family and community environment, which implies it should be possible to 'teach' entrepreneurship to young people (Huber et al., 2014; Elert et al., 2015; Guiso et al., 2015). In particular, our results suggests that entrepreneurship education could be more effective by exposing young individuals to relevant role-models and potentially by emphasizing leadership skills, especially for individuals without access to opportunities to learn about entrepreneurship (Eesley and Wang, 2017; Lyons and Zhang, 2018). These also represent areas that would-be entrepreneurs should especially seek to develop as they build their human capital profile. Finally, policies designed to equip would be entrepreneurs with relevant skills may even generate a social multiplier effect if the behavior of a successfully treated person also affects the behavior of other family members, especially for future generations.

At the same time, one can not ignore the large role of family background in determining entrepreneurial outcomes. It is not clear that all young people with similar entrepreneurial skills have the same opportunities to actually develop into entrepreneurs. As such, there may be a pool of entrepreneurial talent that society could dip into and develop; and in doing so increase both equality of opportunity and economic efficiency.

Appendix A

A.1 Additional Figures and Tables

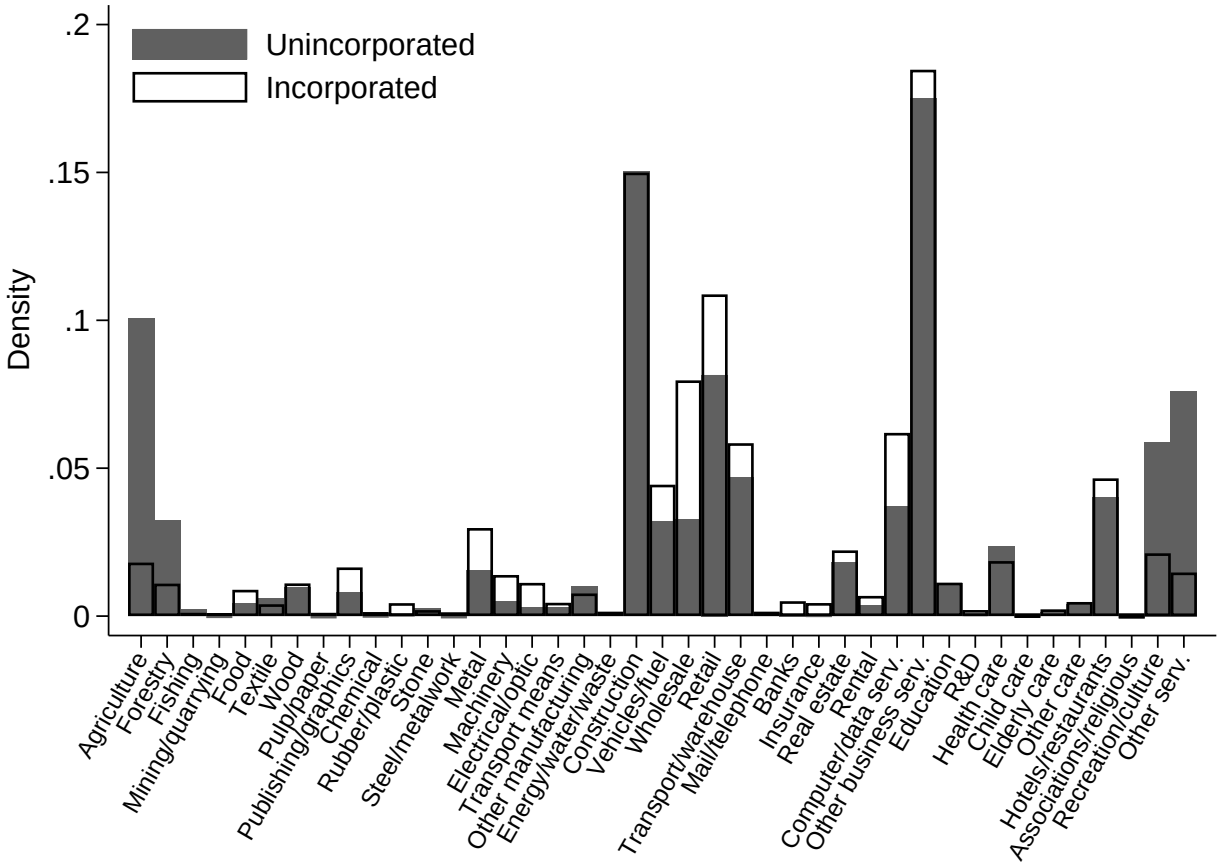

Figure A.1.1: Individual modal industries (1993-2010), by type of business.

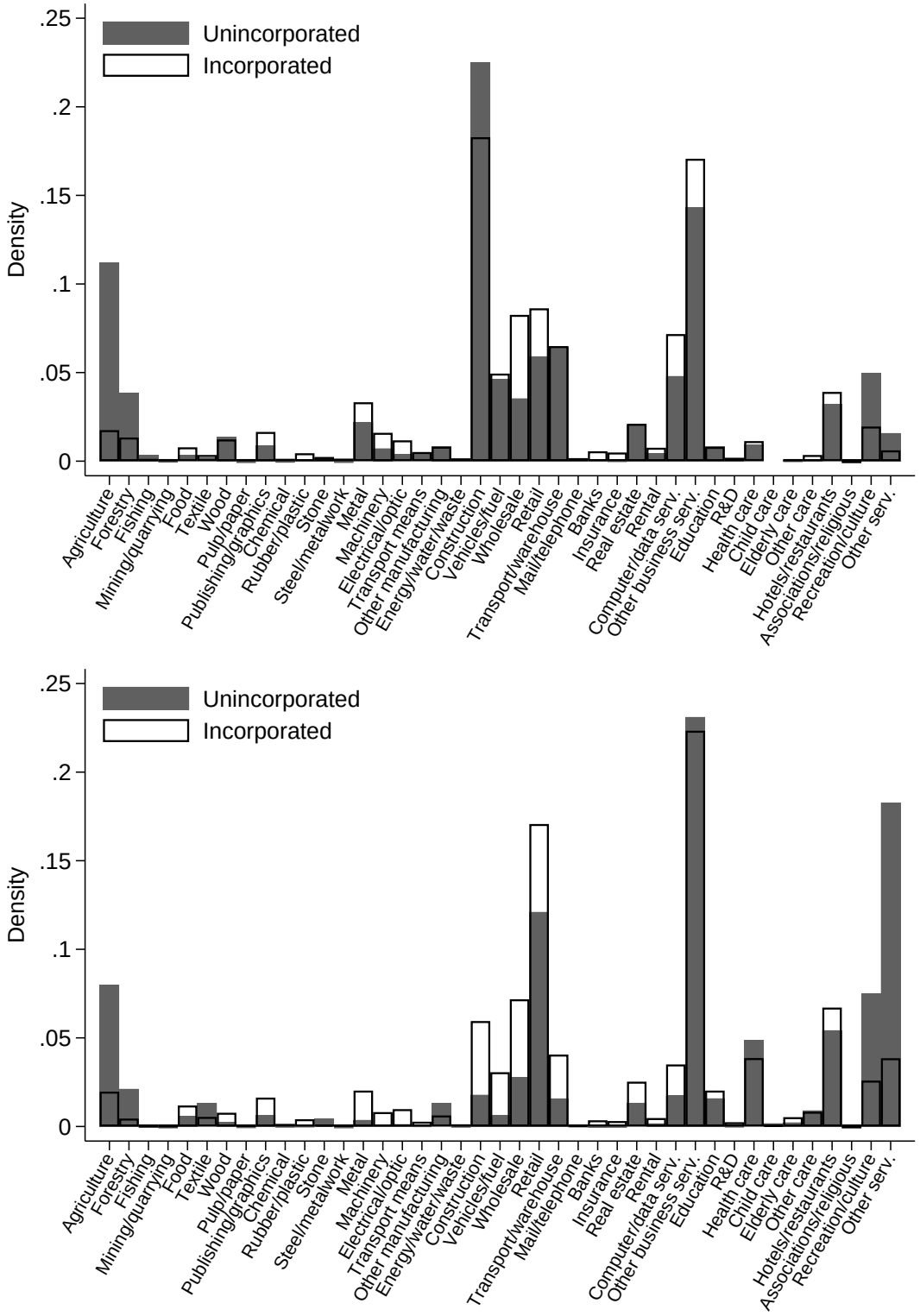

Figure A.1.2: Individual modal industries (1993-2010), by type of business and gender (upper panel – men; lower panel – women).

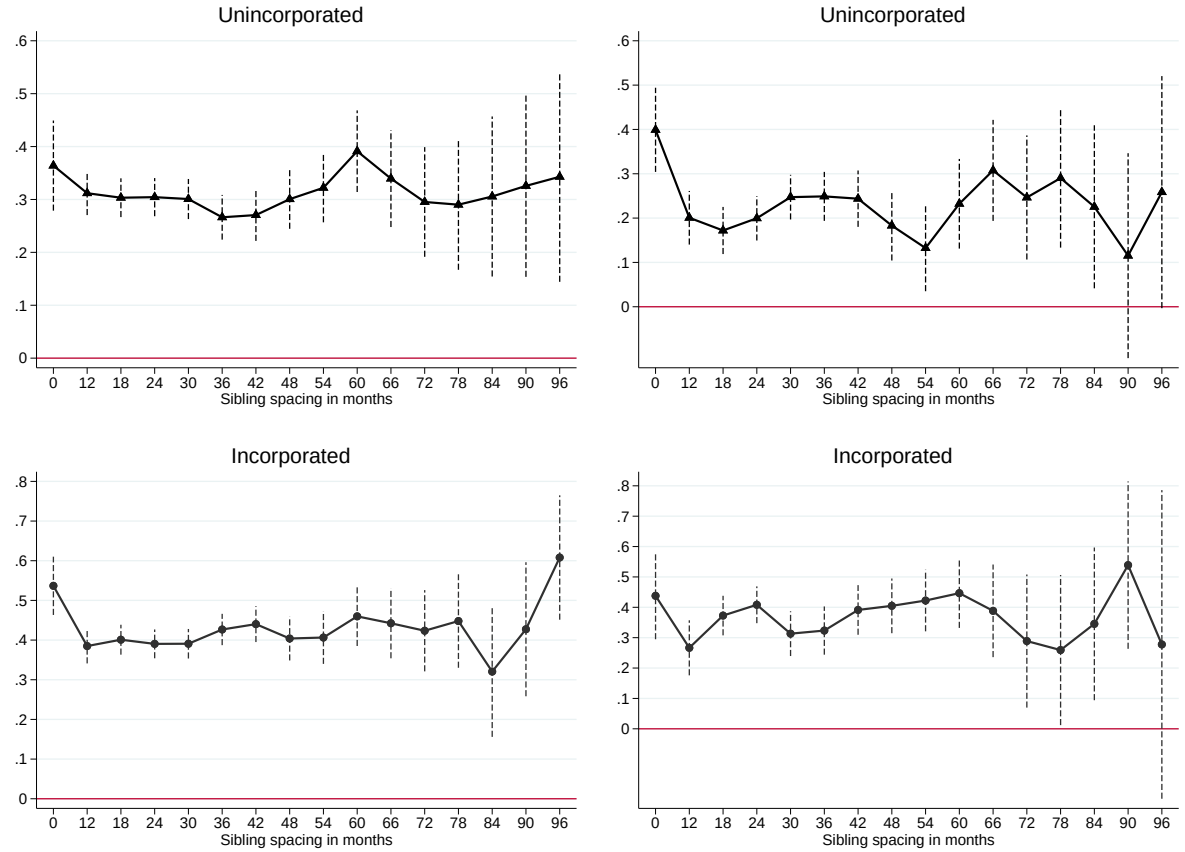

Figure A.1.3: Twin and sibling correlations in business ownership by sibling spacing and gender (left panel – men; right panel – women), with 95% confidence intervals.

Table A.1.1: **Number of Families with *N* Children**

N Children	No. of Families	%	No. of Individuals	%
1	227,860	52.88	227,860	32.73
2	152,050	35.28	304,100	43.68
3	41.818	9.70	125,454	18.02
4	7,592	1.76	30,368	4.36
5	1,312	0.30	6,560	0.94
6	243	0.06	1,458	0.21
7	49	0.01	343	0.05
8	11	0.00	88	0.01
Total	430,935	100.00	696,231	100.00

All children of the same mother are defined as belonging to the same family.

Table A.1.2: **Sensitivity Analyses**

	Entry		Years	
All siblings	Unincorporated (1)	Incorporated (2)	Unincorporated (3)	Incorporated (4)
(1) Excl. singletons	0.212	0.342	0.215	0.387
	(0.004)	(0.007)	(0.009)	(0.009)
(2) Incl. 1985-1992	0.232		0.214	
	(0.004)		(0.008)	
(3) Outcomes, ages 25-40	0.222	0.369	0.222	0.403
	(0.005)	(0.007)	(0.011)	(0.014)
(4) Father	0.204	0.331	0.214	0.386
	(0.004)	(0.005)	(0.009)	(0.009)
(5) Excl. adoptive fathers	0.206	0.331	0.216	0.387
	(0.004)	(0.005)	(0.009)	(0.009)
(6) Excl. adoptive mothers	0.212	0.342	0.215	0.387
	(0.004)	(0.005)	(0.009)	(0.009)
(7) Non-twin pairs	0.214	0.336	0.210	0.408
	(0.006)	(0.007)	(0.014)	(0.012)
(8) Non-twins, 12-24 months	0.208	0.298	0.230	0.430
	(0.012)	(0.015)	(0.028)	(0.027)
(9) Complete families	0.200	0.341	0.216	0.382
	(0.006)	(0.007)	(0.014)	(0.012)
(10) Complete parental data	0.208	0.341	0.213	0.394
	(0.004)	(0.005)	(0.010)	(0.009)
(11) Birth year dummies	0.211	0.340	0.214	0.388
	(0.004)	(0.005)	(0.009)	(0.009)
(12) Parents $\leq$ 65yo in 1993	0.210	0.341	0.202	0.379
	(0.005)	(0.005)	(0.010)	(0.010)
(13) Parents in data in 1993	0.212	0.341	0.201	0.392
	(0.005)	(0.006)	(0.011)	(0.010)
(14) Excl. farmer parents	0.207	0.340	0.182	0.378
	(0.005)	(0.005)	(0.010)	(0.010)
(15) Placebo families	0.002	0.003	0.004	0.005
	(0.002)	(0.005)	(0.006)	(0.007)

Standard errors in parentheses. Row (1) excludes singletons; row (2) includes data on unincorporated business ownership, 1985-1992; row (3) measures the outcomes only between ages 25 and 40; row (4) defines the family through the father; row (5) omits families with an adoptive father; row (6) omits families with an adoptive mother; row (7) restricts the analysis to families with two children; row (8) restricts it further to closely spaced non-twin pairs (born 12 to 24 months apart); row (9) restricts the analysis to families completely captured in our sample; row (10) restricts the analysis to observations for which all parental characteristics are observed; row (11) includes individual and parental birth year dummies; row (12) retains families with parents below age 65 in 1993; row (13) retails families with parents who have not left the data set and are below age 65 in 1993; row (14) drops families where one of the parents is a farmer; finally, row (15) is a placebo test, where the family cluster structure is replicated and individuals randomly allocated to families, with 100 bootstrap replications.

Table A.1.3: **Sibling Correlations in Business Ownership, by Family Type**

	Entry		Years		Income	
	Unincorporated	Incorporated	Unincorporated	Incorporated	Unincorporated	Incorporated
	(1)	(2)	(3)	(4)	(5)	(6)
A. Male only sibships						
ρ	0.269	0.399	0.318	0.463	0.291	0.415
	(0.012)	(0.012)	(0.023)	(0.018)	(0.026)	(0.021)
Individuals	69,340	69,340	10,597	8,466	10,597	8,466
Families	43,983	43,983	9,446	7,363	9,446	7,363
B. Female only sibships						
ρ	0.206	0.351	0.167	0.358	0.113	0.345
	(0.017)	(0.022)	(0.045)	(0.046)	(0.045)	(0.046)
Individuals	62,296	62,296	6,185	2,923	6,185	2,923
Families	40,304	40,304	5,783	2,747	5,783	2,747
C. Mixed gender sibships						
ρ	0.173	0.311	0.175	0.321	0.202	0.304
	(0.008)	(0.009)	(0.018)	(0.018)	(0.018)	(0.021)
Individuals	138,654	138,654	17,729	12,423	17,729	12,423
Families	57,416	57,416	15,258	10,831	15,258	10,831

Standard errors in parentheses. The sample is restricted to families captured completely in our sample in order to correctly define family type (by child gender: male only, female only, mixed).

Table A.1.4: **Neighborhood Correlations in Business Ownership**

	Entry		Years		Income	
	(1) Uninc.	(2) Inc.	(3) Uninc.	(4) Inc.	(5) Uninc.	(6) Inc.
A. No controls						
All	0.028	0.020	0.038	0.027	0.021	0.027
	(0.001)	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)
	13.03%	5.89%	17.90%	7.03%	9.94%	7.67%
Brothers	0.38	0.20	0.73	0.036	0.037	0.031
	(0.002)	(0.001)	(0.004)	(0.003)	(0.003)	(0.003)
	13.11%	5.06%	23.67%	7.96%	12.59%	7.36%
Sisters	0.018	0.022	0.002	0.013	0.007	0.020
	(0.001)	(0.002)	(0.001)	(0.004)	(0.002)	(0.004)
	8.40%	6.24%	1.16%	3.30%	4.14%	5.55%
B. Parental controls (excl. business ownership)						
All	0.027	0.015	0.028	0.021	0.011	0.011
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
	12.76%	4.53%	13.27%	5.48%	5.13%	3.32%
Brothers	0.036	0.016	0.056	0.027	0.021	0.013
	(0.002)	(0.001)	(0.004)	(0.003)	(0.002)	(0.002)
	12.46%	3.98%	18.18%	5.96%	7.21%	3.07%
Sisters	0.015	0.013	0.002	0.011	0.002	0.008
	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)	(0.003)
	7.04%	3.75%	1.07%	2.98%	1.58%	2.16%
C. Parental controls (incl. business ownership)						
All	0.017	0.010	0.017	0.016	0.007	0.009
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
	8.10%	2.99%	7.81%	4.18%	3.58%	2.48%
Brothers	0.023	0.011	0.034	0.020	0.014	0.009
	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)	(0.002)
	7.99%	2.60%	10.96%	4.52%	4.79%	2.22%
Sisters	0.011	0.010	0.002	0.011	0.002	0.007
	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)	(0.003)
	5.21%	2.75%	1.05%	2.76%	1.54%	1.98%
D. All controls (incl. share of business owners in neighborhood)						
All	0.013	0.006	0.012	0.014	0.006	0.006
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	6.10%	1.84%	5.44%	3.69%	2.99%	1.88%
Brothers	0.018	0.006	0.022	0.018	0.011	0.007
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)
	6.10%	1.58%	7.20%	3.93%	3.70%	1.59%
Sisters	0.018	0.006	0.002	0.009	0.002	0.006
	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)	(0.003)
	3.81%	1.62%	0.97%	2.42%	1.34%	1.55%

Standard errors in parentheses. The percentages indicate the contribution of the neighborhood correlation to the corresponding sibling correlations in Table 2.3.

Table A.1.5: **Accounting Exercise: Effect of Noisy Random Variables**

	Entry		Years		Income	
	(1) Uninc.	(2) Inc.	(3) Uninc.	(4) Inc.	(5) Uninc.	(6) Inc.
A. Individual level: 20 random variables in (0,1)						
Brothers	0.292	0.404	0.309	0.448	0.297	0.416
	(0.007)	(0.007)	(0.013)	(0.011)	(0.014)	(0.014)
	0.01%	0.02%	0.05%	-0.21%	-0.11	-0.11%
Sisters	0.213	0.352	0.170	0.380	0.158	0.360
	(0.010)	(0.013)	(0.026)	(0.030)	(0.026)	(0.033)
	0.03%	-0.06%	-0.55%	0.30%	-0.46%	0.04%
B. Individual level: 100 random variables in (0,1)						
Brothers	0.292	0.404	0.309	0.446	0.298	0.416
	(0.007)	(0.007)	(0.013)	(0.011)	(0.014)	(0.013)
	0.05%	-0.11%	0.04%	0.11%	-0.48%	-0.02%
Sisters	0.213	0.352	0.169	0.385	0.160	0.362
	(0.010)	(0.013)	(0.026)	(0.030)	(0.026)	(0.034)
	-0.12%	-0.08%	-0.16%	-1.16%	-1.59%	-0.53%
C. Family level: 20 random variables in (0,1)						
Brothers	0.292	0.403	0.309	0.447	0.296	0.416
	(0.007)	(0.007)	(0.013)	(0.011)	(0.014)	(0.013)
	0.06%	0.04%	0.09%	0.00%	0.03%	-0.01%
Sisters	0.213	0.351	0.168	0.381	0.157	0.361
	(0.010)	(0.013)	(0.026)	(0.029)	(0.026)	(0.033)
	0.09%	0.07%	0.17%	-0.06%	0.63%	-0.07%
D. Family level: 100 random variables in (0,1)						
Brothers	0.291	0.403	0.309	0.446	0.296	0.415
	(0.007)	(0.007)	(0.013)	(0.011)	(0.014)	(0.013)
	0.24%	0.09%	0.14%	0.09%	0.17%	0.10%
Sisters	0.212	0.350	0.170	0.378	0.157	0.361
	(0.010)	(0.013)	(0.026)	(0.030)	(0.026)	(0.033)
	0.53%	0.38%	-0.58%	0.72%	0.11%	-0.08%

Standard errors in parentheses. The percentages indicate the change in the sibling correlations in Table 3 once the noisy random variables are controlled for (a negative percentage change indicates an increase in the sibling correlation, while a positive sign indicates a decrease). In panels A and B, the noisy random variables are generated at the individual level, and the largest change in the sibling correlation is a 1.59% *increase*. In panels C and D, the noisy random variables are generated at the family level, such that they are the same for siblings, and should have a higher explanatory power than those generated at the individual level. Even so, they explain at most 0.72% of the sibling correlation. While the number of variables appears inconsequential when variables are generated at the individual level, when they are generated at the family level the decrease is slightly larger when 100 variables are added instead of 20. Our models in Table 2.5 include far less than 100 variables, suggesting little cause for concern that the explanatory power is generated by random noise.

Table A.1.6: **Accounting Exercise, Extensive and Intensive Margin Outcomes**

		Entry		Years		Income	
		(1) Uninc.	(2) Inc.	(3) Uninc.	(4) Inc.	(5) Uninc.	(6) Inc.
A. Parental education	All	0.210	0.335	0.208	0.379	0.190	0.321
		(0.004)	(0.005)	(0.009)	(0.009)	(0.010)	(0.011)
		1.07%	1.84%	3.06%	1.91%	9.18%	7.17%
	Brothers	0.289	0.399	0.300	0.437	0.273	0.390
		(0.007)	(0.007)	(0.013)	(0.011)	(0.014)	(0.014)
		0.86%	1.13%	3.14%	2.07%	7.88%	6.12%
	Sisters	0.205	0.341	0.167	0.379	0.149	0.347
		(0.010)	(0.013)	(0.026)	(0.030)	(0.027)	(0.034)
		3.66%	2.98%	0.99%	0.47%	5.62%	3.67%
B. Parental income	All	0.209	0.315	0.204	0.386	0.169	0.325
		(0.004)	(0.005)	(0.009)	(0.009)	(0.010)	(0.011)
		1.46%	7.58%	4.80%	0.21%	19.00%	5.96%
	Brothers	0.288	0.382	0.294	0.446	0.249	0.397
		(0.007)	(0.007)	(0.013)	(0.011)	(0.015)	(0.014)
		1.47%	5.41%	4.99%	0.20%	15.87%	4.50%
	Sisters	0.206	0.321	0.168	0.378	0.139	0.340
		(0.010)	(0.013)	(0.026)	(0.030)	(0.027)	(0.034)
		3.14%	8.67%	0.51%	0.75%	12.01%	5.49%
C. Parental business ownership	All	0.189	0.282	0.196	0.352	0.197	0.332
		(0.004)	(0.005)	(0.009)	(0.010)	(0.010)	(0.011)
		10.67%	17.26%	8.38%	8.81%	5.62%	4.10%
	Brothers	0.267	0.348	0.284	0.413	0.279	0.399
		(0.007)	(0.008)	(0.013)	(0.012)	(0.014)	(0.014)
		8.66%	13.75%	8.12%	7.47%	5.80%	4.03%
	Sisters	0.199	0.309	0.164	0.350	0.154	0.354
		(0.010)	(0.014)	(0.026)	(0.031)	(0.026)	(0.033)
		6.37%	12.15%	2.79%	8.07%	2.46%	1.65%
D. Other family traits	All	0.211	0.337	0.209	0.381	0.203	0.345
		(0.004)	(0.005)	(0.009)	(0.009)	(0.010)	(0.011)
		0.39%	1.34%	2.28%	1.46%	2.68%	0.23%
	Brothers	0.290	0.399	0.304	0.440	0.291	0.415
		(0.007)	(0.007)	(0.013)	(0.011)	(0.014)	(0.013)
		0.56%	1.18%	1.81%	1.38%	1.86%	0.23%
	Sisters	0.213	0.348	0.168	0.379	0.153	0.360
		(0.010)	(0.013)	(0.026)	(0.030)	(0.027)	(0.033)
		0.05%	1.06%	0.49%	0.63%	3.02%	0.09%
E. All family controls	All	0.183	0.266	0.184	0.344	0.158	0.228
		(0.004)	(0.005)	(0.009)	(0.010)	(0.010)	(0.012)
		13.48%	22.05%	13.90%	11.08%	24.34%	16.62%
	Brothers	0.258	0.334	0.268	0.403	0.234	0.359
		(0.007)	(0.008)	(0.013)	(0.012)	(0.015)	(0.015)
		11.63%	17.23%	13.27%	9.83%	20.93%	13.66%
	Sisters	0.190	0.287	0.163	0.349	0.133	0.319
		(0.010)	(0.014)	(0.026)	(0.031)	(0.027)	(0.036)
		10.75%	18.20%	3.40%	8.42%	15.64%	11.36%

Standard errors in parentheses. The percentages indicate the contribution of parental characteristics to the corresponding sibling correlations in columns (1) and (2) of Table 2.3.

A.2 Peer Effects Model and Results

We provide a formal exposition of the correlated random effects model suggested by Altonji et al. (2017) that we adopt for the purpose of estimating sibling peer effects in entrepreneurship. We begin by estimating the raw association between sibling i's unincorporated or incorporated business ownership at time t, $S_t^{i'}$, and sibling i''s business ownership at time $t-1$, $S_{t-1}^{i'}$:

$$S_t^i = \beta_0 + \beta_1 S_{t-1}^{i'} + u_t^2, \tag{A.2.1}$$

where the family subscript f is suppressed. We then add the set of controls used in the accounting exercise, X^f, and age dummies age_t^i for the focal sibling i:

$$S_t^i = \beta_0 + \beta_1 S_{t-1}^{i'} + X^f + age_t^i + \epsilon_t^2. \tag{A.2.2}$$

We estimate equations (A.2.1) and (A.2.2) (corresponding to columns (1), (2), (6) and (7) in Tables A.2.1 to A.2.5) by using the panel structure of our data, limiting the sample to families with two children.[1] We later split the sample into pairs of males, females, and mixed gender pairs, where the younger sibling is male or female. We use logistic regressions in order to maintain consistency with previous estimation techniques, and we report both odds ratios and (approximated) sibling correlations, as explained in the notes to Table A.2.1.

Part of the effect of sibling i''s business ownership on sibling i's business ownership estimated in equation (A.2.2), however, may be due to correlated random family effects, rather than direct peer effects. Altonji et al. (2017) suggest the use of a correlated random effects regression to isolate the direct sibling effect, achieving causal inference by assuming one-directional causation (whereas our study does not attempt to directly target causality); they control for the sum of sibling i''s business ownership at time $t-1$ and $t+1$ to net out the unobservable family component. We can then write:

$$S_t^i = \beta_0 + \beta_1 (S_{t-1}^{i'} + S_{t+1}^{i'}) + \lambda_0 S_{t-1}^{i'} + X^f + age_t^i + age_t^{i'} + \epsilon_t^2, \tag{A.2.3}$$

where the direct (lagged) sibling effect is captured by λ_0.[2] Similarly, we can also include a direct contemporaneous sibling influence by including sibling i''s business ownership at time t, $S_t^{i'}$, in conjunction with an expanded control for correlated random effects:

$$S_t^i = \beta_0 + \beta_1 (S_{t-1}^{i'} + S_t^{i'} + S_{t+1}^{i'}) + \lambda_0 S_{t-1}^{i'} + \lambda_1 S_t^{i'} + X^f + age_t^i + age_t^{i'} + \epsilon_t^2, \tag{A.2.4}$$

where λ_1 is the estimate of the 'contemporaneous' effect. This estimate should not be interpreted as a true contemporaneous effect, but rather as a transitory and common shock to both siblings in the same family. Hence, we do not sum the lagged and contemporaneous sibling effect when analyzing the contribution of peers to the sibling correlation (in contrast to Eriksson et al., 2016, for instance). Results for equations (A.2.3) and (A.2.4) are given in columns (3), (4), (8) and (9) of Tables A.2.1-A.2.5 below, while columns (5) and (10) present results from a variation of equation (A.2.1), where the lagged sibling effect is replaced by the contemporaneous one (this equation being necessary for calibration purposes).

As an example of how these tables should be interpreted, Table A.2.1 shows the results of our sibling peer effects exercise on the sub-sample of sibling pairs, with panel A referring to unincorporated business ownership and panel B to incorporation; in columns (1) to (5) sibling i

[1] Sibling correlations for this sample are reported in row (7) of Appendix Table A.1.2 and closely match those reported in Table 2.3; see also footnote 33.

[2] A detailed description of the assumptions and mechanics of this model is provided in Altonji et al. (2017). Importantly, they assume that only older siblings can influence the younger ones (and not the other way around), and that parental treatment of younger siblings does not change upon observing the behavior of older siblings.

is the younger one, whereas in columns (6)-(10), sibling i is the older one. The results suggest a positive and significant (at 10 percent) impact of the younger sibling's incorporation status at time $t-1$ on the older sibling's incorporation status at time t, as shown in column (9) of Table A.2.1. This translates into a sibling correlation $\rho = 0.009$ as given by the lagged sibling effect, representing 2.85 percent of the baseline sibling correlation, as shown in column (4) of Table 2.6 in the main text. Tables A.2.2-A.2.5 then perform the same exercise separately for male pairs, female pairs, and mixed gender pairs with a younger male and younger female, respectively. For unincorporated brothers, peer effects account for 8% of the sibling correlations; for other outcomes and sibling types, peer effects are small and usually not statistically significant.

While we argue that contemporaneous effects should not be included in the contribution of peer effects to the sibling correlation (due to common environmental influences), they do provide useful information. First, they are rarely significant for unincorporated self-employment, suggesting a limited role for common transitory shocks for this type of business. Second, they are always significant and positive for incorporation, and explain around 20% of the sibling correlation for all sibling types together (around 25% for brothers, 12.5% for sisters, and between 13% and 23% for mixed gender siblings).

In theory, this contemporaneous effect is consistent both with large common transitory shocks and with the possibility that siblings co-found businesses or take over the family firm. However, it suggests a rather loose upper bound on the importance of inheritance for our sibling correlations in the region of 25% for brothers, and smaller for other types of siblings. In our sample, siblings are observed as incorporated business owners at the same time and in the same industry in at most 3% of years, suggesting that co-ownership by siblings is a limited phenomenon and that common transitory shocks account for the majority of the contemporaneous effects in incorporation.[3] We also speculate that these results may also contribute to the distinction between unincorporated business ownership – usually a small scale, individual endeavor – and incorporated business ownership, a more complex organizational form likely to require a larger founding team.

[3] Note also that sibling entrepreneurs perform rather poorly, at least compared to spousal entrepreneurs (see, e.g., Bird and Zellweger, 2018).

Table A.2.1: **Peer Effects Exercise, All Sibling Types**

	A. Unincorporated									
	Old on young $[\phi_{t-1} = 0.084, \phi_t = 0.082]$					Young on old $[\phi_{t-1} = 0.084, \phi_t = 0.082]$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OR($S_{t-1}^{i'}$)	3.096***	2.510***	1.065	1.065		3.067***	2.487***	0.975	0.955	
ρ	0.208	0.166	0.011	0.011		0.208	0.166	-0.004	-0.008	
OR($S_t^{i'}$)				1.041	3.185***				0.955	3.185***
ρ				0.007	0.208				-0.007	0.208
	B. Incorporated									
	Old on young $[\phi_{t-1} = 0.068, \phi_t = 0.066]$					Young on old $[\phi_{t-1} = 0.068, \phi_t = 0.066]$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OR($S_{t-1}^{i'}$)	7.802***	4.584***	0.936	0.968		0.942***	4.583***	1.052	1.070*	
ρ	0.324	0.231	-0.009	-0.004		0.324	0.229	0.007	0.009	
OR($S_t^{i'}$)				1.650***	8.412***				1.632***	8.412***
ρ				0.068	0.324				0.067	0.324
Family background		Yes	Yes	Yes			Yes	Yes	Yes	
Age dummies, i		Yes	Yes	Yes			Yes	Yes	Yes	
Age dummies, i'			Yes	Yes				Yes	Yes	
$S_{t-1}^{i'} + S_{t+1}^{i'}$			Yes					Yes		
$S_{t-1}^{i'} + S_t^{i'} + S_{t+1}^{i'}$				Yes					Yes	

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. In columns (1)-(5), sibling i is the younger sibling; in columns (6)-(10), sibling i is the older sibling. The odds ratios (OR) are estimated using logistic regressions. Family background variables are those used in the accounting exercise: parental education, income, immigration, ownership of an unincorporated or incorporated business, and the family structure variables. Sibling correlations in columns (1), (5), (6) and (10) are estimated using Stata's *xtlogit* command; those in columns (2)-(4) and (7)-(9) are approximated using the following formula (Bonett, 2007): $\rho \approx (OR^{\phi} + 1)(OR^{\phi} - 1)$, where ϕ is calibrated using the odds ratios and sibling correlations estimated in columns (1) and (6) for lagged effects (ϕ_{t-1}), and (5) and (10) for contemporaneous effects (ϕ_t).

Table A.2.2: **Peer Effects Exercise, Brothers**

	A. Unincorporated									
	Old on young $[\phi_{t-1} = 0.084, \phi_t = 0.082]$					Young on old $[\phi_{t-1} = 0.084, \phi_t = 0082]$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OR$(S_{t-1}^{i'})$	4.530***	3.596***	1.131**	1.142**		4.487***	3.542***	0.980	0.967	
ρ	0.289	0.240	0.021	0.023		0.289	0.238	-0.003	-0.006	
OR$(S_t^{i'})$				1.178**	4.723***				1.029	4.723***
ρ				0.027	0.289				0.005	0.289
	B. Incorporated									
	Old on young $[\phi_{t-1} = 0.075, \phi_t = 0.073]$					Young on old $[\phi_{t-1} = 0.075, \phi_t = 0.73]$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OR$(S_{t-1}^{i'})$	9.542***	5.754***	0.984	1.023		9.646***	5.730***	1.006	1.040	
ρ	0.405	0.302	-0.002	0.003		0.405	0.300	0.001	0.006	
OR$(S_t^{i'})$				1.945***	10.395***				1.821***	10.395***
ρ				0.102	0.405				0.091	0.405
Family background		Yes	Yes	Yes			Yes	Yes	Yes	
Age dummies, i		Yes	Yes	Yes			Yes	Yes	Yes	
Age dummies, i'			Yes	Yes				Yes	Yes	
$S_{t-1}^{i'} + S_{t+1}^{i'}$			Yes					Yes		
$S_{t-1}^{i'} + S_t^{i'} + S_{t+1}^{i'}$				Yes					Yes	

See notes to Table A.2.1. The p-values of the significant odds ratios of $S_{t-1}^{i'}$ in columns (3) and (4) of panel A are 0.045 and 0.034, respectively.

Table A.2.3: **Peer Effects Exercise, Sisters**

	A. Unincorporated									
	Old on young $[\phi_{t-1} = 0.087, \phi_t = 0.085]$					Young on old $[\phi_{t-1} = 0.088, \phi_t = 0.085]$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OR($S_{t-1}^{i'}$)	3.136***	2.668***	1.137	1.130		3.095***	2.629***	1.052	0.993	
ρ	0.221	0.187	0.023	0.022		0.221	0.186	0.009	-0.001	
OR($S_t^{i'}$)				1.120	3.259***				1.023	3.259***
ρ				0.019	0.221				0.004	0.221
	B. Incorporated									
	Old on young $[\phi_{t-1} = 0.062, \phi_t = 0.060]$					Young on old $[\phi_{t-1} = 0.061, \phi_t = 0.060]$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OR($S_{t-1}^{i'}$)	11.005***	6.870***	0.776*	0.818		11.509***	7.107***	1.047	1.056	
ρ	0.345	0.269	-0.031	-0.025		0.345	0.268	0.006	0.007	
OR($S_t^{i'}$)				1.428*	11.990***				1.459*	11.990***
ρ				0.043	0.345				0.046	0.345
Family background		Yes	Yes	Yes			Yes	Yes	Yes	
Age dummies, i		Yes	Yes	Yes			Yes	Yes	Yes	
Age dummies, i'			Yes	Yes				Yes	Yes	
$S_{t-1}^{i'} + S_{t+1}^{i'}$			Yes					Yes		
$S_{t-1}^{i'} + S_t^{i'} + S_{t+1}^{i'}$				Yes					Yes	

See notes to Table A.2.1.

Table A.2.4: **Peer Effects Exercise, Mixed (Younger Brother)**

	A. Unincorporated									
	Old on young $[\phi_{t-1} = 0.087, \phi_t = 0.085]$					Young on old $[\phi_{t-1} = 0.086, \phi_t = 0.085]$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OR($S_{t-1}^{i'}$)	2.026***	1.772***	0.927	0.939		2.043***	1.784***	0.980	0.972	
ρ	0.133	0.107	-0.013	-0.011		0.133	0.107	-0.004	-0.005	
OR($S_t^{i'}$)				0.852	2.063***				0.914	2.062***
ρ				-0.027	0.133				-0.015	0.133
	B. Incorporated									
	Old on young $[\phi_{t-1} = 0.051, \phi_t = 0.050]$					Young on old $[\phi_{t-1} = 0.051, \phi_t = 0.050]$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OR($S_{t-1}^{i'}$)	5.491***	3.051***	0.896	0.916		5.587***	3.121***	1.150	1.155	
ρ	0.189	0.120	-0.011	-0.009		0.189	0.121	0.014	0.015	
OR($S_t^{i'}$)				1.351**	5.792***				1.309**	5.792***
ρ				0.030	0.189				0.027	0.189
Family background		Yes	Yes	Yes			Yes	Yes	Yes	
Age dummies, i		Yes	Yes	Yes			Yes	Yes	Yes	
Age dummies, i'			Yes	Yes				Yes	Yes	
$S_{t-1}^{i'} + S_{t+1}^{i'}$			Yes					Yes		
$S_{t-1}^{i'} + S_t^{i'} + S_{t+1}^{i'}$				Yes					Yes	

See notes to Table A.2.1.

Table A.2.5: **Peer Effects Exercise, Mixed (Younger Sister)**

	A. Unincorporated									
	Old on young $[\phi_{t-1} = 0.078, \phi_t = 0.078]$					Young on old $[\phi_{t-1} = 0.080, \phi_t = 0.078]$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OR($S^{i'}_{t-1}$)	2.230***	1.950***	1.006	0.984		2.178***	1.922***	0.904	0.886	
ρ	0.133	0.110	0.001	-0002		0.133	0.111	-0.016	-0.019	
OR($S^{i'}_{t}$)				0.915	2.235***				0.793**	2.235***
ρ				-0.014	0.133				-0.035	0.133
	B. Incorporated									
	Old on young $[\phi_{t-1} = 0.054, \phi_t = 0.052]$					Young on old $[\phi_{t-1} = 0.053, \phi_t = 0.052]$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OR($S^{i'}_{t-1}$)	4.950***	3.040***	0.898	0.926		5.111***	3.126***	1.147	1.138	
ρ	0.189	0.128	-0.012	-0.008		0.189	0.128	0.015	0014	
OR($S^{i'}_{t}$)				1.273*	5.286***				1.518***	5.286***
ρ				0.025	0.189				0.044	0.189
Family background		Yes	Yes	Yes			Yes	Yes	Yes	
Age dummies, i		Yes	Yes	Yes			Yes	Yes	Yes	
Age dummies, i'			Yes	Yes				Yes	Yes	
$S^{i'}_{t-1} + S^{i'}_{t+1}$			Yes					Yes		
$S^{i'}_{t-1} + S^{i'}_{t} + S^{i'}_{t+1}$				Yes					Yes	

See notes to Table A.2.1.

Chapter 3

Same, but Different?

Birth Order, Family Size, and Sibling Sex Composition Effects in Entrepreneurship

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3.1 Introduction

Family background matters for occupational choice and children with entrepreneurial parents are more likely to become entrepreneurs themselves.[1] This intergenerational association stems from the transmission of genes (Nicolaou et al., 2008; Lindquist et al., 2015), exposure to role models (Sørensen, 2007b; Hoffmann et al., 2015), and the transfer of general and business specific human and financial capital (Blanchflower and Oswald, 1998; Dunn and Holtz-Eakin, 2000; Hout and Rosen, 2000). More broadly, families provide children with a home environment, resources, networks, and social context, especially in early life when preferences for entrepreneurship are formed (Sørensen, 2007b; Huber et al., 2014; Elert et al., 2015; Guiso et al., 2015). These aspects are shared by children in the same family, and studies of siblings show that family background may explain up to half the variation in entrepreneurship outcomes, with the remaining variation attributed to influences specific to each sibling, as shown in the previous chapter.[2]

Prime candidates for such idiosyncratic influences are the differences between siblings that families themselves generate. In a popular example, first born children are regarded as more conforming, disciplined, and responsible, making them more likely to inherit the family firm (Bennedsen et al., 2007; Calabrò et al., 2018) or become entrepreneurs; by contrast, last born children are often perceived as risk taking, creative, or disruptive, traits typically associated with successful entrepreneurs (Sulloway, 1996; Han and Greene, 2016; Black et al., 2018). This may be particularly true in larger families, where the scope for individual differentiation is greater and family resources – both human and financial capital – are more thinly spread across children. Beyond influencing birth order, family size may thus hinder entrepreneurship due to resource constraints.[3] Moreover, boys and girls differ in the relationships they establish with parents and the way they experience

[1] I refer to entrepreneurship as a measure of business ownership, disaggregated between unincorporated and incorporated businesses. This term is often used interchangeably with self-employment; for clarity, I only refer to self-employment when the papers I discuss do so (and do not provide more granular measures).

[2] Chapter 2 is an update of Lindquist et al. (2018); for brevity, I refer to the latter throughout this chapter.

[3] For popular coverage of birth order and family size and entrepreneurship, see also the Kauffman Foundation 'Anatomy of an Entrepreneur' report (Wadhwa et al., 2009) and Tech Crunch (2012).

mothers and fathers as entrepreneurial role models (Hoffmann et al., 2015; Lindquist et al., 2015; Mishkin, 2017). Such distinctions are important if individuals are systematically hindered in their acquisition of entrepreneurial human capital by factors related to their family and deserve further investigation (Bertrand and Schoar, 2006).

To understand how differences generated inside the household affect siblings' occupational choices, I assess the differential effects of i) birth order, ii) family size, and iii) sibling sex composition on entrepreneurship entry. These three closely related factors have been highlighted in human capital research and may prove particularly relevant for entrepreneurship. In addition, I embrace heterogeneity with regards to gender, as well as unincorporated and incorporated business ownership, which represent types of entrepreneurship with strikingly different income, growth, innovation, and job creation outcomes (Levine and Rubinstein, 2017). My approach is to first analyze the causal effects of each factor separately, then analyze their joint impact on estimates of the total importance of family and community background in a variance decomposition framework, drawing on the method outlined by Björklund and Jäntti (2012).

Prior research suggests that later born children are more likely to be self-employed, which Han and Greene (2016) and Black et al. (2018) interpret as evidence for the hypothesis that later borns are more creative and innovative (Sulloway, 1996). However, this result requires closer attention, given that later born children actually fare worse in cognitive and non-cognitive ability, and educational attainment (Black et al., 2005, 2018; Lehmann et al., 2018). While later borns could be expected to be more entrepreneurial, they may be poor entrepreneurs, driven by necessity, rather than the pursuit of opportunity. Birth order effects may vary with the number of siblings, but evidence of family size effects in entrepreneurship is notably absent. Family size is expected to hinder successful entry, since larger families impede human capital accumulation in financially constrained households (Black et al., 2005; Åslund and Grönqvist, 2010; Lafortune and Lee, 2014) and limit children's ability to use parental resources in starting up. The allocation of parental

resources may also vary with child gender, and Mishkin (2017) finds that women whose father is self-employed are less likely to become self-employed when a brother is present. However, she does not analyze the independent effect of opposite gender siblings, which other studies suggest may influence education and earnings (Butcher and Case, 1994; Hauser and Kuo, 1998), reinforcing traditional gender roles and affecting women's educational choices (Cools and Patacchini, 2019; Peter et al., 2018). Thus, there appears to be substantial scope for within-household sibling differences in entrepreneurship, with important implications for both understanding family dynamics and increasing women's entrepreneurship.

To analyze these differences, I use administrative data from Sweden. My dataset covers 700,000 individuals born between 1960 and 1970 in 430,000 families, with detailed information on the family's socio-economic status and men's cognitive and non-cognitive ability. I follow the Swedish tax authority to classify individuals as entrepreneurs if they draw the majority of their taxable labor income from an unincorporated or incorporated business they own in part or in full. Unincorporated and (non-listed, limited liability) incorporated firms are not just legally different, but also represent different phenomena. Incorporation serves as a proxy for growth oriented entrepreneurship (Henrekson and Sanandaji, 2014, 2019; Tåg et al., 2016), with positively selected founders (Lindquist et al., 2018; Humphries, 2017), higher presence in capital intensive industries (Lindquist et al., 2018), and better outcomes in terms of revenue (Berglann et al., 2011; Levine and Rubinstein, 2017; van Praag and Raknerud, 2017; Halvarsson et al., 2018), job creation (Åstebro and Tåg, 2017), patenting (Levine and Rubinstein, 2017), and exit (Guzman and Stern, 2016). This distinction allows me to refine theoretical expectations and to provide much-needed nuance to the analysis relative to previous studies.

Using a fixed effects approach to address the confounding effects of family size, I find positive birth order effects, i.e. later born children are more likely to become entrepreneurs, without distinguishing by type (Han and Greene, 2016; Black et al., 2018). These effects are driven by

men's unincorporated entrepreneurship and I find no birth order effects for women or incorporation. Moreover, men's positive birth order effects are mediated by education, pointing towards selection into unincorporated firms from the lower tail of the ability distribution (Åstebro et al., 2011; Andersson-Joona and Wadensjö, 2013; Levine and Rubinstein, 2018). The empirical distinction between unincorporated and incorporated firms thus reinforces the conceptualized dichotomy between subsistence and transformational entrepreneurship (Schoar, 2010).

Turning to family size, to overcome the endogeneity of parental fertility decisions, I use multiple births and sibling sex composition as instruments for family size in linear and non-linear instrumental variable (IV) models (Black et al., 2005; Angrist et al., 2010; Mogstad and Wiswall, 2016). Intuitively, the arrival of twins or triplets and the presence of children of different genders may push families above their desired fertility in a manner exogenous to entrepreneurship. While OLS regressions show evidence of the hypothesized positive (negative) family size effects for unincorporated (incorporated) entrepreneurship, IV estimates are mostly insignificant. Family size only acts a barrier to incorporation in sibships with more than five children, where human capital and financial constraints are likely to become binding.

When I analyze sibling sex composition effects in the sample of complete families, the presence of a brother or sister does not affect entrepreneurship. However, this analysis may be confounded by parental preferences over children's gender. In a more causal approach, I examine the outcomes of first born children as a function of the next child's gender, assuming its quasi-exogeneity conditional on the family's fertility decision (Cools and Patacchini, 2019; Peter et al., 2018). Sibling sex composition effects are again absent. In a recent paper, Mishkin (2017) argues that daughters of self-employed fathers are less likely to become self-employed in the presence of a brother, a result I replicate for unincorporated fathers and daughters, but is weaker in Sweden than in the United States. However, incorporated fathers (and entrepreneur mothers) may increase the likelihood of daughters' entrepreneurship when a brother is present, with countervailing effects.

What is the quantitative implication of these findings? By calculating sibling correlations, Lindquist et al. (2018) show that up to half the variation in incorporation in Sweden is due to shared family and community background. However, this measure does not include sibling differences and may be understated (Conley, 2004). I therefore produce revised measures of the importance of family, accounting for birth order, family size, and sibling sex composition effects (Björklund and Jäntti, 2012). The revised estimates are at most 1.2-2 percentage points (3.2%-6.7%) larger than the original sibling correlations, suggesting that within-family sibling differences are not as important as one might expect them to be.

I make several contributions to the entrepreneurship literature in this paper. First, I systematically assess a set of determinants of entrepreneurship that have received limited attention: birth order, family size, and sibling sex composition have small, but theoretically consistent causal effects for entrepreneurship. Most notably, if later born children are more entrepreneurial, it is not necessarily because they are creative or disruptive, but because they have lower ability and education. Second, by focusing on within-household differences, I complement research on the importance of shared family traits for entrepreneurship, and show in a unified framework that sibling correlations do not understate the importance of family background by much. Third, I offer evidence on the long-run occupational choice effects of factors known to affect education and earnings. On average, these factors do not impede Swedish individuals' ability to become growth oriented, incorporated entrepreneurs, but may push them into unincorporated self-employment if they fare worse in the labor market. Finally, I add to the evidence that unincorporated self-employment is broadly equivalent to subsistence entrepreneurship, and unlikely to be the driver of employment and growth that policy aims to foster.

The paper proceeds as follows. I describe the data in Section 3.2. The next sections describe how each of the three mechanisms may generate sibling differences, lay out the theoretical expectations and empirical approach, and present results. I analyze the differential effects of birth

order in Section 3.3, family size in Section 3.4, and sibling gender in Section 3.5. In Section 3.6 I discuss the implications of within-family differences in entrepreneurship for sibling correlations. Section 3.7 discusses the results and concludes.

3.2 Data

To analyze the role of sibling differences, I use Swedish administrative data. The Swedish Multigenerational Register includes all individuals born after 1932 who lived in Sweden at any time after 1961, and contains unique personal identification numbers that allow me to identify parents and siblings. I begin with a 25% random sample of individuals, then match on all their siblings, as well as information on their parents. Families are defined as all children belonging to the same mother, regardless of whether they are adopted or not. As data on unincorporated (incorporated) entrepreneurship is only available after 1985 (1993), I restrict the sample to individuals born between 1960 and 1970 in order to balance coverage of family composition and labor market histories. This approach yields a 70% coverage of cohorts born in this period and more than 700,000 individuals. However, the largest spacing between siblings in my sample is 10 years, and siblings born outside the interval I cover are not included in the sample.

Despite some sibships not being 'complete', *Family size* and *Birth order* are recorded correctly, counting all the children in the family, regardless of birth year. One potential issue is the presence of multiple births (twins, triplets, quadruplets, quintuplets), which are given consecutive birth orders that may not correspond to the real birth order. For instance, twins with birth orders 1 and 2 may not have been born in the same order; more correctly, they should both be recorded with birth order 1, but this creates ambiguity regarding the birth order of subsequent children. I therefore drop families that include multiple births from the analysis of birth order effects; I also restrict the sample for this analysis to families with two to five children, as singletons cannot contribute information in a fixed effects approach, and there are few families with more than 5 children, offering limited statistical power.

Whereas the section on family size effects includes all children, the analysis of sibling gender effects requires an accurate record of the presence of brothers and sisters. Hence, I focus on the sample of complete families with at least two children, with more than 230,000 individuals (split roughly equally between men and women), larger than most other studies of sibling gender effects.[4] I create a set of indicator variables to capture sibling sex composition: *Sister*, *Brother*, *Younger sister*, *Older sister*, *Younger brother*, and *Older brother* code the presence of any such sibling as 1. I also create variables for *Number of brothers* and *Number of sisters*, as well as *Percent sisters*, the latter counting the share of women in the sibship.

Following the Swedish tax authority, I define entrepreneurs as individuals who derive the majority of their taxable labor income from a partly or fully owned business, and consider unincorporated and incorporated firm ownership separately. The legal distinction between firm types is that the latter represents a privately owned, non-listed, limited liability stock company, allowing founders to pursue riskier investments. In practice, incorporated enterprises have higher ability founders and better outcomes (Berglann et al., 2011; Andersson-Joona and Wadensjö, 2013; Guzman and Stern, 2016; Åstebro and Tåg, 2017; Levine and Rubinstein, 2017; Humphries, 2017; van Praag and Raknerud, 2017; Halvarsson et al., 2018). As a result, incorporation is considered a proxy for growth orientation (Henrekson and Sanandaji, 2014, 2019; Tåg et al., 2016; Lindquist et al., 2018; Levine and Rubinstein, 2018). With this distinction in mind, I focus on individuals' long run propensity to become entrepreneurs, and define them as *Unincorporated* or *Incorporated* entrepreneurs if they are recorded as such at any point in the years when the data is available (1985/1993-2012). For comparisons with Black et al. (2018), I define an individual as an *Entrepreneur* if they have ever been unincorporated or incorporated.[5]

[4] The results hold when I restrict the sample to complete families. In addition, sibling correlations for this sample are the same as those in the larger sample (Lindquist et al., 2018).

[5] Results (available upon request) are similar if I i) measure entrepreneurship between ages 25 and 40 for consistency across cohorts, ii) measure entrepreneurship from 1993 onwards for both unincorporated and incorporated business ownership, or iii) use a stricter definition, focusing on those who are entrepreneurs for more than the median number of years, potentially in different spells – unfortunately, I do not have access to firm-level data.

To understand the channels that generate birth order effects, I assess the mediating role of education, cognitive, and noncognitive skills. The data record the number of years of completed schooling (seven levels, from the old minimum of 7 years, to 12 years for high school, and a maximum of 19 years for a PhD). Cognitive and noncognitive ability data come from military draft records: at age 18, Swedish male citizens serve a mandatory military stage. At entry, they are administered a battery of tests, and their logical, verbal, spatial, and technical skills are scored on a 1 to 9 scale. In addition, a psychologist conducts a structured interview with each individual, scoring their leadership skills on a 1 to 9 scale. These scores are consequential, as they are used to sort young men into officer training programs (Lindqvist and Vestman, 2011) and have been shown to affect entrepreneurship (Aldén et al., 2017; Humphries, 2017).

Throughout the analyses, in addition to dummies for individual and parents' year of birth, I control for i) mother's age at first birth, ii) parental immigration status, iii) parental entrepreneurship, iv) parental education, and v) parental income, the log of the sum of mother's and father's average pre-tax total factor income for all available years between 1968 and 2012. This measure captures labor earnings and returns on capital, is strongly associated with wealth (Lefgren et al., 2012), and is introduced as a set of dummies for distribution deciles, the top five and top one percent. These controls are mainly used for improving precision, as results are similar with and without them. Since the sample for each causal analysis is slightly different, I present descriptive statistics for each sample in the relevant section. Overall, around 14.5% of individuals are *Unincorporated*, and around 8.5% *Incorporated*. Men are always more entrepreneurial than women, and the same is true for parents.

3.3 Birth Order and Entrepreneurship

Differences between siblings may emerge through birth order, which affects parental preferences and strategies, children's bid for parents' attention, and sibling interactions. On the one hand, parents invest more temporal and financial resources in the first born, but allow more freedom to

later borns (Price, 2008; Pavan, 2016; Mechoulan and Wolff, 2015; Hotz and Pantano, 2015). On the other hand, siblings engage in an individuation process, adopting different roles. A popular hypothesis in psychology contends that first borns are more conforming, while later borns are more creative and disruptive (Sulloway, 1996), though Rohrer et al. (2015) find no birth order effects with regards to personality traits. Empirically, birth order effects in education and cognitive ability are negative, with potential consequences for earnings (Black et al., 2005; Lehmann et al., 2018). Due mainly to socialization factors (as opposed to innate traits), later born children have lower noncognitive ability; consequently, they are less likely to attain managerial positions and do not necessarily pursue occupations requiring more creativity (Black et al., 2018).

What does this imply for entrepreneurship? Several competing perspectives exist. If new ventures require higher ability, then earlier born individuals could be more likely to enter, but if entrepreneurship represents only a necessary response to lack of labor market opportunities engendered by low ability (Åstebro et al., 2011; Andersson-Joona and Wadensjö, 2013; Levine and Rubinstein, 2017), then later born children could have higher odds of becoming entrepreneurs. The latter may also be more entrepreneurial if creativity is required (Sulloway, 1996).[6] To disentangle these competing mechanisms, I analyze unincorporated and incorporated business ownership separately. Broadly speaking, incorporated firms operate at larger scale and are more growth oriented than unincorporated firms, thus requiring a higher ability entrepreneur in order to be successful. Clearer predictions then emerge:

Hypothesis 1. ***a)*** *Later born children are more likely to become unincorporated than earlier born children, i.e. there are positive birth order effects in unincorporated entrepreneurship. By contrast,* ***b)*** *later born children are less likely to become incorporated than earlier born children, i.e. there are negative birth order effects in incorporated entrepreneurship.*

[6] Under primogeniture preference, business inheritance may favor first borns (Bloom and van Reenen, 2007). Without firm-level data, I cannot address this possibility; however, children in Scandinavia become entrepreneurs in the same industry and around the same time as their parents exit in at most 8% of cases (Sørensen, 2007b; Lindquist et al., 2015). Birth order and gender are the least important factors in family firm succession in Canada (Chrisman et al., 1998), but they matter in Italy (Calabrò et al., 2018).

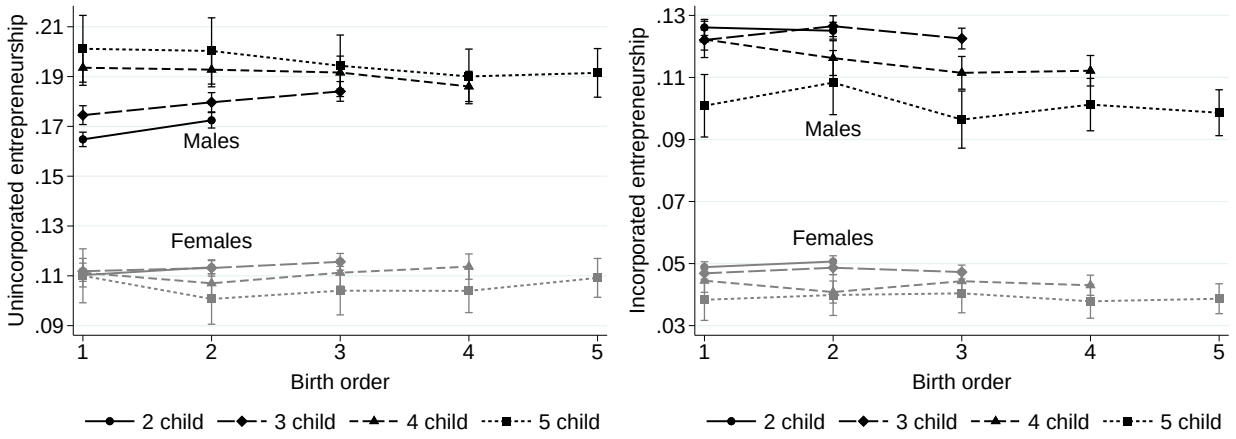

Figure 3.1: Unincorporated (left) and incorporated (right) entrepreneurship, by birth order and family size, with 95% confidence intervals.

Estimates of birth order effects in entrepreneurship are scant. An early study finds no such effects in a small sample of innovative business founders (Robinson and Hunt, 1992). Recently, Han and Greene (2016) use data from the British 1970 cohort study and fixed effects estimations to show that later born children are more likely to be self-employed, which they interpret as evidence for the 'born to rebel' hypothesis (Sulloway, 1996).[7] In Sweden, Black et al. (2018) find evidence of relatively weak positive birth order effects in self-employment; their definition, however, conflates unincorporated and incorporated entrepreneurship, and does not assess sources of heterogeneity or mediating factors. I therefore extend their analysis, adding a novel focus on gender and firm type. I first show the effects visually, then estimate OLS regressions, and finally use a fixed effects approach to account for the confounding effects of family size.

Figure 3.1 plots the raw levels of unincorporated and incorporated entrepreneurship by birth order and family size, separately by gender. While the gender gap in entrepreneurship is striking, these profiles are flat with regards to birth order; yet, later born children are younger and display slightly lower entrepreneurship rates (Appendix Figure B.1.1). While a formal investigation is therefore necessary, the fact that a relationship is not immediately evident – compared with the results for education in Appendix Figure B.1.2 – hints towards a limited role of birth order effects.

[7] Note that around three quarters of the self-employed in this cohort study have no employees.

The sample consists of families with two to five children, excluding families with multiple births due to the birth order ambiguity they create.[8] Appendix Table B.1.1 presents summary statistics, separately by birth order: earlier (later) born children are more likely to be incorporated (unincorporated) entrepreneurs. OLS models controlling for birth year, gender, and family size, are shown in Appendix Table B.1.2. Second and third born children are more likely to be entrepreneurs, but fourth and fifth born children are less likely, driven by positive effects in unincorporated entrepreneurship and negative effects in incorporation for men. When I control for parental characteristics, birth order effects become positive.

As higher birth orders are only present in larger families, the previous specifications control for family size, as well as own and mother's birth year. However, conditioning on these factors generates imbalances in parental characteristics (Black et al., 2018). I then estimate family fixed effects models, allowing me to compare children within families, i.e. keeping parental traits and family size constant. Table 3.1 presents the results, displaying the likelihood of entry for children at the second to fifth birth orders relative to a first born. The positive birth order effects overall are driven mainly by unincorporated entrepreneurship, with a higher entry likelihood for later born men in particular. For instance, fifth born men are 4.26 percentage points more likely than first born men to become unincorporated, relative to a male average of 17.82%. Later born women display a higher, but insignificant, likelihood of becoming unincorporated entrepreneurs.[9]

Heterogeneity and robustness To examine how parental characteristics may affect birth order effects, I split the sample by parental entrepreneurship and income in Appendix Table B.1.4. I find strong birth order effects for unincorporated (and weaker for incorporated) entrepreneurship when parents are not entrepreneurs. Having an entrepreneurial role model may thus dominate the role of birth order in generating sibling differences, and essentially equalize the playing field (Han

[8] Results are robust to their inclusion, as well as the exclusion of families with adopted children.

[9] Appendix Table B.1.3, Panel A, shows that birth order effects are only found outside of female-only families. The results in Table 3.1 are robust to controlling for mother's age at the birth of each individual, potentially correlated with biological factors (Black et al., 2018).

and Greene, 2016). Alternatively, both earlier and later born children of unincorporated, necessity entrepreneurs may wish to avoid this outcome, and therefore refrain from entrepreneurship. The weak birth order effects in incorporation in families with entrepreneur parents also suggest that firm inheritance by the first born is not a large concern in my analysis. Regarding parents' financial resources, birth order effects are stronger for wealthier parents, and become positive and weakly significant for incorporation, hinting that later born children may become entrepreneurs if they have sufficient resources at their disposal.

Two key results emerge from this analysis.[10] First, broadly in line with Hypothesis 1a, birth order effects in unincorporated entrepreneurship are positive and significant for men and positive, but not significant for women. This parallels the findings in Black et al. (2018), although their results for self-employment seem to be driven mainly by unincorporated business ownership. Second, contrary to Hypothesis 1b, birth order effects in incorporated entrepreneurship are largely absent, speaking to the different nature of this type of entrepreneurship.

Mechanisms What could drive birth order effects in entrepreneurship? The majority of birth order studies have focused on outcomes more proximate to childhood or stable traits such as education, cognitive and noncognitive ability, or psychological traits, which may lie on the causal pathway from birth order to entrepreneurship. I now turn to an examination of these mechanisms, focusing on cognitive and noncognitive ability, as well as education.

[10] In additional analyses, I also find positive birth order effects in unincorporated entrepreneurship in non-immigrant families (available upon request). Splitting the sample by number of children produces some evidence that higher birth order children are more likely to become unincorporated entrepreneurs, but this effect becomes negative and weakly significant in large families, pointing towards a non-linear effect of birth order, moderated by family size (Appendix Table B.1.4). The results also hold for linear birth order effects (Appendix Table B.1.5). Birth order has a positive effect on unincorporated entrepreneurship, dampened by family size and entrepreneurial parents, but boosted by wealthier parents. For incorporation, linear birth order is not generally significant in fixed effects specifications, although the parental entrepreneurship and income mechanisms remain significant. I also follow Black et al. (2018) in estimating birth order effects within same gender siblings; for this exercise I restrict the sample to complete families in order to retain the correct within-gender order (Appendix Table B.1.6). The results show that order within the entire family, rather than within siblings of the same gender, drives birth order effects, and also suggests a limited role of sibling sex composition, which I later return to. Finally, subject to the caveat of smaller samples and noisier estimates, I find virtually no birth order effects for proxies of performance, i.e. years in entrepreneurship and entrepreneurial income (available upon request).

Table 3.1: **Birth Order: Family Fixed Effects Models**

	Entrepreneur			Unincorporated			Incorporated		
	(1) All	(2) Male	(3) Female	(4) All	(5) Male	(6) Female	(7) All	(8) Male	(9) Female
2nd born	0.0054**	0.0056	0.0049	0.0053**	0.0065	0.0043	0.0015	-0.0033	0.0026
	(0.0023)	(0.0044)	(0.0039)	(0.0020)	(0.0040)	(0.0036)	(0.0016)	(0.0033)	(0.0023)
3nd born	0.0098**	0.0047	0.0119	0.0103***	0.0095	0.0099	0.0008	-0.0101	0.0060
	(0.0043)	(0.0082)	(0.0073)	(0.0039)	(0.0074)	(0.0066)	(0.0030)	(0.0062)	(0.0043)
4th born	0.0119*	0.0093	0.0095	0.0121**	0.0145	0.0107	0.0020	-0.0096	0.0046
	(0.0065)	(0.0124)	(0.0109)	(0.0059)	(0.0112)	(0.0099)	(0.0045)	(0.0092)	(0.0066)
5th born	0.0300***	0.0346*	0.0180	0.0291***	0.0426**	0.0187	0.0082	-0.0049	0.0071
	(0.0098)	(0.0188)	(0.0164)	(0.0089)	(0.0170)	(0.0149)	(0.0067)	(0.0135)	(0.0098)
N	603,616	309,706	293,910	603,616	309,706	293,910	603,616	309,706	293,910

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. All models control for birth year dummies, and gender. First born children represent the baseline category.

Table 3.2: **Birth Order Mechanisms: Education, Cognitive, and Noncognitive Ability**

	All children				Male					
	Unincorporated		Incorporated		Unincorporated			Incorporated		
	(1)	(2) Education	(3)	(4) Education	(5)	(6) Cognitive	(7) Noncog.	(8)	(9) Cognitive	(10) Noncog.
2nd born	0.0053***	0.0029	0.0015	0.0003	0.0067*	0.0093*	0.0109	-0.0038	-0.0066	-0.0093
	(0.0020)	(0.0021)	(0.0016)	(0.0016)	(0.0040)	(0.0054)	(0.0068)	(0.0033)	(0.0046)	(0.0062)
3rd born	0.0103***	0.0060	0.0008	-0.0015	0.0102	0.0177*	0.0174	-0.0107*	-0.0178**	-0.0142
	(0.0039)	(0.0039)	(0.0030)	(0.0030)	(0.0074)	(0.0102)	(0.0134)	(0.0062)	(0.0085)	(0.0120)
4th born	0.0121**	0.0071	0.0020	-0.0012	0.0155	0.0270*	0.0295	-0.0106	-0.0203	-0.0183
	(0.0059)	(0.0060)	(0.0045)	(0.0046)	(0.0113)	(0.0154)	(0.0212)	(0.0092)	(0.0127)	(0.0187)
5th born	0.0291***	0.0227***	0.0082	0.0045	0.0436**	0.0791***	0.0687**	-0.0060	-0.0110	-0.0061
	(0.0089)	(0.0091)	(0.0067)	(0.0068)	(0.0171)	(0.0235)	(0.0335)	(0.0136)	(0.0189)	(0.0289)
N	603,616	595,710	603,616	595,710	309,219	227,803	169,065	309,219	227,803	169,065

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. All models control for birth year and gender and include fixed effects. First born children represent the baseline category. Coefficients on education levels, cognitive and noncognitive ability (all introduced as dummies) not reported for brevity. Columns (5) and (8) restrict the sample to individuals with at least one non-missing ability score, although some scores on cognitive and noncognitive ability are missing (results are similar when I restrict the sample to individuals with non-missing data, though sample size is smaller and estimates less precise). College completion has a strong negative effect on unincorporated entrepreneurship, while years of schooling have an inverse U-shaped relationship with incorporation, with the lowest rates of incorporation for those with the minimum 7-year education and those with doctoral degrees. Spatial ability is strongly associated with unincorporated entrepreneurship, and cognitive ability is strongly associated with incorporated entrepreneurship.

In columns (1)-(4) of Table 3.2 I focus on the mediating role of education and ability, controlling for them in family fixed effects models. Individual education explains a large share of birth order effects in unincorporated entrepreneurship: later born children are more likely to become unincorporated entrepreneurs as a result of their poorer educational outcomes (Appendix Figure B.1.2 and Table B.1.7). Unincorporated entrepreneurship thus corresponds to a lack of labor market opportunities for lower ability individuals (Åstebro et al., 2011; Andersson-Joona and Wadensjö, 2013; Levine and Rubinstein, 2017).[11] For incorporation, education does not alter the general picture of insignificant birth order effects. Results are similar when I perform this exercise separately by gender in Panel B of Appendix Table B.1.3.

To investigate the role of cognitive and noncognitive ability, I use data on men's logical, verbal, spatial, technical, and leadership skills (measured on a 1-9, stanine scale). Black et al. (2018) show positive birth order effects in noncognitive skills, which positively affect incorporation (Lindquist et al., 2018). Indeed, I find very strong negative birth order effects in these outcomes in Appendix Table B.1.7. For this mediation exercise, I introduce test scores as a set of dummies in columns (5)-(10) of Table 3.2. For incorporation, cognitive and noncognitive skills are positive determinants, but reveal little with regards to birth order. For unincorporated entrepreneurship, however, controlling for ability boosts the effects of birth order, driven mainly by a positive effect of spatial ability and a negative effect of technical ability and leadership skills. Overall, education has the largest impact, though controlling for cognitive and noncognitive ability may reveal slightly stronger birth order effects for fifth born men.

I conclude that positive birth order effects exist only for unincorporated entrepreneurship, and are small, limited to males, and partly mediated by educational achievement. A later born child is around 4.3% more likely to become unincorporated, on a baseline of 17.8%: while this effect is sizable, it is much smaller for most birth orders, and since fifth born children are a small fraction

[11] Reassuringly, later born children are less likely to report November employment and more likely to have zero earnings or earnings less than 25% and 50% of the median earnings of paid employees in a given year between the ages of 25 and 40, reflecting low labor market attachment (results available upon request).

of the population, it can be considered negligible. Finally, compared to more proximate outcomes that display strong negative patterns, birth order effects in entrepreneurship are remarkably weak, both economically and statistically.

3.4 Family Size and Entrepreneurship

While entrepreneurship displays a rather stable pattern regarding birth order, family size may differentially affect male and female entrepreneurship (Figure 3.1), requiring a formal investigation. The prediction that family size affects child ability as parents spread resources across more children has received mixed empirical support. Some studies find no effects on cognitive and non-cognitive ability and educational attainment (Kessler, 1991; Black et al., 2005; Conley and Glauber, 2006; Angrist et al., 2010), and others find negative effects (Cáceres-Delpiano, 2006; Black et al., 2010; Mogstad and Wiswall, 2016; Bagger et al., 2018; Fletcher and Kim, 2018), especially in constrained families (Åslund and Grönqvist, 2010; Lafortune and Lee, 2014). Consequently, larger families may impede individual human capital accumulation, preventing opportunity entrepreneurship, but promoting necessity entrepreneurship. Moreover, in starting a more capital intensive, incorporated firm, children in large families are less able to rely on parental financial resources, which are strong determinants of entry (Blanchflower and Oswald, 1998; Dunn and Holtz-Eakin, 2000), especially for women (Lindquist et al., 2018).[12] Theoretically, we expect:

Hypothesis 2. ***a)*** *Children in larger families are more likely to become unincorporated entrepreneurs than children in smaller families. By contrast,* ***b)*** *children in larger families are less likely to become incorporated entrepreneurs than children in smaller families.*

While no studies have specifically examined the role of family size for entrepreneurship, many control for it in occupational choice equations. The effects of family size are absent in the U.S. National Longitudinal Survey (Dunn and Holtz-Eakin, 2000; Hundley, 2006), but negative in the

[12] While one could argue that increased sibling interaction in larger families may foster social skills or leadership, this hypothesis is not supported by previous research (Fletcher and Kim, 2018).

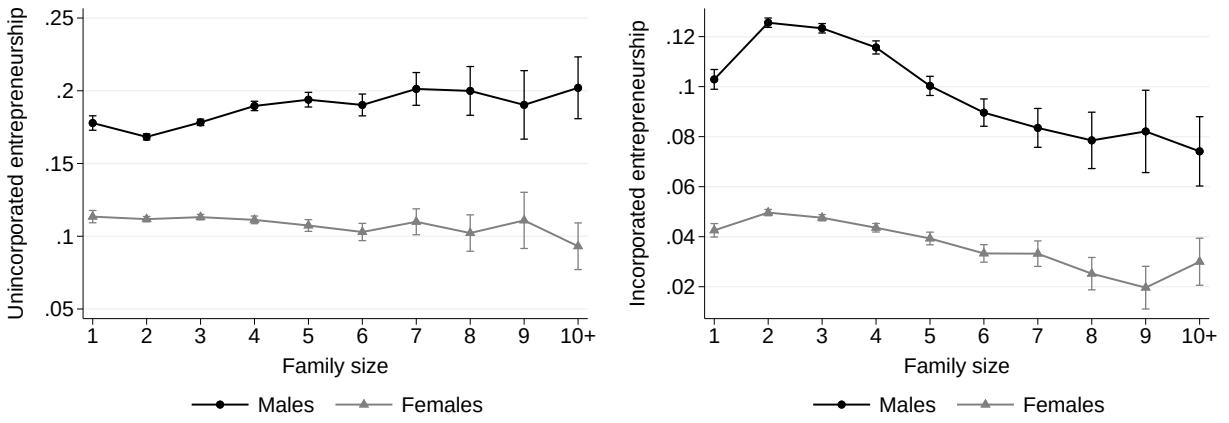

Figure 3.2: Unincorporated (left) and incorporated (right) entrepreneurship, by family size, with 95% confidence intervals.

General Social Survey (Hout and Rosen, 2000; Hundley, 2006) and for children of the self-employed in Finland (Niittykangas and Tervo, 2005; Tervo, 2006); in Sweden, Lindquist et al. (2018) find positive (negative) non-causal effects on unincorporated (incorporated) entrepreneurship. Appendix Table B.1.8 presents the descriptives for the sample, disaggregated by family size. Unincorporated entrepreneurship increases with family size, while incorporation first increases, then decreases with family size, as visible in Figure 3.2. However, parental characteristics also vary with family size, raising concerns about confounders.

Empirically, I first estimate OLS models, then address the endogeneity of family size in instrumental variable approaches (Black et al., 2005). Appendix Tables B.1.9 to B.1.12 estimate the role of family size for entrepreneurship in OLS models, with i) quadratic functions and dummies, ii) different sets of control variables, including birth order, iii) by gender, and iv) by parental income and entrepreneurship. Together, they suggest largely positive effects of family size on unincorporated entrepreneurship and negative effects for incorporation, though the estimates are sensitive to the inclusion of different controls and relatively noisy.

Parents' fertility decision, nonetheless, is likely endogenous. For instance, lower ability parents may have lower ability, potentially less entrepreneurial children, but may also have more children: family size effects are then confounded by unobservables (Kessler, 1991; Black et al., 2005; Angrist

Table 3.3: **Family Size, Instrumental Variable Approach**

	Family size	Unincorporated		Incorporated	
	(1) 1^{st} Stage	(2) OLS	(3) IV	(4) OLS	(5) IV
A. Instrument: multiple second birth; sample: 1st born, 2+ children					
Family size		0.0058***	-0.0034	-0.0038***	0.0035
		(0.0008)	(0.0122)	(0.0006)	(0.0100)
Multiple birth	0.7281***				
	(0.0183)				
F-statistic	1,584.99				
N	224,345				
B. Instrument: multiple third birth; sample: 1st-2nd born, 3+ children					
Family size		0.0054***	0.0118	-0.0046***	0.0151
		(0.0010)	(0.0130)	(0.0007)	(0.0107)
2nd born		0.0007	-0.0002	0.0029**	0.0002
		(0.0016)	(0.0024)	(0.0013)	(0.0019)
Multiple birth	0.8538***				
	(0.0294)				
F-statistic	845.38				
N	213,838				
C. Instrument: multiple fourth birth; sample: 1st-3rd born, 4+ children					
Family size		0.0039***	0.0128	-0.0045***	-0.0298**
		(0.0014)	(0.0188)	(0.0009)	(0.0118)
2nd born		-0.0069**	-0.0077**	-0.0017	0.0006
		(0.0028)	(0.0033)	(0.0021)	(0.0024)
3rd born		-0.0116***	-0.0135***	-0.0039	0.0017
		(0.0033)	(0.0053)	(0.0025)	(0.0035)
Multiple birth	0.9179***				
	(0.0622)				
F-statistic	217.80				
N	105,958				

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses. In Panel A, standard errors are heteroskedasticity robust; in Panels B and C, standard errors are clustered at the family level, and I report the Kleibergen-Paap F-statistics, adjusting for clustering. All models control for own and mother's birth year, gender, and birth order. Other controls include father's birth year, and mother's and father's education level. Multiple births include twins, triplets, quadruplets, and quintuplets.

et al., 2010). The search for exogenous variation in family size has resulted in the use of multiple births as instruments: the birth of twins, triplets, etc. may push parents beyond their desired fertility, allowing family size to be exogenous for previously born children (Black et al., 2005; Bagger et al., 2018). I create instruments based on a multiple birth taking place at order t to analyze the outcomes of children born at orders 1, 2, ..., $t-1$.[13] This approach only exploits exogenous

[13] Twins with birth orders 3 and 4 a multiple birth at both 3rd and 4th birth orders, such that i) the first stage

family size variations, and leads to large and positive first stage estimates, with Kleibergen-Paap F-statistics above 200.[14]

The two-stage least squares IV estimates for these samples are shown in Table 3.3.[15] While OLS estimates suggest a positive effect of family size on unincorporated business ownership, IV estimates are not significantly different from zero. For incorporation, OLS estimates are negative, whereas most IV estimates are insignificant, with the exception of families with at least 4 children. As the latter represent less than 2.5% of families, family size is unlikely to constitute an obstacle to the pursuit of entrepreneurship for a large fraction of the population. In addition, I repeat the exercise separately for men and women, where differential effects may exist if parents allocate resources unequally as a function of gender. As Table 3.4 shows, results are very similar, although the negative effect of large families is only present for men.

Robustness checks Alternatively, I use the first two siblings' gender as an instrument for family size, as parents with a preference for gender diversity will have more children (Angrist and Evans, 1998). Indeed, this is characteristic of the Swedish context (Andersson et al., 2006). To ensure correctly computed instruments, I restrict the sample to complete families and focus on children with birth orders 1 and 2.[16] The first stages are strong, with $F > 125$: same gender pairs lead to larger families, irrespective of gender. The OLS and IV results in Panels A and B of Appendix Table B.1.13 display no evidence of causal family size effects. While this instrument analyzes a different complier population (Angrist et al., 2010), exogeneity may be violated if child gender

effect on family size is understated, and ii) the instrumented effects of family size may be affected by twin interactions or other effects. I therefore only define a multiple birth as taking place at birth order t if no multiple birth has occurred at orders 1, 2, ..., $t-1$. In principle, a family could comprise several multiple births; I only use the first one for any given mother to avoid ambiguities. Bhalotra and Clarke (2018) show that twin births may be correlated with maternal characteristics. To alleviate this concern, I control for mother's age, education, and income as correlates of maternal health and later on report results using a different set of instruments.

[14] I report Kleibergen-Paap F-statistics, adjusting for the family clusters (Cameron and Miller, 2015). Olea and Pflueger (2013) effective F-statistics, correcting for potential heteroskedasticity, are very similar.

[15] Results from instrumental variable models for binary dependent variables (Newey, 1987, 1990) are qualitatively similar, but their magnitude is more difficult to interpret (available upon request).

[16] IV results are similar when i) parents live together, ii) parental income is below/above median, iii) the parents are entrepreneurs or not, and iv) I restrict the sample to complete families (available upon request).

mix affects entrepreneurship through other channels than family size; these results should thus be taken with caution (Black et al., 2005).[17]

In order to gain more precise estimates, I also use the method proposed by Angrist et al. (2010) for combining multiple birth and sex composition instruments (Panels C-E of Appendix Table B.1.13). This approach produces weak evidence for family size effects, a reassuring result given the generally smaller standard errors it produces. Nonetheless, the linear family size effects assumption may be too strong: for instance, family size shifts the likelihood of primogeniture in family firm successions (Calabrò et al., 2018). To estimate the effects of family size at different birth orders, I estimate non-linear instrumental variable models (Mogstad and Wiswall, 2016), as described in Appendix B.2. I find no significant marginal family size effects for unincorporated entrepreneurship, and negative marginal family size effects for incorporated entrepreneurship in families with more than five children (Appendix Table B.2.1). While very large families effectively deny incorporation to children (large decreases relative to the mean), given the small fraction of such families where human capital and financial constraints become binding, these effects need not yield substantial explanatory power.

Overall, the weight of the evidence pushes against large causal family size effects in entrepreneurship, contrary to Hypotheses 2a and 2b. In this setting, IV estimation – using several different instruments, as well as non-linear approaches – does point towards substantial endogeneity and a bias in OLS estimates.[18] Nonetheless, large families do hinder entrepreneurship, and I later attempt to assess the contribution of family size effects to sibling correlations. I now turn to the final causal exercise, where I relate sibling gender to entrepreneurship outcomes.

[17] Booth and Kee (2009) propose breaking the mechanical relationship with family size ($\rho \approx 0.69$) by using a birth order index, calculated as the ratio between an individual's absolute birth order and the average birth order in their family ($\rho \approx 0.12$). This approach yields some significant effects, but with an extremely small magnitude, and effectively negligible (Appendix Table B.1.14). Note that this alternative estimation fails to account for the endogeneity of family size, so the estimates in Table 3.3 remain the preferred ones.

[18] The main IV approach relies on multiple births as an exogenous source of variation in family size; however, multiple births may also change the way in which parents allocate attention to children and invest in their human capital. As a result, the IV estimates may only apply to a particular subsample of the population and may capture a wide array of changes induced by higher family size; to alleviate these concerns, I used a set of different instruments and approaches, whose results are internally consistent despite relying on different assumptions.

Table 3.4: **Family Size: Instrumental Variable Approach, by Gender**

	Male				Female			
	Unincorporated		Incorporated		Unincorporated		Incorporated	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
A. Instrument: multiple second birth; sample: 1st born, 2+ children								
Family size	0.0111***	-0.0091	-0.0052***	-0.0119	0.0001	0.0018	-0.0023***	0.0183
	(0.0013)	(0.0191)	(0.0010)	(0.0163)	(0.0011)	(0.0154)	(0.0007)	(0.0118)
N	115,217				109,115			
B. Instrument: multiple third birth; sample: 1st-2nd born, 3+ children								
Family size	0.0104***	0.0354*	-0.0060***	0.0134	0.0000	-0.0125	-0.0030***	0.0169
	(0.0015)	(0.0206)	(0.0012)	(0.0171)	(0.0012)	(0.0150)	(0.0008)	(0.0121)
N	110,089				103,759			
C. Instrument: multiple fourth birth; sample: 1st-3rd born, 4+ children								
Family size	0.0057***	0.0154	-0.0068***	-0.0592***	0.0018	0.0078	-0.0021**	0.0097
	(0.0021)	(0.0266)	(0.0016)	(0.0152)	(0.0016)	(0.0228)	(0.0009)	(0.0155)
N	54,155				51,803			

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses. In Panel A, standard errors are heteroskedasticity robust; in Panels B and C, standard errors are clustered at the family level. All models control for own and mother's birth year, and gender. IV models also control for birth order (not reported for brevity). Other controls include father's birth year, and mother's and father's education level (further controls for income or mother's age at first birth do not affect the results). Multiples include twins, triplets, quadruplets, and quintuplets. The first stage F-statistics for men and, respectively, women are 939.13 and 692.22 in Panel A, 591.46 and 561.10 in Panel B, and 152.85 and 137.43 in Panel C; in Panels B and C, these are Paap-Kleibergen F-statistics, adjusting for clustering (Cameron and Miller, 2015).

3.5 Sibling Sex Composition and Entrepreneurship

The last within-household difference I consider refers to sibling sex composition: I assess whether growing up with a brother or sister affects men's and women's entrepreneurship. Sibling gender may affect both intra-household resource allocation and the nature and quality of sibling interactions. Parental preference for sons (Bennedsen et al., 2007; Dahl and Moretti, 2008) may reduce investments in women, lowering their educational attainment; by contrast, growing up with a brother may raise parents' expectations of women's achievement (Butcher and Case, 1994). Opposite gender children may also influence parenting styles, reinforcing traditional gender roles (Brenøe, 2018) and reducing women's competitiveness (Niederle and Vesterlund, 2007); however, the latter may increase when growing up with a brother, as sibling rivalry is exacerbated (McHale et al., 2012). Given that entrepreneurship is a stereotypically male occupation, requiring human capital and competitiveness, sibling sex composition may be an important determinant.

So far, empirical results have been inconsistent. Butcher and Case (1994) find that a brother improves women's educational achievement, though Kaestner (1997) and Hauser and Kuo (1998) dispute this. Cyron et al. (2017) find a positive effect of sisters on men's cognitive ability, Cools and Patacchini (2019) find lower earnings for women with a brother, while Rao and Chatterjee (2018) find positive earnings effects for men with a brother. Using twins, Bhai (2016) finds that having a brother raises women's high school completion rates and earnings, whereas Peter et al. (2018) find higher earnings for men with a brother and women with a sister. Finally, women with a brother are more likely to pursue a science and technology (STEM) education when their father works in a STEM occupation (Oguzoglu and Ozbeklik, 2016), but may be less likely to work in STEM themselves (Brenøe, 2018).

One could speculate on the implications of these findings for entrepreneurship. If women with brothers are more competitive and acquire more education, they may be more likely to become incorporated; by contrast, if traditional roles are reinforced or women obtain lower education in

the presence of brothers, they may be more (less) likely to become unincorporated (incorporated) entrepreneurs. The existence and nature of sibling sex composition effects in entrepreneurship appear to be an empirical matter and evidence so far is scant. This is surprising in light of the attention paid to the gender gap in entrepreneurship, its drivers, and potential solutions. One exception is Mishkin (2017), who finds that women with a brother are less likely to become self-employed when the father is self-employed. I discuss this mechanism later and now turn to a formal investigation of pure sibling sex composition effects. While I also analyze men's outcomes, the main focus is on women's entrepreneurship, with the baseline expectation that:

Hypothesis 3. *Women growing up with a brother are **a)** more likely to become unincorporated entrepreneurs, but **b)** less likely to become incorporated entrepreneurs.*

The literature on sibling gender effects has not converged on either the appropriate theory or the preferred empirical specification (Butcher and Case, 1994; Hauser and Kuo, 1998; Cools and Patacchini, 2019). I therefore provide a set of different specifications and sample cuts. These exercises rely on a large set of controls for family background to alleviate the endogeneity of child gender preferences. However, they all point towards a limited role of sibling sex composition effects in entrepreneurship. In Figures 3.3 and 3.4, no clear relationship between number of brothers or sisters and entrepreneurship entry emerges for men or women: most rates of entrepreneurship in the raw data are overlapping (separately by gender).

To formally analyze the role of sibling gender, I restrict the sample to complete families with at least two children in order to get an accurate picture of the number of men and women. Descriptive statistics for this sample are shown in Appendix Table B.1.15. In Appendix Tables B.1.16 to B.1.18 I use indicators for the presence and number of same or opposite gender sibling, the percent of sisters in the family, and the presence of younger or older siblings. In these models, I control for individual and parental birth year, parental education, immigration, and entrepreneurship, family structure at age 15, and family size. The findings parallel the pattern of mixed results in the

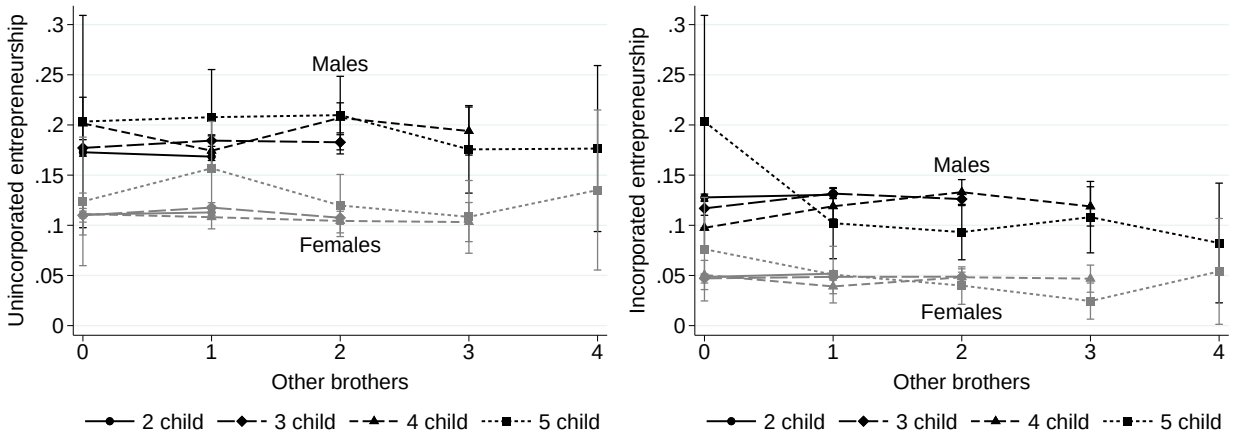

Figure 3.3: Unincorporated (left) and incorporated (right) entrepreneurship, by number of brothers and family size, with 95% confidence intervals.

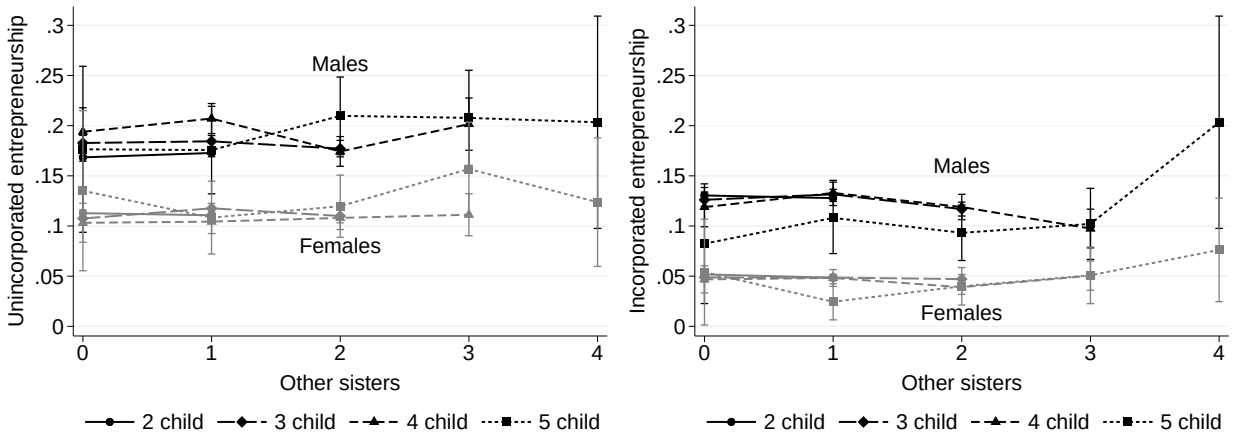

Figure 3.4: Unincorporated (left) and incorporated (right) entrepreneurship, by number of sisters and family size, with 95% confidence intervals.

literature, although the presence of (younger) brothers and sisters has a somewhat positive effect on men's entrepreneurship (of either type) and women's unincorporated entrepreneurship.

Although I control for a large set of background characteristics, sibling gender may reflect unobserved parental preferences potentially related to entrepreneurship. For instance, more risk-averse, less entrepreneurial parents (thus less likely to transmit entrepreneurship to children) may also prefer to have male and female children to diversify old-age risks. Alternatively, fathers may wish to impart entrepreneurial skills to a son, to the detriment of a daughter, such that a negative effect of having a brother on the sister's entrepreneurship is confounded. To get closer to a causal interpretation, I therefore focus on first born children, assuming that the second child's gender is

quasi-exogenous to the first child conditional on parents' decision to have another child (Cools and Patacchini, 2019; Mishkin, 2017; Peter et al., 2018).[19]

Tables 3.5 and 3.6 show the effect of growing up with an opposite gender child on the entrepreneurship outcomes of first born men and women, respectively, controlling for a wide set of demographics. For first born men, there is a small positive effect of having a sister in families of two children, but most of the evidence points towards no significant effect of the next sibling's gender, even controlling for birth spacing as a proxy for sibling interaction intensity (Buckles and Munnich, 2012). For first born women, the next sibling's gender does not affect entry into entrepreneurship, with the exception of the small subsample of families of four children. Overall, evidence for the existence of pure sibling sex composition effects is limited, although I later discuss the interaction of sibling gender and parental entrepreneurship in detail.[20]

Heterogeneity Intra-household resource allocation may depend on parental traits. For instance, parents may favor sons in transferring entrepreneurship relevant human capital or the business in its entirety (Mishkin, 2017). For first born women, Panels A and B of Appendix Table B.1.21 show that having a second born brother reduces women's likelihood of becoming unincorporated entrepreneurs when parents are entrepreneurs in all family sizes, though not always significantly. If parents are not entrepreneurs, the second child's gender does not matter for first born women.

[19] The first born child's gender may influence parents' subsequent fertility decisions: under son preference, the family will continue to have children if the first child is a daughter (Dahl and Moretti, 2008; Jayachandran and Pande, 2017). While this may generate a selection effect on family size, the gender of the second child should be orthogonal to the gender of the first child (Peter et al., 2018). Given that all the individuals in the sample are born before 1970, parental fertility and children's gender are not substantially affected by the introduction of the birth control pill (Black et al., 2005) or sex-selective abortions (Lindquist et al., 2015; Peter et al., 2018), which were introduced only after 1965 and slowly. Appendix Table B.1.19 shows covariate balance for first born children with same- and opposite- sex next siblings; the small differences become insignificant in multivariate analyses. As family size does differ between groups, I control for it in the regressions.

[20] Another approach is to use dizygotic twins, where the co-twin gender is as good as random; this approach delivers similar results to using first born children and the next sibling's gender (Peter et al., 2018). Encouragingly, they note (footnote 31) the absence of sibling gender effects in self-employment using the latter approach. Unfortunately, I do not have access to zygosity indicators in order to perform a twin analysis, and disregarding zygosity may bias the results in unpredictable ways (Peter et al., 2018). I assess the role of sibling gender for all twins (irrespective of zygosity) in Appendix Table B.1.20: having a brother does not affect unincorporated entrepreneurship, but increases both men and women's likelihood of becoming incorporated. These results, however, may be driven by identical twins, such that they provide inconclusive evidence of sibling gender effects.

Table 3.5: **Sibling Sex Composition: First Born Males, by Family Size**

	2 children		3 children		4 children		2+ children	
	(1) Uninc.	(2) Inc.	(3) Uninc.	(4) Inc.	(5) Uninc.	(6) Inc.	(7) Uninc.	(8) Inc.
A. Basic controls								
Next sibling sister	0.0080**	-0.0047	-0.0013	-0.0009	-0.0078	0.0027	0.0043	-0.0036
	(0.0039)	(0.0035)	(0.0072)	(0.0062)	(0.0187)	(0.0160)	(0.0034)	(0.0030)
N	37,623	37,623	12,081	12,081	1,863	1,863	51,864	51,864
B. Basic controls + parental income								
Next sibling sister	0.0090**	-0.0053	-0.0025	-0.0017	-0.0117	-0.0025	0.0047	-0.0043
	(0.0040)	(0.0035)	(0.0073)	(0.0062)	(0.0190)	(0.0162)	(0.0034)	(0.0030)
N	36,927	36,927	11,815	11,815	1,793	1,793	50,819	50,819
C. Basic controls + parental income + spacing interaction								
Next sibling sister	0.0104	-0.0062	0.0027	-0.0065	0.0277	0.0277	0.0046	-0.0044
	(0.0094)	(0.0083)	(0.0171)	(0.0144)	(0.0441)	(0.0378)	(0.0078)	(0.0068)
Spacing	0.0002	0.0001	0.0005	-0.0002	0.0002	0.0011	0.0001	0.0001
	(0.0002)	(0.0001)	(0.0004)	(0.0003)	(0.0011)	(0.0010)	(0.0001)	(0.0001)
Next sibling sister	-0.0000	0.0000	-0.0002	0.0002	-0.0017	-0.0011	0.0000	0.0000
× spacing	(0.0002)	(0.0002)	(0.0005)	(0.0004)	(0.0017)	(0.0015)	(0.0002)	(0.0002)
N	36,927	36,927	11,815	11,815	1,793	1,793	50,819	50,819

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. Basic controls include individual and parental birth year dummies, parental education and entrepreneurship, parental immigration, family structure at age 15, and family size dummies; sibling spacing is measured in months between births. Sample restricted to first born males.

Table 3.6: **Sibling Sex Composition: First Born Females, by Family Size**

	2 children		3 children		4 children		2+ children	
	(1) Uninc.	(2) Inc.	(3) Uninc.	(4) Inc.	(5) Uninc.	(6) Inc.	(7) Uninc.	(8) Inc.
A. Basic controls								
Next sibling brother	0.0005	0.0020	-0.0060	-0.0040	-0.0257*	-0.0125	-0.0025	0.0001
	(0.0034)	(0.0024)	(0.0063)	(0.0043)	(0.0147)	(0.0102)	(0.0029)	(0.0020)
N	35,702	35,702	10,979	10,979	1,736	1,736	48,717	48,717
B. Basic controls + parental income								
Next sibling brother	0.0007	0.0029	-0.0038	-0.0027	-0.0271**	-0.0166	-0.0019	0.0008
	(0.0034)	(0.0024)	(0.0064)	(0.0043)	(0.0152)	(0.0104)	(0.0029)	(0.0020)
N	35,062	35,062	10,724	10,724	1,665	1,665	47,734	47,734
C. Basic controls + parental income + spacing interaction								
Next sibling brother	-0.0025	0.0057	0.0054	0.0098	-0.0209	-0.0597**	-0.0052	0.0020
	(0.0082)	(0.0055)	(0.0150)	(0.0107)	(0.0384)	(0.0268)	(0.0067)	(0.0046)
Spacing	-0.0000	0.0001	0.0007**	0.0006***	0.0007	-0.0001	0.0001	0.0001*
	(0.0001)	(0.0001)	(0.0003)	(0.0003)	(0.0010)	(0.0008)	(0.0001)	(0.0001)
Next sibling brother	0.0001	-0.0001	-0.0003	-0.0004	-0.0003	0.0018	0.0001	-0.0000
× spacing	(0.0002)	(0.0001)	(0.0005)	(0.0003)	(0.0015)	(0.0011)	(0.0002)	(0.0001)
N	35,062	35,062	10,724	10,724	1,665	1,665	47,734	47,734

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. Basic controls include individual and parental birth year dummies, parental education and entrepreneurship, parental immigration, family structure at age 15, and family size dummies; sibling spacing is measured in months between births. Sample restricted to first born females.

In Panels C and D of Appendix Table B.1.21, I split the sample between women whose mother earns at least as much as, or less than the father, as an operationalization of gendered roles, i.e. men are breadwinners and women are homemakers (Bertrand et al., 2015). The presence of a brother may make women more 'feminine' (Brenøe, 2018), thus reducing their entry into entrepreneurship; this effect may be stronger in families with more stereotypical gender roles, e.g. where the mother earns less than the father (Cools and Patacchini, 2019). However, I find that parents' adherence to gendered roles does not reinforce any potential negative effects of a second born brother's presence on first born women's entrepreneurship. Finally, in Panels E and F, I split the sample by parental income below/above median, with similarly insignificant differences.

Chen et al. (2017) argue that an opposite gender sibling has i) a direct effect through differential parental behavior or sibling interactions, and ii) an indirect effect through family size; these effects may cancel each other out, hiding a significant impact. For example, if a first born female's next sibling is male, parents with a preference for sons will have fewer children, thus generating an indirect effect through lower family size. To assess the direction and magnitude of these effects, I use IV models interacting the gender of the next sibling and family size, instrumented by multiple birth at the second birth order and its interaction with the next sibling's gender ($F > 600$). Appendix Table B.1.22 performs this decomposition: both direct and indirect effects are zero and precisely estimated. Overall, I find little evidence of sibling sex composition effects, consistent with a lack of adult sibling peer effects (Lindquist et al., 2018).

Sibling Gender and Intergenerational Associations Using U.S. data from the Panel Study of Income Dynamics, Mishkin (2017) argues that sibling sex composition effects arise only under specific circumstances. She shows that women with a brother are less likely to be self-employed when their father is self-employed, representing an 80% reduction in the father-daughter association in self-employment. In her data, this result is weaker and less significant for father-daughter associations in incorporation. Nonetheless, this effect, as well as the potential for similar effects

for mothers' or male children's entrepreneurship, comes in contrast with my finding of no pure sibling sex composition effects, including in entrepreneurial families.

To assess the source of this discrepancy, I replicate and extend her research by differentiating between individual and parental unincorporated and incorporated entrepreneurship, and examining men's outcomes and mothers' entrepreneurship. Compared to Mishkin (2017), I have a larger sample and data on more than 20 years of individuals' careers, allowing me to examine the long-run entrepreneurial propensity. Column (5) in Panel A of Appendix Table B.1.23 shows a negative effect of having a brother and an unincorporated father on women's self-employment. This effect is reversed for men, for whom a sister amplifies the father-son association in unincorporated entrepreneurship in column (1). Instead, column (8) suggests that having a brother *increases* women's likelihood of incorporation when the father is incorporated, a result robust to controlling for parental traits. In Panel B, the effects are directionally similar, but less statistically significant. However, when the mother is an (unincorporated or incorporated) entrepreneur, having a brother boosts women's incorporation.

Endogenous fertility and child gender decisions limit the ability to claim causality when using all children in families with at least two siblings. I therefore restrict the sample to first born children in complete families and assess the role of the next sibling's gender on the intergenerational transmission of entrepreneurship. Results in Panels C and D largely confirm the previous findings, although father's unincorporated entrepreneurship now increases women's likelihood of incorporation in the presence of a brother. In addition, the presence of a brother no longer moderates the effect of mother's entrepreneurship.

The channel proposed by Mishkin (2017) appears limited to fathers' and individuals' unincorporated entrepreneurship and is more muted in Sweden than in the U.S.: 20-30% compared to 80%. However, maternal entrepreneurship is a stronger influence for women than paternal entrepreneurship (Lindquist et al., 2015), and mothers may even favor daughters when a brother is present,

such that the father-daughter transmission and its relation to sibling gender fades in importance. In addition, incorporated fathers may increase daughters' incorporation when a brother is present, with potentially large welfare implications (van Praag and Raknerud, 2017). Overall, while the presence of brothers weakens the father-daughter association in unincorporated self-employment, it does not hold for incorporation or mother entrepreneurs, with the latter producing potentially countervailing effects. Indeed, the analysis in Panel A of Table B.1.21 barely picks up the effect that Mishkin (2017) proposes.[21] I conclude that sibling sex composition effects play an overall minor role for entrepreneurship. Nonetheless, in the next section I attempt to account for them in a revised measure of the influence of families.

3.6 Sibling Differences and Sibling Correlations

By creating sibling similarities, family and community background can explain 20-30% of variation in entry into unincorporated entrepreneurship and 40-50% for incorporation, as estimated by sibling correlations (Lindquist et al., 2018). These correlations compute the share of variation in a given outcome explained by between-family variation as opposed to within-family variation, and represent a broad measure of the importance of background (Solon, 1999). Intuitively, sibling correlations capture all factors shared by siblings, including genetic endowments, family resources and role models, schools, or neighborhoods. More formally, $\rho = \sigma_a^2/(\sigma_a^2+\sigma_b^2)$, where σ_a^2 is between-family variance and σ_b^2 is within-family variance. These are estimated from a random effects model: $E_{if}^* = \mathbf{X'}_{if}\beta + a_f + b_{if}$, where E is the entrepreneurship outcome of individual i in family f, and X' includes birth year and gender. The sibling correlation ρ, however, does not capture sibling differences generated inside the family, through preferential treatment of children or sibling interactions (Conley, 2004), such that the total effect of background is underestimated and the role of sibling similarities is overestimated. It is important to revise the sibling correlation to account for any sibling differences the family may generate.

[21] Mishkin (2017) also examines whether this relationship is affected by family size and birth order (potential confounders). Reassuringly, she finds no effects of these variables on the coefficients of interest.

Björklund and Jäntti (2012) propose a useful method for recovering the share of individual variation produced by within-family differences. This technique: 1) estimates the random effects regression needed for the sibling correlation (ρ); 2) predicts the individual level residuals (i.e. within-family variation, where shared family characteristics have been parsed out), with variance σ_b^2; 3) regresses these residuals on factors that may generate sibling differences and obtains the R^2; and 4) calculates a revised measure of family influence that adds this explanatory power to the original correlation: $\tilde{\rho} = (\sigma_a^2 + R^2\sigma_b^2)/(\sigma_a^2 + \sigma_b^2)$.

In this paper, I have shown positive birth order effects for men, some negative family size effects in large families, and a differential effect of the presence of an opposite gender sibling when the father is an entrepreneur. I now incorporate these differences in a unified framework, producing a revised measure of family influence, accounting for the two- and three-way interactions of i) birth order, gender, and parental entrepreneurship, ii) gender, the presence of opposite sex siblings, and parental entrepreneurship, and iii) gender, family size, and parental income dummies. In this exercise birth order, family size, and sibling sex composition remain endogenous; as previous sections have shown the causal effects of these factors to be limited compared to OLS specifications, their explanatory power in this exercise is likely an upper bound on their true contribution to the importance of family background.[22]

Table 3.7 shows the results of this exercise, performed on the sample of complete families. Panel A displays sibling correlations for each group and outcome, and Panel B shows the R^2 from the residual regression, used to calculate the revised sibling correlations in Panel C. Columns (1) and (2) in Panel D show that the revised measures are 4.3-5% larger than the original sibling correlations. In columns (3)-(6) I analyze men and women separately with similar results, though differences are smaller for women. I focus on families with both male and female children in

[22] Since the residuals have by definition been parsed of shared family factors, the coefficients on shared parental characteristics (such as income or entrepreneurship) in the residual regressions should be insignificant and precisely estimated. Reassuringly for the validity of this method, this is indeed the case. In other words, the R^2 we obtain accurately captures differential treatment inside the household.

Table 3.7: **Sibling Differences and Sibling Correlations**

	All families		Males (all families)		Females (all families)		Mixed gender families	
	Uninc. (1)	Inc. (2)	Uninc. (3)	Inc. (4)	Uninc. (5)	Inc. (6)	Uninc. (7)	Inc. (8)
	A. Sibling correlations							
ρ	0.2249	0.3425	0.3001	0.4113	0.2306	0.3637	0.1979	0.3126
	(0.0055)	(0.0069)	(0.0091)	(0.0105)	(0.0126)	(0.0173)	(0.0069)	(0.0089)
	B. Individual variation explained by differential treatment							
R^2	0.0146	0.0225	0.0197	0.0329	0.0107	0.0183	0.0165	0.0239
	C. Revised measure of family influence							
$\tilde{\rho}$	0.2363	0.3573	0.3139	0.4307	0.2388	0.3754	0.2111	0.3290
	(0.0055)	(0.0067)	(0.0091)	(0.0101)	(0.0125)	(0.0170)	(0.0068)	(0.0087)
	D. Percentage increase from ρ to $\tilde{\rho}$							
	5.047%	4.320%	4.585%	4.710%	3.583%	3.207%	6.688%	5.249%
N_{ind}	277,410	277,410	142,522	142,522	134,888	134,888	142,910	142,910
N_{fam}	144,939	144,939	103,858	103,858	100,136	100,136	59,055	59,055

All models control for individual birth year and gender. Sample restricted to complete families in order to correctly define sibling gender (results are similar in the full sample). Columns (1) and (2) include all children; columns (3) and (4) consider males; columns (5) and (6) consider females (regardless of siblings' gender); columns (7) and (8) restrict the sample to children in families that include both males and females. I account for two- and three-way interactions of i) gender, parental entrepreneurship (i.e. maternal and paternal unincorporated and incorporated entrepreneurship), and birth order dummies, ii) gender, family size dummies, and parental income dummies (percentiles 10, 20, ..., 90, 95, 99), and iii) gender, the presence of opposite gender siblings, and parental entrepreneurship.

columns (7) and (8), where family-generated differences should be largest due to potential sibling gender effects. Indeed, there is a stronger role for such differences, as the revised measures are 6.7% larger for unincorporated entrepreneurship and 5.3% larger for incorporation.

Thus, when I account for a broad set of sibling differences with regards to birth order, family size, and sibling gender (interacted with gender, parental entrepreneurship and parental income), the latter add at most 7% to the sibling correlation (or 2 percentage points). As sibling differences are small, sibling correlations do not substantially understate the role of family and community background in entrepreneurship, and marginally overstate the role of sibling similarities in driving this explanatory power, as captured by sibling correlations.

3.7 Conclusion

In this paper, I examine the role of three closely related and salient sources of sibling differences generated inside the family for Swedish individuals' propensity to become entrepreneurs. Birth order effects, previously found to increase self-employment (Han and Greene, 2016; Black et al., 2018), are driven by a higher likelihood of later born men to be unincorporated business owners. Despite a popular perception that first borns are over-represented in entrepreneurship, I find no support for this contention. Moreover, the positive birth order effects for men are largely mediated by education: later born men have lower human capital, but a larger likelihood of unincorporated business ownership. I conclude that higher entry into unincorporated entrepreneurship for men is largely explained by poor labor market prospects, pointing towards the necessity aspect of this type of entrepreneurship (Åstebro et al., 2011; Andersson-Joona and Wadensjö, 2013; Levine and Rubinstein, 2018), rather than the widely held view that younger children are 'born to rebel' or more disruptive (Sulloway, 1996).

Beyond contributing to our understanding of birth order effects, these findings reinforce the theorized dichotomy between unincorporated and incorporated firms as different types of entrepreneurship (Henrekson and Sanandaji, 2014, 2019; Levine and Rubinstein, 2018), often taken to represent an (imperfect) approximation of necessity and opportunity entrepreneurship. They also imply that policy makers should take this distinction seriously, with a view towards fostering growth oriented ventures and reducing necessity self-employment, notwithstanding heterogeneity within the latter type of entrepreneurship (Dencker et al., 2019).

While family size is negatively related to entry in OLS regressions, linear and non-linear IV approaches dealing with endogenous fertility decisions show some evidence of negative causal family size effects for incorporation in large families (whereas I find no effects for unincorporated business ownership). This accords with a view of incorporated entrepreneurship as intensive in both human and financial capital, where large families exhibit potentially binding resource constraints

with regards to investments in children's human capital as well as their businesses. However, as developed societies transition to lower fertility rates, family size is unlikely to represent a strong barrier to growth oriented entrepreneurship. For developing countries where large families are still prevalent, family size may nonetheless hinder the entry and growth of ambitious, innovative new ventures.

Growing up with siblings of the opposite sex has been shown to affect individuals' education and income, but does not seem to affect entrepreneurship entry. This result is robust to addressing the endogeneity of family preferences for child gender, suggesting an absence of pure sibling sex composition effects. Nonetheless, sibling gender may affect the transmission of entrepreneurship from parents to children, as daughters with an unincorporated father are less likely to enter unincorporated entrepreneurship when a brother is present. While this effect is smaller in Sweden than in the U.S. (Mishkin, 2017), it suggests that unincorporated self-employment – reliant on manual tasks (Levine and Rubinstein, 2017) and concentrated in male-dominated occupations (Lindquist et al., 2018) – may be characterized by a narrow set of occupation-specific skills that fathers find easier or more desirable to pass on to sons, rather than daughters. By contrast, incorporated fathers *increase* daughters' entry when a brother is present, as do mothers in some specifications. This result reinforces the importance of role models in growth oriented, incorporated business ownership, a necessary element for increasing women's likelihood of entry in an occupation with an established gender gap.

Given the results of the three causal exercises, what is the joint quantitative value of these effects for the total importance of family background? While background explains up to half of the variation in entrepreneurship outcomes (Lindquist et al., 2018), sibling correlations only account for factors that siblings share. By overlooking differences generated inside the household, sibling correlations may understate the true explanatory power of family background (Björklund and Jäntti, 2012). Once I account for these differences in families with both men and women, where they

are expected to have the largest impact, sibling correlations rise by only 1.2-2 percentage points (or 3.2%-6.7%). Therefore, the role of background for entrepreneurship is marginally understated, confirming the importance of families in generating similarities, rather than differences, between children in occupational choice. In other words, the sibling correlations in the previous chapter provide an accurate indication of the total importance of family and community background for entrepreneurship in Sweden.

The results in this paper are important for completing our picture of the importance of family background, by examining potential household-level determinants of entrepreneurship that have not received much attention, as well as the interplay of sibling differences and similarities. While birth order, family size, and sibling sex composition affect ability, education, earnings, and labor market outcomes (Black et al., 2005, 2018; Bagger et al., 2018; Peter et al., 2018), they have small causal effects on one aspect of labor market choices, namely selection into entrepreneurship. Moreover, evidence for other outcomes suggests that the small effects observed for entrepreneurship are not necessarily specific to Sweden. Nonetheless, sibling interactions may influence entrepreneurship in more subtle ways than I am able to pick up. Closer investigation of such interactions and the potential for sibling differences across countries – given substantial variation in primogeniture and child gender norms (Ejrnæs and Pörtner, 2004; Bertrand and Schoar, 2006; Bloom and van Reenen, 2007; Dahl and Moretti, 2008) – may yield additional insight into the role of family background for the different types of entrepreneurship.

Appendix B

B.1 Additional Figures and Tables

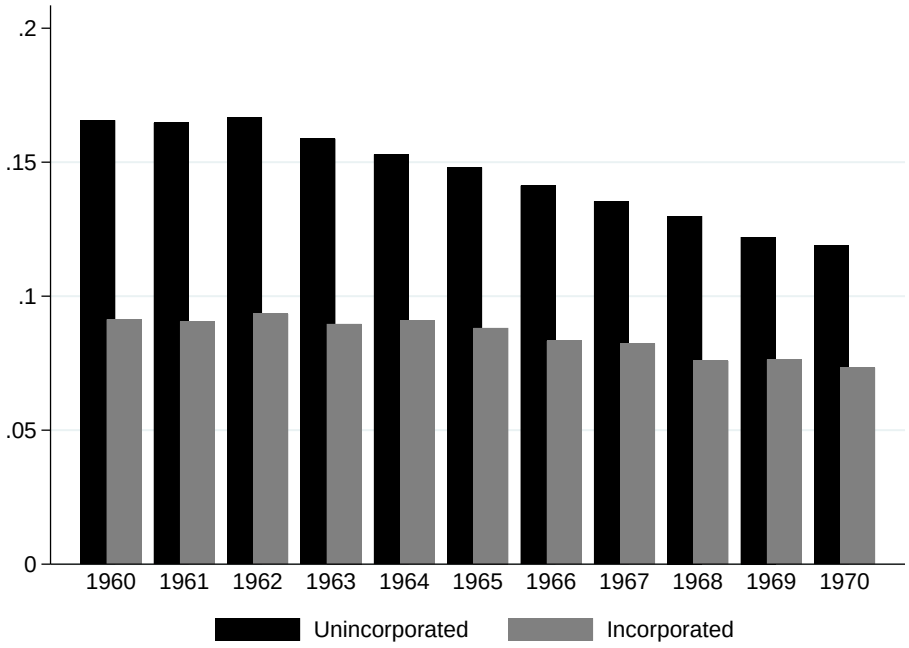

Figure B.1.1: Unincorporated and incorporated entrepreneurship, by birth year.

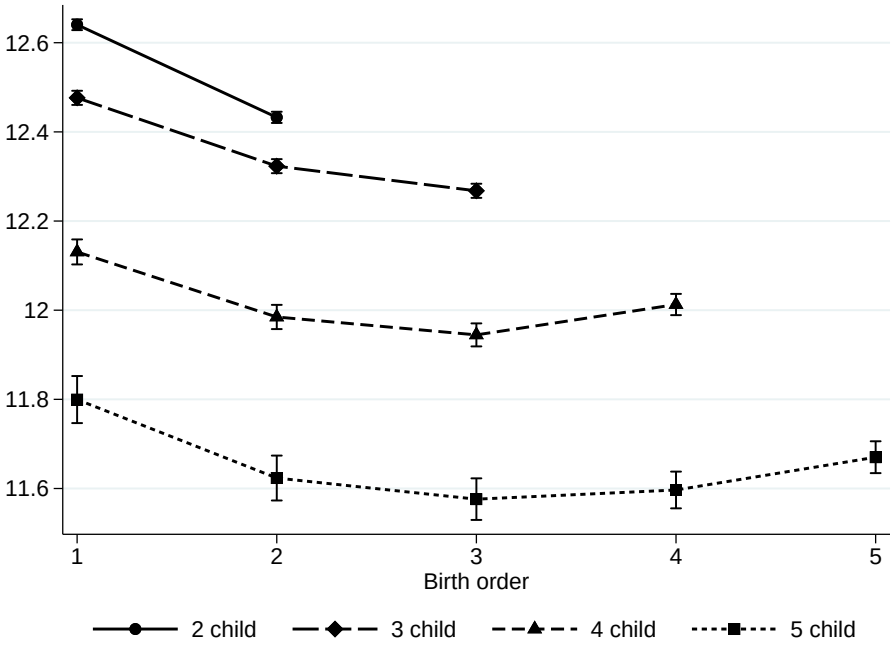

Figure B.1.2: Years of schooling, by birth order and family size, with 95% confidence intervals.

Table B.1.1: **Descriptive statistics: Birth Order**

	All	1st born	2nd born	3rd born	4th born	5th born
Entrepreneur	0.199	0.198	0.201	0.202	0.196	0.192
	(0.400)	(0.398)	(0.401)	(0.402)	(0.397)	(0.394)
Unincorporated	0.146	0.142	0.146	0.151	0.150	0.151
	(0.353)	(0.349)	(0.353)	(0.358)	(0.357)	(0.358)
Incorporated	0.085	0.087	0.087	0.083	0.076	0.069
	(0.279)	(0.281)	(0.282)	(0.276)	(0.266)	(0.254)
Years of schooling	12.318	12.509	12.319	12.136	11.914	11.670
	(2.166)	(2.180)	(2.151)	(2.154)	(2.109)	(2.015)
Male	0.513	0.514	0.513	0.512	0.513	0.504
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Mother age at first birth	22.933	23.005	23.256	22.612	22.054	21.601
	(3.996)	(4.010)	(4.167)	(3.755)	(3.492)	(3.263)
Mother years of schooling	10.135	10.694	10.150	9.577	8.894	8.264
	(2.805)	(2.742)	(2.788)	(2.771)	(2.568)	(2.174)
Father years of schooling	10.072	10.446	10.091	9.719	9.193	8.632
	(3.066)	(3.041)	(3.048)	(3.088)	(2.958)	(2.622)
Unincorporated mother	0.156	0.164	0.156	0.151	0.140	0.119
	(0.363)	(0.370)	(0.363)	(0.358)	(0.347)	(0.324)
Unincorporated father	0.256	0.273	0.253	0.244	0.224	0.192
	(0.436)	(0.445)	(0.435)	(0.429)	(0.417)	(0.394)
Incorporated mother	0.034	0.042	0.034	0.025	0.016	0.010
	(0.181)	(0.201)	(0.181)	(0.155)	(0.126)	(0.098)
Incorporated father	0.067	0.089	0.067	0.044	0.026	0.015
	(0.250)	(0.285)	(0.249)	(0.205)	(0.159)	(0.121)
Parental income (log)	23.828	23.943	23.868	23.706	23.471	23.260
	(1.089)	(1.044)	(1.051)	(1.131)	(1.224)	(1.180)
Family size	2.931	2.624	2.662	3.398	4.238	5.000
	(0.919)	(0.785)	(0.800)	(0.620)	(0.426)	(0.000)
N	603,616	228,062	216,227	106,468	40,460	12,399

Sample restricted to families with at most 5 children and without multiple births (twins, triplets, quadruplets, quintuplets). The number of observations on parental characteristics varies with data availability (though only at most 5% of observations are missing).

Table B.1.2: **Birth Order: OLS Models**

		Entrepreneur			Unincorporated entrepreneur			Incorporated entrepreneur		
		(1) All	(2) Male	(3) Female	(4) All	(5) Male	(6) Female	(7) All	(8) Male	(9) Female
A. No, controls	2nd born	0.0040***	0.0050***	0.0030**	0.0039***	0.0061***	0.0016	0.0011	0.0008	0.0014
		(0.0011)	(0.0018)	(0.0015)	(0.0010)	(0.0015)	(0.0013)	(0.0008)	(0.0013)	(0.0009)
	3rd born	0.0058***	0.0055**	0.0062***	0.0059***	0.0073***	0.0044**	-0.0002	-0.0014	0.0011
		(0.0015)	(0.0024)	(0.0020)	(0.0014)	(0.0021)	(0.0018)	(0.0011)	(0.0018)	(0.0012)
	4th born	-0.0010	-0.0056	0.0039	0.0011	-0.0014	0.0037	-0.0030*	-0.0056**	-0.0004
		(0.0024)	(0.0037)	(0.0030)	(0.0021)	(0.0033)	(0.0027)	(0.0016)	(0.0027)	(0.0018)
	5th born	-0.0024	-0.0083	0.0038	-0.0007	-0.0043	0.0033	-0.0065**	-0.0122***	-0.0008
		(0.0040)	(0.0062)	(0.0049)	(0.0036)	(0.0056)	(0.0045)	(0.0026)	(0.0043)	(0.0028)
	N	603,616	309,706	293,910	603,616	309,706	293,910	603,616	309,706	293,910
B. With controls	2nd born	0.0117***	0.0148***	0.0083***	0.0090***	0.0121***	0.0057***	0.0054***	0.0071***	0.0035***
		(0.0014)	(0.0022)	(0.0019)	(0.0013)	(0.0020)	(0.0017)	(0.0010)	(0.0017)	(0.0011)
	3nd born	0.0201***	0.0249***	0.0149***	0.0156***	0.0197***	0.0112***	0.0077***	0.0108***	0.0042**
		(0.0024)	(0.0037)	(0.0030)	(0.0021)	(0.0033)	(0.0027)	(0.0016)	(0.0027)	(0.0018)
	4th born	0.0208***	0.0249***	0.0165***	0.0161***	0.0185***	0.0136***	0.0090***	0.0135***	0.0041
		(0.0035)	(0.0054)	(0.0044)	(0.0032)	(0.0048)	(0.0040)	(0.0024)	(0.0040)	(0.0026)
	5th born	0.0294***	0.0334***	0.0244***	0.0211***	0.0227***	0.0193***	0.0110***	0.0141**	0.0067*
		(0.0052)	(0.0080)	(0.0065)	(0.0047)	(0.0072)	(0.0059)	(0.0034)	(0.0057)	(0.0038)
	N	595,091	305,248	289,843	595,091	305,248	289,843	595,091	305,248	289,843

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. All models control for birth year dummies, gender, and family size. In Panel B, controls include dummies for mother's birth year, education level, and age at first birth dummies, and parental entrepreneurship.

Table B.1.3: **Birth Order: by Family Type, and Effects of Education, by Gender**

A. Family type	All families		Male only		Female only		Mixed	
	(1) Uninc.	(2) Inc.	(3) Uninc.	(4) Inc.	(5) Uninc.	(6) Inc.	(7) Uninc.	(8) Inc.
2nd born	0.0053***	0.0016	0.0101**	-0.0023	0.0067	0.0029	0.0031	0.0024
	(0.0020)	(0.0016)	(0.0047)	(0.0040)	(0.0043)	(0.0028)	(0.0027)	(0.0021)
3nd born	0.0104***	0.0010	0.0155*	-0.0113	0.0087	0.0052	0.0087*	0.0031
	(0.0039)	(0.0030)	(0.0094)	(0.0079)	(0.0086)	(0.0057)	(0.0049)	(0.0037)
4th born	0.0122**	0.0022	0.0216	-0.0156	0.0086	0.0051	0.0092	0.0054
	(0.0059)	(0.0045)	(0.0148)	(0.0123)	(0.0133)	(0.0090)	(0.0074)	(0.0056)
5th born	0.0292***	0.0085	0.0600***	-0.0178	0.0019	0.0038	0.0258**	0.0155*
	(0.0089)	(0.0067)	(0.0225)	(0.0181)	(0.0200)	(0.0132)	(0.0112)	(0.0084)
N	603,616	603,616	184,720	184,720	169,900	169,900	248,996	248,996
B. Gender	Unincorporated entrepreneur				Incorporated entrepreneur			
	Male		Female		Male		Female	
	(1)	(2) Ed.	(3)	(4) Ed.	(5)	(6) Ed.	(7)	(8) Ed.
2nd born	0.0066*	0.0038	0.0043	0.0029	-0.0029	-0.0034	0.0025	0.0031
	(0.0040)	(0.0040)	(0.0036)	(0.0037)	(0.0033)	(0.0034)	(0.0023)	(0.0024)
3rd born	0.0098	0.0042	0.0098	0.0069	-0.0091	-0.0110*	0.0056	0.0046
	(0.0074)	(0.0075)	(0.0066)	(0.0067)	(0.0062)	(0.0063)	(0.0043)	(0.0044)
4th born	0.0149	0.0096	0.0106	0.0071	-0.0085	-0.0108	0.0042	0.0021
	(0.0112)	(0.0115)	(0.0099)	(0.0101)	(0.0092)	(0.0094)	(0.0066)	(0.0067)
5th born	0.0432**	0.0364**	0.0186	0.0128	-0.0034	-0.0076	0.0065	0.0040
	(0.0170)	(0.0173)	(0.0149)	(0.0151)	(0.0135)	(0.0138)	(0.0098)	(0.0099)
N	309,706	305,426	293,910	290,284	309,706	305,426	293,910	290,284

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. All models control for birth year dummies and gender (when not split) and include fixed effects. Coefficients on education levels (as dummies) not reported for brevity.

Table B.1.4: **Birth Order, by Family Size and Traits**

A. Family size	2 children		3 children		4 children		5 children	
	(1) Uninc.	(2) Inc.	(3) Uninc.	(4) Inc.	(5) Uninc.	(6) Inc.	(7) Uninc.	(8) Inc.
2nd born	0.0129***	-0.0017	0.0057*	0.0068***	0.0020	-0.0026	-0.0156**	0.0004
	(0.0042)	(0.0033)	(0.0032)	(0.0025)	(0.0045)	(0.0034)	(0.0077)	(0.0056)
3nd born			0.0169***	0.0097**	0.0106	-0.0052	-0.0169	-0.0067
			(0.0063)	(0.0049)	(0.0074)	(0.0056)	(0.0107)	(0.0077)
4th born					0.0176	-0.0067	-0.0189	-0.0050
					(0.0114)	(0.0085)	(0.0148)	(0.0108)
5th born							-0.0097	-0.0031
							(0.0206)	(0.0148)
N	234,087	234,087	220,422	220,422	105,839	105,839	43,268	43,268
B. Family traits	Parents entrepreneurs		Parents not entrepreneurs		Below median income		Above median income	
	(9) Uninc.	(10) Inc.	(11) Uninc.	(12) Inc.	(13) Uninc.	(14) Inc.	(15) Uninc.	(16) Inc.
2nd born	-0.0011	0.0012	0.0098***	0.0021	0.0024	-0.0006	0.0083***	0.0049**
	(0.0038)	(0.0030)	(0.0024)	(0.0018)	(0.0030)	(0.0021)	(0.0029)	(0.0025)
3nd born	0.0005	-0.0051	0.0177***	0.0055	0.0089	-0.0014	0.0131**	0.0057
	(0.0074)	(0.0057)	(0.0045)	(0.0034)	(0.0054)	(0.0038)	(0.0056)	(0.0047)
4th born	0.0031	-0.0078	0.0201***	0.0090*	0.0102	-0.0015	0.0168*	0.0079
	(0.0114)	(0.0090)	(0.0069)	(0.0051)	(0.0081)	(0.0057)	(0.0089)	(0.0075)
5th born	0.0260	0.0050	0.0350***	0.0129*	0.0243**	0.0010	0.0469***	0.0208*
	(0.0182)	(0.0136)	(0.0102)	(0.0076)	(0.0118)	(0.0083)	(0.0153)	(0.0122)
N	218,810	218,810	384,806	384,806	283,889	283,889	319,727	319,727

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. All models control for birth year dummies and gender (when not split) and include fixed effects. Families where at least one parent was an entrepreneur of any kind are considered entrepreneurial (further dividing unincorporated and incorporated entrepreneurs does not add insight, and some statistical power is lost).

Table B.1.5: **Linear Birth Order**

	Unincorporated entrepreneur						
	(1) OLS	(2) FE	(3) OLS	(4) FE	(5) FE	(6) FE	(7) FE, ed.
Birth order	0.0015***	0.0051***	0.0118***	0.0140***	0.0071***	0.0034*	0.0096***
	(0.0005)	(0.0018)	(0.0018)	(0.0036)	(0.0019)	(0.0020)	(0.0037)
Family size	0.0021***		0.0078***				
	(0.0006)		(0.0011)				
Birth order × Family size			-0.0028***	-0.0024***			-0.0017**
			(0.0005)	(0.0008)			(0.0008)
Birth order × Parents entrepreneurs					-0.0051***		
					(0.0017)		
Birth order × Above median income						0.0035**	
						(0.0016)	
N	603,616	603,616	603,616	603,616	603,616	603,616	595,710
	Incorporated entrepreneur						
	(8) OLS	(9) FE	(10) OLS	(11) FE	(12) FE	(13) FE	(14) FE, ed.
Birth order	0.0006	0.0010	0.0047***	0.0011	0.0021	0.0024	-0.0007
	(0.0004)	(0.0014)	(0.0014)	(0.0028)	(0.0015)	(0.0015)	(0.0029)
Family size	-0.0053***		-0.0024***				
	(0.0005)		(0.0009)				
Birth order × Family size			-0.0015***	-0.0000			0.0001
			(0.0004)	(0.0006)			(0.0006)
Birth order × Parents entrepreneurs					-0.0030**		
					(0.0013)		
Birth order × Above median income						-0.0026**	
						(0.0012)	
N	603,616	603,616	603,616	603,616	603,616	603,616	595,710

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. All models control for birth year dummies and gender.

Table B.1.6: **Birth Order: Order among Same Sex Siblings**

	Unincorporated entrepreneur				Incorporated entrepreneur			
	Male		Female		Male		Female	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2nd	0.0138**	0.0147	0.0118**	0.0087	-0.0073	-0.0085	0.0058	0.0059
	(0.0059)	(0.0125)	(0.0054)	(0.0108)	(0.0050)	(0.0108)	(0.0036)	(0.0069)
3rd	0.0209*	0.0211	0.0202**	0.0161	-0.0093	-0.0127	0.0101	0.0100
	(0.0109)	(0.0202)	(0.0097)	(0.0174)	(0.0092)	(0.0173)	(0.0065)	(0.0113)
4th	0.0299*	0.0350	0.0253*	0.0205	-0.0063	-0.0074	0.0048	0.0070
	(0.0173)	(0.0279)	(0.0149)	(0.0240)	(0.0144)	(0.0236)	(0.0099)	(0.0153)
5th	0.0370	0.0390	0.0742**	0.0611	0.0274	0.0334	-0.0054	0.0029
	(0.0368)	(0.0474)	(0.0326)	(0.0410)	(0.0277)	(0.0369)	(0.0202)	(0.0254)
	Males:							
2nd		-0.0011				0.0014		
		(0.0122)				(0.0105)		
3rd		0.0012				0.0056		
		(0.0201)				(0.0172)		
4th		-0.0272				-0.0130		
		(0.0348)				(0.0296)		
5th		0.0686				-0.0471		
		(0.1159)				(0.0507)		
	Females:							
2nd				0.0034				-0.0001
				(0.0104)				(0.0067)
3rd				0.0037				0.0005
				(0.0174)				(0.0110)
4th				0.0051				-0.0120
				(0.0305)				(0.0203)
5th				0.0788				-0.0518
				(0.0991)				(0.0577)
N	114,884	114,884	108,466	108,466	114,884	114,884	108,466	108,466

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. All models control for birth year dummies and gender (when not split) and include family fixed effects. I restrict the sample to families completely captured in the data to accurately describe birth order among same sex siblings.

Table B.1.7: **Birth Order: Education, Cognitive and Non-Cognitive Scores**

	Years of schooling (1)	Logical ability (2)	Verbal ability (3)	Spatial ability (4)	Technical ability (5)	Leadership skills (6)	Skill (im)balance (7)
2nd born	-0.2412***	-0.2357***	-0.3749***	-0.1560***	-0.1353***	-0.1233***	0.0091***
	(0.0107)	(0.0224)	(0.0204)	(0.0234)	(0.0235)	(0.0247)	(0.0020)
3rd born	-0.3956***	-0.4341***	-0.6213***	-0.3449***	-0.3368***	-0.2134***	0.0173***
	(0.0202)	(0.0420)	(0.0384)	(0.0435)	(0.0437)	(0.0479)	(0.0038)
4th born	-0.4896***	-0.4998***	-0.7529***	-0.4086***	-0.4277***	-0.3709***	0.0267***
	(0.0310)	(0.0639)	(0.0581)	(0.0666)	(0.0667)	(0.0758)	(0.0059)
5th born	-0.6084***	-0.5591***	-0.8499***	-0.4533***	-0.4899***	-0.4054***	0.0250***
	(0.0460)	(0.0976)	(0.0887)	(0.1029)	(0.1022)	(0.1216)	(0.0093)
N	595,710	227,837	229,378	229,379	234,614	169,065	230,981
Outcome mean	12.3183	4.9839	4.8463	5.1477	4.9284	5.2980	0.2573
	(2.1661)	(1.9428)	(1.7443)	(1.9116)	(1.9482)	(1.4980)	(0.1408)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level; the last row shows the standard deviation of the given outcome in the sample used in each model. All models control for birth year dummies and include fixed effects. In columns (2)-(6), the sample is restricted to males in families with two to five children with available cognitive and noncognitive scores (measured on a 1 to 9 scale). In column (7), skill balance is measured by the intra-individual coefficient of variation (standard deviation divided by mean) of cognitive and noncognitive skills, using all available data; results are similar if I restrict the sample to observations with data on all skills.

Table B.1.8: **Descriptive statistics: Family size**

	All	1 child	2 child	3 child	4 child	5 child	6+ child
Unincorporated	0.146	0.146	0.141	0.147	0.151	0.152	0.150
	(0.353)	(0.354)	(0.348)	(0.354)	(0.358)	(0.359)	(0.357)
Incorporated	0.083	0.073	0.089	0.087	0.080	0.070	0.059
	(0.276)	(0.261)	(0.284)	(0.281)	(0.272)	(0.256)	(0.235)
Years of schooling	12.261	12.302	12.543	12.363	12.028	11.665	11.288
	(2.168)	(2.140)	(2.144)	(2.173)	(2.152)	(2.081)	(1.981)
Male	0.513	0.512	0.512	0.515	0.512	0.510	0.509
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Mother age at first birth	23.037	26.011	24.026	22.722	21.896	21.280	20.881
	(4.271)	(6.517)	(4.297)	(3.754)	(3.521)	(3.359)	(3.145)
Mother years of schooling	10.044	10.168	10.389	10.265	9.750	9.108	8.410
	(2.799)	(2.704)	(2.765)	(2.834)	(2.792)	(2.589)	(2.227)
Father years of schooling	9.985	10.005	10.283	10.203	9.727	9.123	8.507
	(3.045)	(2.847)	(2.991)	(3.133)	(3.089)	(2.830)	(2.440)
Unincorporated mother	0.152	0.124	0.148	0.164	0.162	0.147	0.118
	(0.359)	(0.330)	(0.355)	(0.370)	(0.368)	(0.354)	(0.323)
Unincorporated father	0.251	0.229	0.247	0.266	0.260	0.238	0.207
	(0.434)	(0.420)	(0.431)	(0.442)	(0.439)	(0.426)	(0.404)
Incorporated mother	0.032	0.024	0.037	0.036	0.028	0.019	0.009
	(0.175)	(0.152)	(0.189)	(0.187)	(0.164)	(0.136)	(0.094)
Incorporated father	0.064	0.059	0.076	0.070	0.053	0.036	0.018
	(0.244)	(0.235)	(0.266)	(0.256)	(0.223)	(0.185)	(0.134)
Parental income (log)	23.775	23.781	23.966	23.867	23.644	23.360	22.929
	(1.153)	(1.312)	(0.998)	(1.066)	(1.172)	(1.253)	(1.543)
Multiple birth	0.021	0.000	0.008	0.023	0.037	0.042	0.044
	(0.144)	(0.000)	(0.087)	(0.150)	(0.188)	(0.202)	(0.204)
N	705,262	44,420	235,092	227,311	111,881	46,342	39,406

Some observations on parental traits are missing (less than 10%, with smaller numbers in smaller families).

Table B.1.9: **Family Size: Unincorporated Entrepreneurship**

	Basic controls		+ Demographic controls		+ Birth order		+ Education		+ Parental income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Family size	0.0035***		0.0018*		-0.0020*		-0.0018		-0.0025**	
	(0.0009)		(0.0010)		(0.0011)		(0.0011)		(0.0011)	
Fam. size^2	-0.0003***		-0.0001		0.0002*		0.0002		0.0002	
	(0.0001)		(0.0001)		(0.0001)		(0.0001)		(0.0001)	
2 children		-0.0047***		-0.0067***		-0.0100***		-0.0071***		-0.0098***
		(0.0018)		(0.0019)		(0.0020)		(0.0020)		(0.0020)
3 children		-0.0004		-0.0047**		-0.0107***		-0.0079***		-0.0110***
		(0.0018)		(0.0019)		(0.0021)		(0.0021)		(0.0021)
4 children		0.0027		-0.0009		-0.0077***		-0.0059**		-0.0091***
		(0.0020)		(0.0021)		(0.0023)		(0.0024)		(0.0024)
5 children		0.0017		-0.0003		-0.0075**		-0.0069**		-0.0107***
		(0.0025)		(0.0025)		(0.0029)		(0.0030)		(0.0030)
6+ children		-0.0006		-0.0019		-0.0054		-0.0060		-0.0113***
		(0.0027)		(0.0027)		(0.0036)		(0.0037)		(0.0036)
2nd born					0.0063***	0.0075***	0.0026**	0.0034***	0.0071***	0.0081***
					(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
3rd born					0.0112***	0.0117***	0.0045***	0.0049***	0.0125***	0.0128***
					(0.0016)	(0.0016)	(0.0016)	(0.0017)	(0.0016)	(0.0017)
4th born					0.0111***	0.0094***	0.0024	0.0013	0.0126***	0.0110***
					(0.0024)	(0.0024)	(0.0024)	(0.0025)	(0.0024)	(0.0024)
5th born					0.0116***	0.0097***	0.0006	-0.0001	0.0131***	0.0120***
					(0.0034)	(0.0036)	(0.0035)	(0.0037)	(0.0034)	(0.0036)
6th+ born					0.0012	0.0005	-0.0108**	-0.0108**	0.0035	0.0028
					(0.0045)	(0.0046)	(0.0046)	(0.0047)	(0.0045)	(0.0046)
N	705,262	705,262	665,694	665,694	665,694	665,694	658,488	658,488	665,679	665,679

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. All models control for own and mother's birth year and gender. Demographic controls include father's birth year, parents' education level, unincorporated and incorporated entrepreneurship. Results robust to including neighborhood fixed effects and to restricting the sample to families without multiple births or adopted children.

Table B.1.10: **Family Size: Incorporated Entrepreneurship**

	Basic controls		+ Demographic controls		+ Birth order		+ Education		+ Parental income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Family size	-0.0005		-0.0000		-0.0019**		-0.0017*		0.0006	
	(0.0007)		(0.0008)		(0.0009)		(0.0009)		(0.0009)	
Fam. size^2	-0.0004***		-0.0003***		-0.0002		-0.0002		-0.0002*	
	(0.0001)		(0.0001)		(0.0001)		(0.0001)		(0.0001)	
2 children		0.0146***		0.0101***		0.0079***		0.0084***		0.0073***
		(0.0014)		(0.0014)		(0.0015)		(0.0015)		(0.0015)
3 children		0.0120***		0.0077***		0.0040**		0.0047***		0.0059***
		(0.0014)		(0.0014)		(0.0016)		(0.0016)		(0.0016)
4 children		0.0057***		0.0052***		0.0004		0.0011		0.0053***
		(0.0015)		(0.0016)		(0.0018)		(0.0018)		(0.0018)
5 children		-0.0043**		-0.0004		-0.0061***		-0.0053**		0.0021
		(0.0018)		(0.0019)		(0.0022)		(0.0022)		(0.0022)
6+ children		-0.0155***		-0.0078***		-0.0149***		-0.0139***		-0.0029
		(0.0018)		(0.0019)		(0.0025)		(0.0025)		(0.0025)
2nd born					0.0062***	0.0048***	0.0055***	0.0041***	0.0051***	0.0042***
					(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)
3rd born					0.0074***	0.0070***	0.0062***	0.0058***	0.0053***	0.0051***
					(0.0013)	(0.0013)	(0.0013)	(0.0013)	(0.0013)	(0.0013)
4th born					0.0074***	0.0083***	0.0057***	0.0066***	0.0046***	0.0050***
					(0.0018)	(0.0018)	(0.0018)	(0.0019)	(0.0018)	(0.0018)
5th born					0.0055**	0.0093***	0.0032	0.0070***	0.0021	0.0046*
					(0.0025)	(0.0026)	(0.0025)	(0.0026)	(0.0025)	(0.0026)
6th+ born					0.0096***	0.0115***	0.0070**	0.0090***	0.0037	0.0060*
					(0.0032)	(0.0032)	(0.0033)	(0.0032)	(0.0032)	(0.0032)
N	705,262	705,262	665,694	665,694	665,694	665,694	658,488	658,488	665,679	665,679

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. All models control for own and mother's birth year and gender. Demographic controls include father's birth year, parents' education level, unincorporated and incorporated entrepreneurship. Results robust to including neighborhood fixed effects and to restricting the sample to families without multiple births or adopted children.

Table B.1.11: **Family Size, by Gender**

A. Unincorporated

	Male			Female		
	(1)	(2) + Dem.	(3) + Order	(4)	(5) + Dem.	(6) + Order
Family size	0.0075***	0.0052***	0.0014	-0.0007	-0.0018	-0.0056***
	(0.0015)	(0.0015)	(0.0017)	(0.0011)	(0.0011)	(0.0013)
Family size^2	-0.0004**	-0.0003	0.0002	-0.0002*	0.0001	0.0003**
	(0.0002)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
2nd born			0.0069***			0.0058***
			(0.0017)			(0.0014)
3rd born			0.0111***			0.0116***
			(0.0025)			(0.0021)
4th born			0.0087**			0.0142***
			(0.0036)			(0.0030)
5th born			0.0079			0.0154***
			(0.0053)			(0.0043)
6th+ born			-0.0104			0.0134**
			(0.0069)			(0.0056)
N	361,556	341,299	341,299	343,706	324,395	324,395

B. Incorporated

	Male			Female		
	(7)	(8) + Dem.	(9) + Order	(10)	(11) + Dem.	(12) + Order
Family size	0.0001	0.0011	-0.0017	-0.0013*	-0.0012*	-0.0023***
	(0.0012)	(0.0013)	(0.0015)	(0.0007)	(0.0007)	(0.0008)
Family size^2	-0.0006***	-0.0004***	-0.0003	-0.0002***	-0.0001	-0.0000
	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
2nd born			0.0083***			0.0040***
			(0.0014)			(0.0010)
3rd born			0.0107***			0.0042***
			(0.0021)			(0.0014)
4th born			0.0109***			0.0041**
			(0.0029)			(0.0019)
5th born			0.0079*			0.0031
			(0.0041)			(0.0027)
6th+ born			0.0139**			0.0050
			(0.0055)			(0.0033)
N	361,556	341,299	341,299	343,706	324,395	324,395

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. All models control for own and mother's birth year and gender. Demographic controls include father's birth year, mother's and father's education level, unincorporated and incorporated entrepreneurship.

Table B.1.12: **Family Size, By Parental Entrepreneurship and Income**

	Parents entrepreneurs		Parents not entrepreneurs		Below median income		Above median income	
	(1) Uninc.	(2) Inc.	(3) Uninc.	(4) Inc.	(5) Uninc.	(6) Inc.	(7) Uninc.	(8) Inc.
A. Linear family size, all children								
Family size	-0.0012	-0.0037***	0.0007	-0.0028***	0.0011*	-0.0020***	-0.0026***	-0.0012*
	(0.0009)	(0.0007)	(0.0006)	(0.0004)	(0.0006)	(0.0004)	(0.0008)	(0.0006)
N	235,348	235,348	430,346	430,346	325,616	325,616	340,078	340,078
B. Linear family size, males								
Family size	0.0030**	-0.0051***	0.0026***	-0.0031***	0.0039***	-0.0026***	-0.0007	-0.0001
	(0.0014)	(0.0011)	(0.0009)	(0.0007)	(0.0010)	(0.0007)	(0.0012)	(0.0011)
N	121,421	121,421	219,878	219,878	167,325	167,325	173,974	173,974
C. Linear family size, females								
Family size	-0.0055***	-0.0023***	-0.0014**	-0.0024***	-0.0019**	-0.0013***	-0.0045***	-0.0023***
	(0.0011)	(0.0007)	(0.0007)	(0.0004)	(0.0008)	(0.0004)	(0.0010)	(0.0007)
N	113,927	113,927	210,468	210,468	158,291	158,291	166,104	166,104
D. Quadratic family size, all children								
Family size	-0.0019	0.0002	-0.0017	-0.0025**	0.0004	-0.0021**	-0.0061***	0.0064***
	(0.0024)	(0.0017)	(0.0012)	(0.0010)	(0.0015)	(0.0011)	(0.0022)	(0.0018)
Family size^2	0.0001	-0.0005***	0.0003**	-0.0000	0.0001	0.0000	0.0005*	-0.0012***
	(0.0003)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0003)	(0.0003)
N	235,348	235,348	430,346	430,346	325,616	325,616	340,078	340,078

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. All models control for own and parents' birth year and gender, mother's and father's education level, and birth order.

Table B.1.13: **Family Size: Alternative Instruments**

	Unincorporated entrepreneur		Incorporated entrepreneur	
	(1) OLS	(2) IV	(3) OLS	(4) IV
A. Instrument: same gender siblings				
Sample: first two children, family size 2+				
Family size	0.0006	-0.0188	-0.0048***	-0.0021
	(0.0016)	(0.0314)	(0.0013)	(0.0259)
Second born	0.0076***	0.0136	0.0031**	0.0023
	(0.0020)	(0.0099)	(0.0016)	(0.0081)
N	193,906	193,906	193,906	193,906
B. Instrument: first two males/females				
Sample: first two children, family size 2+				
Family size		-0.0187		-0.0021
		(0.0314)		(0.0259)
Second born		0.0136		0.0023
		(0.0099)		(0.0081)
N	193,906	193,906	193,906	193,906
C. Instrument: multiple second birth + sex composition				
Sample: first born children, family size 2+				
Family size	0.0028	-0.0150	-0.0038**	-0.0003
	(0.0022)	(0.0111)	(0.0017)	(0.0093)
N	96,518	96,518	96,518	96,518
D. Instrument: multiple third birth + sex composition				
Sample: first two children, family size 3+				
Family size	-0.0007	0.0137	-0.0056*	0.0063
	(0.0039)	(0.0164)	(0.0030)	(0.0134)
Second born	0.0032	0.0014	0.0041	0.0027
	(0.0038)	(0.0042)	(0.0031)	(0.0034)
N	52,028	52,028	52,028	52,028
E. Instrument: multiple birth + sex composition				
Sample: pooled parity				
Family size	0.0026	0.0039	-0.0047***	-0.0044**
	(0.0020)	(0.0024)	(0.0016)	(0.0019)
N	122,699	122,699	122,699	122,699

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses, robust in Panel C and clustered at the family level in Panels A, B, D, and E. All models control for gender, own and parental birth year, and parental education. The sample is restricted to complete families. Following Angrist et al. (2010), Panel C includes dummies for first two males/females and multiple second birth as instruments; Panel D includes three males/females, boy at third birth, and multiple third birth as instruments, with controls for first two children both male or female; Panel E pools together the instruments and samples in Panels C and D – these results should be interpreted cautiously, as consistency requires implausibly stringent assumptions when instruments are missing non-randomly (Mogstad and Wiswall, 2012). The first stage F-statistics are 261.53, 130.85, 1,990.08, 827.58, and 48,593.70 in Panels A-E; in Panels A, B, D, and E, these are Paap-Kleibergen F-statistics, adjusting for clustering (Cameron and Miller, 2015). Further controlling for parental entrepreneurship and income reduces both the magnitude and significance of family size coefficients.

Table B.1.14: **Family Size: Birth Order Index**

	Unincorporated entrepreneur			Incorporated entrepreneur		
	(1) All	(2) Male	(3) Female	(4) All	(5) Male	(6) Female
A. All children						
Family size	0.0001	0.0013**	-0.0011**	-0.0005*	-0.0000	-0.0009***
	(0.0004)	(0.0005)	(0.0004)	(0.0003)	(0.0004)	(0.0003)
Birth order index	0.0126***	0.0131***	0.0124***	0.0056***	0.0076***	0.0036***
	(0.0014)	(0.0022)	(0.0018)	(0.0011)	(0.0018)	(0.0012)
N	665,679	341,292	324,387	665,679	341,292	324,387
B. Families with no multiple births or adopted children						
Family size	0.0003	0.0016***	-0.0011**	-0.0006**	-0.0003	-0.0010***
	(0.0004)	(0.0006)	(0.0004)	(0.0003)	(0.0004)	(0.0003)
Birth order index	0.0133***	0.0147***	0.0120***	0.0051***	0.0065***	0.0038***
	(0.0015)	(0.0023)	(0.0019)	(0.0012)	(0.0019)	(0.0013)
N	634,003	325,349	308,654	634,003	325,349	308,654
C. Complete families						
Family size	-0.0004	0.0018	-0.0027***	0.0015**	0.0033***	-0.0004
	(0.0009)	(0.0013)	(0.0010)	(0.0007)	(0.0011)	(0.0007)
Birth order index	0.0145***	0.0147***	0.0154***	0.0042**	0.0046	0.0042*
	(0.0026)	(0.0040)	(0.0034)	(0.0021)	(0.0034)	(0.0023)
N	265,682	136,523	129,159	265,682	136,523	129,159

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. All models control for gender, own, mother's, and fathers' birth year, mother's and father's education level, entrepreneurship, and income.

Table B.1.15: **Descriptive statistics: Sibling Sex Composition**

	All	Male	Female
Unincorporated	0.145	0.176	0.112
	(0.352)	(0.381)	(0.315)
Incorporated	0.089	0.127	0.049
	(0.285)	(0.334)	(0.216)
Mother age at first birth	23.743	23.750	23.735
	(4.245)	(4.252)	(4.238)
Mother years of schooling	10.467	10.471	10.463
	(2.820)	(2.820)	(2.821)
Father years of schooling	10.317	10.316	10.317
	(3.103)	(3.104)	(3.103)
Unincorporated mother	0.158	0.160	0.157
	(0.365)	(0.366)	(0.363)
Unincorporated father	0.261	0.264	0.259
	(0.439)	(0.441)	(0.438)
Incorporated mother	0.038	0.039	0.036
	(0.191)	(0.194)	(0.187)
Incorporated father	0.075	0.077	0.073
	(0.263)	(0.266)	(0.260)
Immigrant mother	0.106	0.107	0.106
	(0.308)	(0.309)	(0.308)
Immigrant father	0.092	0.091	0.092
	(0.288)	(0.288)	(0.289)
Parental income (log)	23.966	23.961	23.971
	(0.994)	(0.997)	(0.990)
Any sister	0.588	0.600	0.576
	(0.492)	(0.490)	(0.494)
Any brother	0.616	0.603	0.630
	(0.486)	(0.490)	(0.483)
Number of sisters	0.714	0.722	0.706
	(0.701)	(0.691)	(0.712)
Number of brothers	0.757	0.750	0.764
	(0.712)	(0.726)	(0.696)
Younger sisters	0.357	0.359	0.355
	(0.577)	(0.576)	(0.577)
Older sisters	0.357	0.363	0.351
	(0.568)	(0.566)	(0.569)
Younger brothers	0.382	0.377	0.386
	(0.595)	(0.596)	(0.593)
Older brothers	0.375	0.373	0.379
	(0.579)	(0.596)	(0.574)
Family size	2.487	2.489	2.485
	(0.579)	(0.817)	(0.799)
N	234,036	120,342	113,694

Some observations on parental traits are missing (less than 5%). Sample restricted to complete families with at least two children.

Table B.1.16: **Sibling Sex Composition**

		(1) Un.	(2) Un.	(3) Un.	(4) Un.	(5) Un.	(6) Inc.	(7) Inc.	(8) Inc.	(9) Inc.	(10) Inc.
A. Male,	Sister	0.0037			0.0126*		-0.0008			0.0183***	
$N = 115,937$		(0.0025)			(0.0070)		(0.0022)			(0.0060)	
	Brother		-0.0020					0.0046**			
			(0.0024)					(0.0021)			
	Percent			0.0044	-0.0185				-0.0064	-0.0398***	
	sisters			(0.0047)	(0.0135)				(0.0041)	(0.0115)	
	Younger					0.0097**					0.0083**
	sister					(0.0043)					(0.0037)
	Older					0.0058					0.0009
	sister					(0.0043)					(0.0037)
	Younger					0.0089**					0.0081***
	brother					(0.0041)					(0.0035)
	Older					0.0016					0.0068*
	brother					(0.0042)					(0.0035)
B. Female,	Sister	0.0012			0.0060		-0.0023			-0.0026	
$N = 109,598$		(0.0020)			(0.0059)		(0.0014)			(0.0041)	
	Brother		0.0012					0.0018			
			(0.0021)					(0.0014)			
	Percent			0.0009	-0.0099				-0.0040	0.0008	
	sisters			(0.0040)	(0.0116)				(0.028)	(0.0079)	
	Younger					0.0053					0.0015
	sister					(0.0035)					(0.0023)
	Older					-0.0038					-0.0008
	sister					(0.0036)					(0.0024)
	Younger					0.0053					0.0042*
	brother					(0.0036)					(0.0024)
	Older					-0.0049					0.0003
	brother					(0.0036)					(0.0024)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. Controls include individual and parental birth year, parental education, immigration, and entrepreneurship, family structure, and family size.

Table B.1.17: **Sibling Sex Composition: Presence and Number of Siblings, by Gender**

A. Any	2 children		3 children		4 children		2+ children	
brother/sister	(1) Uninc.	(2) Inc.	(3) Uninc.	(4) Inc.	(5) Uninc.	(6) Inc.	(7) Uninc.	(8) Inc.
A1. Males								
Any sister	0.0046	-0.0024	0.0025	0.0052	0.0100	0.0112	0.0058	0.0079**
	(0.0029)	(0.0025)	(0.0053)	(0.0047)	(0.0145)	(0.0116)	(0.0042)	(0.0037)
Any brother			0.0061	0.0114**	-0.0046	0.0205*	0.0027	0.0110***
			(0.0052)	(0.0045)	(0.0136)	(0.0105)	(0.0042)	(0.0036)
N	72,763	72,763	34,566	34,566	8,608	8,608	115,937	115,937
A2. Females								
Any sister			0.0086**	-0.0009	0.0033	-0.0048	0.0061*	-0.0024
			(0.0043)	(0.0029)	(0.0104)	(0.0069)	(0.0035)	(0.0024)
Any brother	0.0009	0.0026	0.0065	0.0008	-0.0043	-0.0109	0.0061*	-0.0001
	(0.0024)	(0.0017)	(0.0046)	(0.0032)	(0.0117)	(0.0081)	(0.0035)	(0.0024)
N	69,115	69,115	32,187	32,187	8,296	8,296	109,598	109,598
B. Number of	Male				Female			
brothers/sisters	(9) Uninc.	(10) Inc.	(11) Uninc.	(12) Inc.	(13) Uninc.	(14) Inc.	(15) Uninc.	(16) Inc.
Number of sisters	0.0015	-0.0026			0.0007	-0.0013		
	(0.0019)	(0.0017)			(0.0016)	(0.0011)		
Number of brothers			-0.0013	0.0024			-0.0006	0.0014
			(0.0019)	(0.0016)			(0.0016)	(0.0011)
N	115,937	115,937	115,937	115,937	109,598	109,598	109,598	109,598

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. Controls include individual and parental birth year dummies, parental education and entrepreneurship, parental immigration, family structure, and family size.

Table B.1.18: **Sibling Sex Composition: Types of Siblings for Females**

		2 children		3 children		4 children		2+ children	
		(1) Uninc.	(2) Inc.	(3) Uninc.	(4) Inc.	(5) Uninc.	(6) Inc.	(7) Uninc.	(8) Inc.
A. With controls	Any younger sister			0.0055	0.0041	0.0002	-0.0047	0.0053	0.0015
				(0.0048)	(0.0032)	(0.0096)	(0.0066)	(0.0035)	(0.0023)
	Any older sister	-0.0111***	-0.0037	-0.0018	0.0004	0.0035	0.0035	-0.0038	-0.0008
		(0.0039)	(0.0027)	(0.0050)	(0.0033)	(0.0099)	(0.0063)	(0.0036)	(0.0024)
	Any younger brother	0.0020	0.0036	0.0026	0.0047	-0.0079	0.0003	0.0053	0.0042*
		(0.0033)	(0.0023)	(0.0051)	(0.0034)	(0.0106)	(0.0067)	(0.0036)	(0.0024)
	Any older brother	-0.0113***	-0.0023	-0.0050	0.0040	-0.0049	-0.0065	-0.0049	0.0003
		(0.0039)	(0.0027)	(0.0052)	(0.0035)	(0.0106)	(0.0074)	(0.0036)	(0.00255)
B. With controls + birth order	Any younger sister			0.0035	-0.0023	-0.0032	-0.0106	-0.0013	-0.0061*
				(0.0062)	(0.0040)	(0.0121)	(0.0084)	(0.0052)	(0.0034)
	Any older sister	-0.0111***	-0.0037	0.0069	0.0026	0.0121	0.0020	-0.0003	0.0020
		(0.0039)	(0.0027)	(0.0068)	(0.0046)	(0.0125)	(0.0087)	(0.0045)	(0.0031)
	Any younger brother	0.0020	0.0036	0.0003	-0.0018	-0.0117	-0.0055	0.0013	-0.0034
		(0.0033)	(0.0023)	(0.0065)	(0.0043)	(0.0132)	(0.0081)	(0.0053)	(0.0035)
	Any older brother	-0.0113***	-0.0023	0.0038	0.0061	0.0035	-0.0081	-0.0015	0.0032
		(0.0039)	(0.0027)	(0.0069)	(0.0048)	(0.0135)	(0.0099)	(0.0045)	(0.0032)
C. With controls + birth order + parent income	Any younger sister			0.0032	-0.0026	-0.0028	-0.0094	-0.0012	-0.0063*
				(0.0062)	(0.0040)	(0.0121)	(0.0083)	(0.0052)	(0.0034)
	Any older sister	-0.0112***	-0.0033	0.0070	0.0025	0.0119	0.0019	-0.0003	0.0020
		(0.0039)	(0.0027)	(0.0068)	(0.0046)	(0.0125)	(0.0087)	(0.0045)	(0.0031)
	Any younger brother	0.0022	0.0041*	0.0001	-0.0020	-0.0119	-0.0049	0.0013	-0.0033
		(0.0033)	(0.0023)	(0.0065)	(0.0043)	(0.0132)	(0.0080)	(0.0053)	(0.0034)
	Any older brother	-0.0113***	-0.0016	0.0039	0.0063	0.0027	-0.0083	-0.0015	0.0034
		(0.0039)	(0.0027)	(0.0069)	(0.0047)	(0.0135)	(0.0100)	(0.0045)	(0.0032)
	N	69,113	69,113	32,187	32,187	6,678	6,678	109,593	109,593

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. Controls include individual and parental birth year dummies, parental education, entrepreneurship, and immigration, family structure at age 15, and family size.

Table B.1.19: **Balance Check: First Born Children with Next Sibling of Opposite Sex**

	First born male			First born female		
Next sibling:	(1) Same sex	(2) Opposite	(3) Difference	(4) Same sex	(5) Opposite	(6) Difference
Mother age at birth	23.890	23.839	0.051	23.857	23.854	0.003
Mother years of schooling	10.493	10.483	0.010	10.473	10.485	-0.012
Father years of schooling	10.335	10.325	0.009	10.354	10.342	0.013
Mother unincorporated	0.160	0.156	0.004	0.153	0.157	-0.004
Father unincorporated	0.266	0.264	0.002	0.254	0.257	-0.003
Mother incorporated	0.041	0.039	0.002	0.034	0.039	-0.005***
Father incorporated	0.080	0.078	0.002	0.071	0.076	-0.005**
Mother immigrant	0.100	0.100	-0.000	0.100	0.100	-0.001
Father immigrant	0.084	0.085	-0.000	0.086	0.087	-0.001
Parental income (log)	23.971	23.979	-0.009	23.986	23.996	-0.010
Family size	2.354	2.296	0.058***	2.349	2.289	0.060***
N	26,240	25,654		23,359	25,328	

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard deviations in parentheses. Sample restricted to first born children in complete families with at least two children. In multivariate regressions controlling for all parental characteristics, as well as individual and parental birth years, the differences become insignificant.

Table B.1.20: **Sibling Sex Composition: Twin Analysis**

	Unincorporated				Incorporated			
	Male		Female		Male		Female	
Controls:	(1) Basic	(2) Extended	(3) Basic	(4) Extended	(5) Basic	(6) Extended	(7) Basic	(8) Extended
A. All twins in same sex pairs included								
Next sibling brother	-0.0086	-0.0041	0.0044	0.0056	0.0173**	0.0193**	0.0085	0.0096*
	(0.0095)	(0.0097)	(0.0081)	(0.0084)	(0.0082)	(0.0084)	(0.0052)	(0.0054)
N	6,994	6,580	6,964	6,481	6,994	6,580	6,964	6,481
B. Twin in same sex pair randomly selected (100 replications)								
Next sibling brother	-0.0085	-0.0052	0.0043	0.0052	0.0157***	0.0188***	0.0088***	0.0094***
	(0.0037)	(0.0049)	(0.0036)	(0.0039)	(0.0035)	(0.0035)	(0.0023)	(0.0022)
C. Twin in same sex pair randomly selected (1,000 replications)								
Next sibling brother	-0.0087	-0.0043	0.0040	0.0049	0.0170***	0.0189***	0.0088***	0.0093***
	(0.0044)	(0.0049)	(0.0042)	(0.0040)	(0.0040)	(0.0042)	(0.0027)	(0.0022)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. Basic controls include individual and mother birth year, and family size; extended controls also include father birth year, parental education, immigration, and entrepreneurship, and family structure.

Table B.1.21: **Sibling Sex Composition: First Born Females – Heterogeneity**

	2 children		3 children		4 children		2+ children	
	(1) Uninc.	(2) Inc.	(3) Uninc.	(4) Inc.	(5) Uninc.	(6) Inc.	(7) Uninc.	(8) Inc.
A. Parents entrepreneurs: basic controls + parental income								
Next sibling brother	-0.0045	0.0047	-0.0206*	0.0018	-0.0308	-0.0205	-0.0095*	0.0033
	(0.0065)	(0.0046)	(0.0118)	(0.0082)	(0.0332)	(0.0230)	(0.0055)	(0.0039)
N	12,381	12,381	3,927	3,927	580	580	16,969	16,969
B. Parents not entrepreneurs: basic controls + parental income								
Next sibling brother	0.0017	-0.0002	0.0027	-0.0069	-0.0321*	0.0010	0.0005	-0.0019
	(0.0039)	(0.0027)	(0.0076)	(0.0050)	(0.0185)	(0.0131)	(0.0034)	(0.0023)
N	22,032	22,032	6,579	6,579	1,022	1,022	29,814	29,814
C. Mother earns at least as much as father: basic controls + parental income								
Next sibling brother	-0.0024	0.0057	0.0025	-0.0060	-0.0722**	-0.0313	-0.0041	0.0016
	(0.0063)	(0.0043)	(0.0116)	(0.0080)	(0.0282)	(0.0197)	(0.0054)	(0.0037)
N	10,827	10,827	3,430	3,430	602	602	14,965	14,965
D. Mother earns less than father: basic controls + parental income								
Next sibling brother	0.0006	-0.0001	-0.0093	-0.0021	-0.0190	-0.0055	-0.0025	-0.0005
	(0.0041)	(0.0029)	(0.0078)	(0.0052)	(0.0214)	(0.0147)	(0.0035)	(0.0025)
N	23,586	23,586	7,076	7,076	1,000	1,000	31,818	31,818
E. Parental income below median: basic controls								
Next sibling brother	-0.0027	0.0065*	-0.0040	0.0020	-0.0340	-0.0158	-0.0051	0.0040
	(0.0057)	(0.0035)	(0.0098)	(0.0058)	(0.0223)	(0.0134)	(0.0047)	(0.0029)
N	12,471	12,471	4,428	4,428	858	858	17,944	17,944
F. Parental income above median: basic controls								
Next sibling brother	0.0007	-0.0012	-0.0055	-0.0075	-0.0418*	0.0007	-0.0017	-0.0023
	(0.0043)	(0.0032)	(0.0086)	(0.0063)	(0.0239)	(0.0195)	(0.0038)	(0.0028)
N	21,943	21,943	6,078	6,078	745	745	28,841	28,841

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. Basic controls include individual and parental birth year, parental education, immigration, and entrepreneurship, family structure, and family size.

Table B.1.22: **Sibling Sex Composition: Direct and Indirect Effects, Instrumenting for Family Size Instrumented**

	First born male, uninc.		First born female, uninc.		First born male, inc.		First born female, inc.	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Next sib. opposite gender	0.0179	0.0455	0.0199*	-0.0027	-0.0006	0.0122	-0.0520	-0.0626
	(0.0144)	(0.0842)	(0.0120)	(0.0700)	(0.0123)	(0.0741)	(0.0084)	(0.0489)
Family size	0.0062	-0.0036	0.0004	-0.0155	-0.0017	-0.0244	-0.0007	0.0094
	(0.0042)	(0.0261)	(0.0038)	(0.0212)	(0.0037)	(0.0230)	(0.0026)	(0.0148)
Next sib. opposite gender	-0.0056	-0.0178	-0.0098*	-0.0003	-0.0011	0.0007	-0.0053	0.0231
× Family size	(0.0061)	(0.0363)	(0.0050)	(0.0303)	(0.0051)	(0.0319)	(0.0035)	(0.0211)
Decomposition:								
Average Total Effect	0.0047	0.0049	-0.0025	-0.0024	-0.0032	-0.0030	0.0002	0.0001
	(0.0034)	(0.0034)	(0.0030)	(0.0030)	(0.0030)	(0.0030)	(0.0021)	(0.0021)
Average Indirect Effect (*AIE*)	-0.0004	0.0002	-0.0000	0.0009	0.0001	0.0014	0.0000	-0.0006
	(0.0002)	(0.0002)	(0.0002)	(0.0013)	(0.0002)	(0.0013)	(0.0002)	(0.0009)
Average Direct Effect *(ADE)*	0.0051	0.0046	-0.0024	-0.0033	-0.0032	-0.0044	0.0002	0.0007
	(0.0034)	(0.0037)	(0.0030)	(0.0032)	(0.0030)	(0.0031)	(0.0020)	(0.0022)
Controlled Direct Effect *(CDE)*	0.0049	0.0041	-0.0027	-0.0033	-0.0033	-0.0044	0.0000	0.0013
	(0.0034)	(0.0036)	(0.0030)	(0.0031)	(0.0030)	(0.0031)	(0.0020)	(0.0021)
Difference *CDE* − *ADE*	-0.0002	-0.0005	-0.0003*	-0.0000	- 0.0000	0.0000	-0.0002	0.0007
	(0.0002)	(0.0011)	(0.0002)	(0.0009)	(0.0001)	(0.0009)	(0.0001)	(0.0006)
Sample mean	0.1816 (0.3855)		0.1160 (0.3202)		0.1357 (0.3424)		0.0527 (0.2334)	

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses. Controls include individual and parental birth year, parental education, immigration, and entrepreneurship, family structure. Sample restricted to first born children not part of a multiple birth (49,121 males and 46,154 females). Multiple birth at birth order two and its interaction with opposite gender next sibling are used as instruments for family size and its interaction with opposite gender next sibling. First stage F-statistics in IV models are 616.37 for males, and 679.91 for females. Denoting an opposite gender next sibling with D and family size with M, and the regression coefficients with β_D, β_M, and $\beta_{D\times M}$, I compute $AIE = \beta_M \times (E[M|D=1] - E[M|D=0])$, $ADE = \beta_D + \beta_{D\times M} \times E[M|D=1]$, and $CDE = \beta_{D\times M} \times E[M]$ (see Chen et al., 2017). The conditional mean of M is $(E[M|D=1], E[M|D=0]) = (2.2887, 2.3466)$ for first born males and $(2.2818, 2.3413)$ for first born females; the unconditional means are 2.3178 and 2.3102, respectively.

Table B.1.23: **Sibling Sex Composition and the Intergenerational Transmission of Entrepreneurship**

	Child is:	Male, uninc.		Male, inc.		Female, uninc.		Female, inc.	
	Parent is:	(1) Uninc.	(2) Inc.	(3) Uninc.	(4) Inc.	(5) Uninc.	(6) Inc.	(7) Uninc.	(8) Inc.
A. Father, all children	Opposite gender sibling	0.0020	0.0042***	-0.0028**	-0.0020	-0.0004	-0.0024*	-0.0002	-0.0001
		(0.0016)	(0.0015)	(0.0014)	(0.0012)	(0.0013)	(0.0013)	(0.0009)	(0.0008)
	Father entrepreneur	0.1168**	0.0259***	0.0506***	0.2202***	0.0532***	0.0425***	0.0159***	0.0681***
		(0.0023)	(0.0038)	(0.0019)	(0.0045)	(0.0019)	(0.0035)	(0.0013)	(0.0032)
	Opposite	**0.0083****	0.0067	0.0018	0.0049	**-0.0091*****	0.0016	0.0027	0.0105**
	× Father	(0.0036)	(0.0060)	(0.0030)	(0.0070)	(0.0029)	(0.0053)	(0.0019)	(0.0048)
B. Mother, all children	Opposite gender sibling	0.0032**	0.0043***	-0.0024*	-0.0017	-0.0015	-0.0023*	-0.0002	0.0002
		(0.0016)	(0.0015)	(0.0013)	(0.0012)	(0.0013)	(0.0012)	(0.0009)	(0.0008)
	Mother entrepreneur	0.1131***	0.0263***	0.0505***	0.2495***	0.0716***	0.0626***	0.0203***	0.1193***
		(0.0029)	(0.0053)	(0.0024)	(0.0065)	(0.0025)	(0.0052)	(0.0016)	(0.0053)
	Opposite	**0.0071**	0.0101	0.0009	0.0050	**-0.0053**	0.0043	0.0055**	0.0145*
	× Mother	(0.0044)	(0.0084)	(0.0008)	(0.0101)	(0.0037)	(0.0079)	(0.0025)	(0.0080)
C. Father, first born	Next sibling opposite gender	0.0004	0.0049	-0.0061*	-0.0018	0.0022	-0.0021	-0.0022	-0.0001
		(0.0037)	(0.0035)	(0.0034)	(0.0030)	(0.0033)	(0.0030)	(0.0023)	(0.0020)
	Father entrepreneur	0.1096***	0.0311***	0.0513***	0.2415***	0.0555***	0.0574***	0.0125***	0.0736***
		(0.0059)	(0.0092)	(0.0052)	(0.0108)	(0.0053)	(0.0096)	(0.0036)	(0.0082)
	Opposite	**0.0203****	0.0111	0.0079	-0.0213	**-0.0186****	-0.0071	0.0103**	0.0031
	× Father	(0.0085)	(0.0133)	(0.0074)	(0.0153)	(0.0072)	(0.0129)	(0.0050)	(0.0112)
D. Mother, first born	Next sibling opposite gender	0.0046	0.0062*	-0.0027	-0.0025	-0.0024	-0.0029	-0.0002	-0.0002
		(0.0036)	(0.0035)	(0.0032)	(0.0030)	(0.0031)	(0.0030)	(0.0021)	(0.0020)
	Mother entrepreneur	0.1077***	0.0382***	0.0593***	0.2709***	0.0722***	0.0527***	0.0233***	0.1271***
		(0.0073)	(0.0128)	(0.0065)	(0.0151)	(0.0068)	(0.0134)	(0.0046)	(0.0135)
	Opposite	**0.0085**	-0.0114	-0.0072	-0.0210	**-0.0018**	0.0080	0.0038	0.0021
	× Mother	(0.0105)	(0.0183)	(0.0091)	(0.0217)	(0.0093)	(0.0182)	(0.0064)	(0.0183)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses clustered at the family level. Controls include individual and parental birth year dummies and family size dummies. In Panels A and B, the sample is restricted to families with at least two children; in Panels C and D, it is restricted to first born children in complete families. Coefficients of interest in similar models estimated by Mishkin (2017) in bold.

B.2 Non-Linear Family Size Effects

Throughout Section 3.4, all the estimations implicitly assumed linear effects of family size. However, such specifications can be restrictive if the underlying processes are non-linear (e.g., Lochner and Moretti, 2015; Brinch et al., 2017), as would be the case if i) the effect of family size differs from going from 2 to 3 children, to 4 to 5 children, or ii) family size affects the availability of parental time and resources differently for different birth orders. Mogstad and Wiswall (2016) relax the linearity assumption to find substantial marginal family size effects, in contrast to the absence of such effects in, for instance, Black et al. (2005).

The models I estimate in Section 3.4 may therefore be less suitable for detecting differential effects of family size, which is the goal in this paper. To address this, I now estimate unrestricted family size models, using a non-linear instrumental variable strategy. I provide here only a sketch of the empirical approach, emphasizing the differences from the linear IV method employed previously; the interested reader is referred to Mogstad and Wiswall (2016) for a detailed description of the assumptions and mechanics of the model.

The linear IV model uses multiple births at parity t to instrument for family size in analyzing the outcomes of children born at parities $1, 2, ..., t-1$. The estimate is a local average treatment effect of family size increasing from t to $t+k$, where k may be larger than 1 if multiple births shift fertility preferences at higher parities (for instance, through economies of scale). The estimate is therefore a weighted average of the underlying marginal effects of going from family size t to $t+1$, $t+1$ to $t+2$, and so on (Angrist et al., 2010). Mogstad and Wiswall (2016) argue that it is these marginal effects that should be the main focus, rather than the total effect, as the latter can be zero even if the marginal effects are strictly non-zero (but cancel each other out).

Focusing on the analysis of the outcomes of first born children in families with at least two children (with parallel procedures for second born or third born children), the unrestricted model is as follows:

$$Y_i = \gamma_2 d_{2i} + \gamma_3 d_{3i} + \gamma_4 d_{4i} + \delta X_i + \epsilon_i, \tag{B.2.1}$$

where Y_i is entrepreneurship, X_i is a set of controls, and d_{si} are a set of dummies for the number of siblings being at least s (alternatively, family size being at least $s+1$), with $s \in \{2, 3, 4\}$. These dummies provide the marginal effects of having s rather than $s-1$ siblings.[1]

Multiple birth instruments As opposed to the sole endogenous variable in the linear IV estimation, there are now three endogenous variables, requiring at least three instruments. Unfortunately, one cannot rely directly on the multiple birth instruments as in the linear IV case, because a multiple birth at birth order t, for instance, is not defined in a family with $t-1$ children. Mogstad and Wiswall (2016) then propose adjusting the multiple birth instruments as follows, where c denotes family size:

$$Multiple^*_{ci} = \begin{cases} 0, & \text{if } c_i < c, \\ Multiple_{ci} - \hat{E}[Multiple_{ci}|X_i, c_i \geq c], & \text{if } c_i \geq c. \end{cases} \tag{B.2.2}$$

Essentially, these adjustments define the instruments as 0 in cases where they are undefined, and deviations from predicted multiple births at the given parity for families where the multiple birth instrument is defined. In practice, $\hat{E}[Multiple_{ci}|X_i, c_i \geq c]$ is obtained as the linear prediction from

[1] Mogstad and Wiswall (2016) also include a dummy for having at least 5 siblings; since the incidence of such family sizes in my sample is small, I do not include this dummy in the main analysis. When I do use the $s \geq 5$ dummy, the results are similar, although some of the models are under-identified (i.e. very low first stage F-statistics, especially for the sample of first born children). Another difference from their original approach is that they use twin births, rather than multiple births: I use the latter to increase instrument incidence.

a regression of $Multiple_{ci}$ on i) individual gender, ii) individual and parental birth year dummies, iii) mother's age at first birth dummies, iv) mother's and father's education level dummies, v) the interaction of mother's and father's education with mother's and father's age and age squared, and vi) the interaction of mother's and father's ages. This regression approximates a nonparametric estimation, as it offers flexibility with regards to age profiles (especially for the mother, whose age is a strong determinant of multiple births), and includes 177 covariates.

Once these instruments are computed, I construct the following first stage specification for the endogenous variables in equation (B.2.1):

$$d_{si} = \lambda_{s2} Multiple_{2i} + \lambda_{s3} Multiple^*_{3i} + \lambda_{s4} Multiple^*_{4i} + \rho_s X_i + \eta_{si}, \tag{B.2.3}$$

where $s \in \{2, 3, 4\}$, i.e. for number of siblings being larger or equal to two, three, or four (and family size larger or equal to three, four, or five). Empirically, I employ the above procedure three times, for i) first born children in families with at least two children, ii) second born children in families with at least three children, and iii) third born children in families with at least four children. For each of these samples, I bootstrap the whole process (with 50 repetitions) to account for the first stage estimation of the conditional mean function of the imputed multiple birth instruments.

Efficient instruments To ensure that standard error imprecision in instrumental variable models with multiple endogenous dummy variables is not exacerbated, Mogstad and Wiswall (2016) propose another refinement, in the form of 'efficient instruments'. These instruments match the inherent structure of the data, recognizing that a multiple birth must increase family size by at least one child and that the endogenous variables are dummies. For instance, in the sample of first born children, the instrument for moving from one to two siblings is given by:

$$\hat{p}_{2i} = \begin{cases} 1, & \text{if } Multiple_{2i} = 1, \\ f_2(X_i, \hat{\theta}_2), & \text{if } Multiple_{2i} = 0, \end{cases} \tag{B.2.4}$$

where $\hat{\theta}_2$ represents the estimates of the unknown parameters of function f_2. This instrument recognizes that the probability of having at least two siblings conditional on having a multiple birth at the second birth order is 1 (by definition). For those without a multiple birth, the instrument is the predicted probability that the individual has two or more siblings, restricted to the unit interval. Empirically, I estimate $f_2(X_i, \hat{\theta}_2)$ with a probit, where the covariates are i) individual and parental age and age square, ii) mother's age at first birth, iii) the interaction of parental ages, and iv) parental education level and income dummies. The instruments for going from two to three and three to four siblings are computed in a similar way, as predicted probabilities from a probit regression. Specifically, $\hat{p}_{3i} = f_3(X_i, Multiple^*_{3i}, \hat{\theta}_3)$, and $\hat{p}_{4i} = f_4(X_i, Multiple^*_{4i}, \hat{\theta}_4)$, therefore accounting for the multiple birth instruments computed in the previous section and their impact on family size. With these predicted probabilities in hand, the first stage equations become:

$$d_{si} = \lambda_{s2} \hat{p}_{2i} + \lambda_{s3} \hat{p}_{3i} + \lambda_{s4} \hat{p}_{4i} + \rho_s X_i + \eta_{si}, \tag{B.2.5}$$

where $s \in \{2, 3, 4\}$. The estimation is performed for the three different samples, and the whole process of i) computing the imputed multiple birth instruments, ii) computing the efficient instruments, and iii) estimating the instrumental variable model is bootstrapped (with 50 repetitions).

Results Table B.2.1 shows the results of the non-linear instrumental variable estimation, with the imputed multiple birth instruments in Panel A, and the efficient instruments in Panel B. The first stage F statistics are above 100 for both types of instruments, and the higher match between the structure of the data and the efficient instruments is reflected in the slightly smaller

Table B.2.1: **Non-Linear Family Size Effects**

	First born children, family size ≥ 2		Second born children, family size ≥ 3		Third born children, family size ≥ 4	
	Uninc. (1)	Inc. (2)	Uninc. (3)	Inc. (4)	Uninc. (5)	Inc. (6)
A. Imputed multiple birth instruments						
Family size ≥ 3	-0.0026	0.0007				
	(0.0184)	(0.0128)				
Family size ≥ 4	0.0046	0.0461*	0.0233	0.0238*		
	(0.0276)	(0.0258)	(0.0176)	(0.0136)		
Family size ≥ 5	-0.0296	-0.0970***	0.0355	-0.0703***	0.0012	-0.0023
	(0.0640)	(0.0232)	(0.0518)	(0.0216)	(0.0238)	(0.0180)
F-statistic	113.40		400.51		7,027.46	
N	224,336		108,102		38,880	
B. Efficient instruments						
Family size ≥ 3	0.0098	0.0049				
	(0.0180)	(0.0133)				
Family size ≥ 4	0.0260	0.0404	0.0198	0.0192		
	(0.0331)	(0.0294)	(0.0131)	(0.0146)		
Family size ≥ 5	0.0362	-0.1692***	-0.0328	-0.0976***	-0.0256	-0.0354**
	(0.0610)	(0.0389)	(0.0401)	(0.0220)	(0.0242)	(0.0154)
F-statistic	126.07		566.10		3,984.79	
N	224,336		108,102		38,880	
Sample mean	0.1428	0.0862	0.1488	0.0847	0.1517	0.0750
	(0.3499)	(0.2807)	(0.3559)	(0.2785)	(0.3587)	(0.2635)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Bootstrapped standard errors in parentheses (with 50 repetitions). All models control for gender, own and parental birth year dummies, mother's age at first birth, parental education, entrepreneurship, and income. Kleibergen-Paap F-statistics reported; the first stage is the same for unincorporated and incorporated entrepreneurship.

standard errors (though this is not always the case). For unincorporated entrepreneurship, the marginal effects of family size are economically small and statistically insignificant, regardless of the instruments used. For incorporation, most marginal effects of family size are not statistically significant; however, children in families with more than four children are less likely to become incorporated, and the effects are economically and statistically significant.[2] Indeed, this may explain the significant negative effect for larger families in Panel C, column (6) of Table 3.3 (for all children) and Panel C, column (4) of Table 3.4 (for men).

While this result does not overturn the absence of family size effects across a wide range of specifications, it does appear that for very large families, resource constraints bind with regards to incorporated entrepreneurship. On the one hand, a larger family may limit parental investments in individuals' human capital, preventing them from pursuing higher-growth ventures; on the other hand, individuals' ability to rely on parents' financial resources in their entrepreneurial ventures may be lower when more children are present in the household.

[2] As another robustness check of the main results, using these instruments in a linear IV model where I pool the three different samples produces estimates of -0.0072 (standard error 0.0086, $p = 0.402$) for unincorporated entrepreneurship, and -0.0112 (standard error 0.0067, $p = 0.095$) for incorporated entrepreneurship.

Chapter 4

Striking a Balance: Revenue Drift, Incentives, and Effort Allocation in Social Enterprises

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4.1 Introduction

As hybrid organizations combining commercial and social logics (Austin et al., 2006; Dacin et al., 2011; Pache and Santos, 2013), social enterprises must allocate scarce employee effort between commercial and social mission tasks in order to deliver on their dual objectives (Battilana and Dorado, 2010; Battilana and Lee, 2014; Besharov, 2014).[1] In practice, many social enterprise employees prioritize purpose over profits, creating the risk that insufficient effort is allocated to promoting economic performance (Battilana et al., 2015; Stevens et al., 2015; Staessens et al., 2018). An excessive focus on social impact to the detriment of generating revenue – or 'revenue drift' (Ebrahim et al., 2014) – may ultimately threaten social enterprises' financial sustainability and survival (Tracey et al., 2011; Smith et al., 2013).

In such circumstances, we might expect social enterprises to offer pecuniary rewards to induce employees to balance their effort between commercial and social tasks. Yet, social enterprises rarely make use of monetary incentives (Battilana and Lee, 2014). On the one hand, potential employees may perceive a tight coupling of pay and commercial performance as incompatible with social enterprise values and resources (Austin et al., 2006; Tracey et al., 2011; Besharov, 2014). Identity tension is especially likely if employees associate monetary rewards with the competitive 'bonus culture' and profit motive traditionally characterizing a commercial logic (Dees, 2012; Benabou and Tirole, 2016; Dimitriadis et al., 2017). On the other hand, social entrepreneurs may be anxious about the danger of mission drift, where mission-driven organizations emphasize commercial over social imperatives (Ebrahim et al., 2014), thereby choosing to eschew monetary incentives (Bacchiega and Borzaga, 2001).

In this paper, we question whether these concerns are well-founded. As social sector organizations face increasing competition for talent, pressure to professionalize, and market competition

[1] Social enterprises occupy a continuum along the trade-off between economic and social goals (Besharov and Smith, 2014; Shepherd et al., 2019); our focus is on firms where this trade-off is most pronounced, i.e. where multiple logics are core to organizational functioning and provide contradictory prescriptions for action.

(Hwang and Powell, 2009; Smith et al., 2013; Battilana and Lee, 2014), many hybrids are turning their attention to the financial sustainability of their operations and the role of staffing practices in meeting their dual objectives (Battilana and Lee, 2014; Smith and Besharov, 2019). Thus, the role of incentives cannot be dismissed outright, especially for social enterprises whose finances are precarious. Specifically, we ask: can monetary incentives for commercial performance help social enterprises achieve a more balanced allocation of employee effort between commercial and social tasks? If so, how sensitive is effort allocation to monetary rewards – in other words, how 'steep' do incentives need to be to attain a balanced effort allocation? To answer these questions, we conceptually theorize about and conduct an experiment to distinguish between two mechanisms that affect the balance of commercial and social tasks. The first mechanism relates to the attention-directing role of incentives (Ocasio, 1997), which shifts the effort allocations *of a given workforce* (an intensive margin effect). The second relates to a *changing workforce composition* (an extensive margin effect) (Lazear, 2000), acknowledging that less socially motivated individuals may self-select into social enterprises that offer high-powered incentives.

We draw on organizational theory and organizational economics (Eisenhardt, 1989; Kaplan and Henderson, 2005) to analyze these questions conceptually. Given the predominance of socially motivated employees and a mission emphasis in social enterprises (Miller et al., 2012; Smith et al., 2013; Besley and Ghatak, 2017), an absence of pay for performance leads to an unbalanced effort allocation, with employees favoring social impact over revenue generation. In a multitasking framework (Holmström and Milgrom, 1991), workers' high levels of intrinsic motivation induce adverse specialization (MacDonald and Marx, 2001), endangering social enterprises' financial performance. We argue that modest incentives (i.e. small performance bonuses) can restore the balance of employee effort between commercial and social tasks, via action on *both* the intensive and extensive margins. However, we expect that strong incentives (i.e. large bonuses) lead to mission drift by distorting effort too far in the direction of commercial imperatives.

We conduct an incentivized, real-effort, online experiment to test our theoretical predictions. This approach overcomes the limited availability of data on compensation practices and individual effort allocation in social enterprises and the endogeneity of pay for performance adoption decisions; moreover, it allows us to unpack the mechanisms relating effort allocation and incentives. Our experiment uses a labor market framing, where subjects are asked to behave as employees of companies whose descriptions match typical for-profits, non-profits, and social enterprises. Employees move a set of sliders (Gill and Prowse, 2012) to allocate effort between a commercial and a social task, associated respectively with own and 'good cause' payoffs. To disentangle the causal mechanisms underlying our hypothesized effects, we manipulate the strength of commercial performance incentives (i.e. own payoffs) within social enterprises and employees' ability to choose their preferred contract, keeping good cause payoffs constant. Our hypotheses are broadly supported. Monetary incentives elicit a more balanced effort allocation, regardless of their steepness, whereas we find a small, but significant downward shift in employees' social motivation when strong incentives are offered.

The paper makes three main contributions to the organization science literature. First, we extend the logic of incentive theory to social enterprises and highlight the adverse specialization problem stemming from an excessive mission emphasis. Pecuniary rewards allow firms to overcome revenue drift by focusing employee attention on generating the revenue required for delivering their social mission (Tracey et al., 2011; Smith et al., 2013; Stevens et al., 2015). We hope our conceptualization of the social enterprise as a multitasking setting with motivated workers can aid future theory building efforts in the area of hybrid organization hiring and socialization practices, as well as organizational design (Battilana and Lee, 2014).

Second, we implement a novel experimental design which causally isolates the normative role of incentives (Ocasio, 1997; Kaplan and Henderson, 2005; Ethiraj and Levinthal, 2009; Wolfolds, 2018) from their effect on workforce composition (Lazear, 2000; Cadsby et al., 2007; Deserranno,

2019). Contrary to a common belief among social enterprise actors that monetary rewards may be perceived as unfair, controlling, or incongruent with organizational values (Austin et al., 2006; Tracey et al., 2011; Dees, 2012), our experimental results show that pay for performance need not deter socially motivated workers from joining social enterprises. Instead, (modest) incentives work mainly by directing attention towards commercial tasks.

Third, we trace out the implications of our findings for social entrepreneurs. By embracing heterogeneity in other-regarding values (Miller et al., 2012; Stevens et al., 2015; Besley and Ghatak, 2017) and considering the mechanisms through which incentives affect effort (Cadsby et al., 2007), we show that pay for performance does not necessarily crowd-out social motivation by workers and cause mission drift. This challenges a common view and normative pressure in the social enterprise community that monetary rewards should be avoided (Bacchiega and Borzaga, 2001). Therefore, we tentatively recommend that social entrepreneurs worried about revenue drift and adverse specialization should consider incorporating modest incentives into their worker compensation schemes, alongside socialization practices (Battilana and Dorado, 2010) and other governance mechanisms (Ebrahim et al., 2014; Smith and Besharov, 2019).

4.2 Theoretical Background

Social enterprises are double-bottom line organizations. They aim to deliver a social mission, as well as the financial performance which enables them to survive and prosper, and so advance their social mission. In what follows, we study the allocation of social enterprise employees' effort between these two objectives. We first characterize the tensions these 'dual mission' organizations often encounter, before clarifying definitions and assumptions about effort, employee motivation, incentives, and actions within such hybrid organizations. We then discuss effort allocation in the absence and presence of financial incentives offered by social enterprises.

4.2.1 Social and commercial tensions in social enterprises

The social enterprise hybrid organizational form tackles social challenges through business means (Dees, 2001; Margolis and Walsh, 2003; Zahra et al., 2009). Despite the diversity of their business models (Mair et al., 2012), for-profit social enterprises represent a common organizational form, with clearly stated economic and social goals, or a *double bottom line* (Austin et al., 2006; Dacin et al., 2011; Stevens et al., 2015). In 'integrated' hybrids, profit and purpose are pursued jointly, often through direct transactions with beneficiaries (Smith et al., 2013; Besley and Ghatak, 2017; Eldar, 2017).[2] However, profit and purpose represent goals associated with commercial, for-profit and charitable, non-profit institutional logics, respectively (Pache and Santos, 2010; Battilana and Lee, 2014; Besharov and Smith, 2014). Thus, they often place conflicting demands on organizations' attention and resources (Dacin et al., 2011; Stevens et al., 2015).

The trade-off between commercial and social goals is reflected at all levels of the organization, as employees decide how to allocate their effort. The following examples from social enterprises with different missions illustrate this trade-off. Loan officers in commercial microfinance organizations must balance loan size, interest rates, and potential profits on the one hand, with reaching the underprivileged target population on the other (Battilana and Dorado, 2010; Pache and Santos, 2010; Besharov and Smith, 2014; Canales, 2014; Wry and Zhao, 2018). In work integration social enterprises (WISEs), employees must ensure financial sustainability through business activity, as well as disadvantaged workers' skill development (Tracey et al., 2011; Pache and Santos, 2013; Battilana et al., 2015). For 'base of the pyramid' firms, employees must ensure products are sold at prices above cost, yet within reach of the target population (Hockerts, 2015; Santos et al., 2015). Finally, environmental social enterprises must balance generating revenue with costly measures to protect the environment (Pacheco et al., 2014).

[2] By contrast, 'differentiated hybrid' social enterprises rely on cross-subsidization, where commercial revenue is devoted to a social goal (Baron, 2007; Ebrahim et al., 2014). For instance, TOMS Shoes uses its profits to provide shoes and fund investments in hygiene in developing countries (Marquis and Park, 2014).

4.2.2 Terminology and assumptions

Before developing our hypotheses, it is helpful to define terms and clarify the setting we analyze. First, we assume that social enterprise employees must decide how to allocate their effort between two tasks. One task delivers the commercial objective C, e.g. generating sales; the other delivers the social mission S, e.g. alleviating poverty. If a fixed and finite amount of effort is available, one can equivalently analyze the proportion of time a worker devotes to one task rather than the other. We assume that spending more time on one task necessarily means spending less time on the other,[3] and refer to *balanced effort* as allocations in which roughly similar amounts of effort are allocated to each task. In line with standard principal-agent theory, we assume that managers cannot perfectly measure and direct how employees allocate their effort, which is partly at workers' discretion (Battilana and Dorado, 2010; Henderson and Van den Steen, 2015).

Second, we assume there are two types of employees. The I type is intrinsically motivated by the social enterprise mission and has a strong desire to do good for others. The E type is primarily extrinsically motivated (i.e. by money). I types are more predisposed than E types to allocate effort towards the S task and more willing to join non-profits or social enterprises rather than for-profit firms. Another standard principal-agent theory assumption is that founders are imperfectly informed about which type of employee is which; that is, there are 'hidden types'. This rules out job separation arrangements within the organization whereby different worker types could be assigned to work solely on the C or S task.[4] All employees are assumed to pursue both tasks, due to social enterprises' integrated hybrid nature and typically small scale and staffing constraints (Smith et al., 2013; Santos et al., 2015; Battilana, 2018). For the social enterprise, commercial and social tasks are complements (MacDonald and Marx, 2001).

[3] We assume a capped level of total effort due to our core focus on effort allocation. While our experiment allows subjects to also adjust this margin, we find no differences in total effort across treatments.

[4] One WISE studied by Battilana et al. (2015) does practice job separation and enforces 'spaces of negotiation', but this approach risks disconnecting different areas of the organization (Ethiraj and Levinthal, 2009). Another possibility is non-separability of missions within job assignments, i.e. both missions are necessarily present simultaneously; yet workers have discretion over which to emphasize.

Third, we consider two types of incentives. Incentives are an integral part of firms' 'structural distribution of attention' (Ocasio, 1997) and perform a normative function, directing employee effort (Kaplan and Henderson, 2005). Pecuniary rewards can ensure complex organizations reach their goals even when not all outcomes can be measured accurately (Ethiraj and Levinthal, 2009). We distinguish between 'low-powered' and 'high-powered' incentives. Low-powered incentives consist of a fixed wage unrelated to the allocation of effort to, or performance in, either task (Holmström and Milgrom, 1991). By contrast, high-powered incentives are provided by a combination of fixed wage and pay-for-performance on a given outcome, occupying a continuum ranging from 'modest' incentives (i.e. a small bonus tied weakly to task performance) to 'strong' incentives (i.e. a large bonus tied closely to task performance). In principle, high-powered incentives can be attached to either task (Wolfolds, 2018), but in practice tend to be applied to the C task only, given the difficulty of measuring social impact – the output of the S-task – in a timely and standardized manner (Austin et al., 2006; Dacin et al., 2011).

Fourth, incentives affect one or two margins of action, which economists label 'intensive' and 'extensive'. The intensive margin relates to how incentives affect the chosen effort allocation *by a given pool* of employees. The extensive margin relates to how incentives affect the *composition* of the social enterprise's pool of employees itself. Along this margin, employees can self-select into either social enterprises or an alternative organizational form. For example, I types are more likely to select into social enterprises or non-profit organizations than E types, all else equal; the latter may select instead into for-profits (Barigozzi et al., 2018).

4.2.3 Low-powered incentives in social enterprises

We propose that social enterprise employees respond to incentives in ways that affect both the intensive and extensive margins. We first consider the case where low-powered incentives prevail. This is the predominant case in practice: relatively few social enterprises offer financial bonuses for commercial performance (Bacchiega and Borzaga, 2001; Becchetti et al., 2013).

Most social enterprises are founded by compassionate individuals seeking to maximize the social return on their investments (Miller et al., 2012; Santos, 2012; Grimes et al., 2013; Bacq and Alt, 2018). Their emphasis on social mission attracts similarly-minded employees (Besley and Ghatak, 2017), who prefer to exert high levels of (S task) effort (Besley and Ghatak, 2005; Bell and Haugh, 2014; Gerhards, 2015; Henderson and Van den Steen, 2015; Tonin and Vlassopoulos, 2015; Burbano, 2016; Cassar, 2019).

Moreover, social enterprise founders and employees often have experience working within an institutional logic where 'doing good' trumps 'doing well' (Battilana and Dorado, 2010; Besharov, 2014; Hockerts, 2017). A social sector background may thus steer their effort allocation decisions, as 'dangerous idealists' emphasize the 'social' rather than the 'enterprise' aspect of the organization (Battilana and Dorado, 2010; Bacq et al., 2016). In addition, despite often acknowledging the importance of market mechanisms for financial sustainability, social enterprises rely heavily on the mission they champion to attract potential customers, investors, and employees (Dees, 2001; Renko, 2013; Smith et al., 2013; Fosfuri et al., 2016).

Social enterprises may offer low-powered incentives for at least two reasons. First, incentive theory suggests that high-powered incentives for the C task risk shifting employees' effort away from the less measurable S task and towards the more-easily measured and rewarded C task (Kerr, 1975; Holmström and Milgrom, 1991). This represents an intensive margin effect. Founders may be averse to this shift on the grounds of it being perceived as incongruent with organizational values (Tracey et al., 2011; Hossain and Li, 2014; Andersson et al., 2017). Second, founders may worry that high-powered incentives attract financially-motivated employees uninterested in the S task (Bacchiega and Borzaga, 2001; Austin et al., 2006). This extensive margin outcome risks causing 'mission drift', whereby the enterprise abandons social concerns in favor of profit-seeking activities (Ebrahim et al., 2014). Founders may object to this outcome if it threatens to change the composition and hence the long-term purpose of the enterprise.

What are the implications of eschewing high-powered incentives for social enterprises? Beyond the motivational effects of missions, organizational economics suggests that I types have a lower psychic cost of effort for the S task (Murdock, 2002; Schnedler, 2008). In the absence of incentives to do otherwise, their effort allocation is distorted towards the less costly task, leading to 'adverse specialization' (MacDonald and Marx, 2001). For social enterprises, the risk is that I employees exert too much effort on the S task and too little on the C task, reducing financial performance and hence limiting the resources the enterprise needs to survive (Smith et al., 2013). Ebrahim et al. (2014) refer to this outcome as 'revenue drift'.

There are numerous examples of revenue drift. Tracey and Jarvis (2006) and Tracey et al. (2011) discuss how a social enterprise tackling homelessness threatened its own survival by focusing excessively on beneficiary needs at the expense of required operational investments. Beer et al. (2017) document how employees of a youth development social enterprise focused on ensuring disadvantaged individuals had adequate housing, but overlooked the importance of rent collection for keeping the business going.[5] A fair trade social enterprise insufficiently heeding customer demands and prioritizing producers instead suffered from operational difficulties for several years, limiting their ability to serve beneficiaries (Davies and Doherty, 2018). In analyzing Belgian WISEs' productivity, Staessens et al. (2018) find that these companies predominantly use inputs to achieve worker reintegration outcomes rather than to generate revenue, results echoed by Battilana et al. (2015) and Stevens et al. (2015). Overall, social enterprises' cash flow problems, often listed as an impediment to growth along with lack of access to finance, appear partly attributable to an excessive focus on social impact (Smith et al., 2013).

This raises the question of whether high-powered incentives – coupling pay with financial performance – can be used to avoid the adverse specialization problem, helping social enterprises

[5] One housing worker notes: "*We need to get support workers on board with the importance of rent collection... everyone needs to have a common message [with the young people]*". Another housing worker states: "*They [support workers] say 'Our job is not to collect rent, it is to get them on a course'. They need to understand that money is what keeps [Youth Futures] going*".

strike a more balanced allocation of effort across the C and S tasks. And if so, would modest or strong high-powered incentives work best? We explore these questions next.

4.2.4 High-powered incentives in social enterprises

Can high-powered incentives mitigate the problems caused by excessive attention to the social task and thus achieve a balanced effort allocation? To answer this question, we turn to incentive theory. If employees had no preference regarding performing C or S tasks, i.e. there was no heterogeneity along the *I-E* dimension, then insights from the classical two-task multitasking problem would apply. In that case, low-powered incentives would be appropriate, since they would lead workers to exert effort on both tasks (Holmström and Milgrom, 1991). In this setting, moreover, any kind of high-powered incentive would run the risk of dramatically unbalancing the effort allocation, most obviously by directing all employee effort towards the easily-measured C task at the expense of the hard-to-measure S task. This would run counter to social enterprises' central purpose, which might explain why they eschew such incentives (Bacchiega and Borzaga, 2001; Ebrahim et al., 2014). However, the classical analysis needs to be modified when dealing with the more realistic case where some employees are I types and some are E types.

A formal analysis of the case where there is a mixture of E and I employees suggests that both types' effort is responsive to high-powered incentives (Canton, 2005). Such incentives signal the importance of the incentivized task (Kaplan and Henderson, 2005) and make it more financially attractive for employees to perform it (Holmström and Milgrom, 1991; Canton, 2005). In social enterprises, this is equivalent to a distortion of effort towards the C task, which may lead to mission drift. Not only are E types likely to shift their effort to the C task in response to high-powered incentives, but I types may also be induced the same way. The reason is that strong financial incentives may 'crowd out' intrinsic motivation by I types. Evidence from psychology and economics (Deci et al., 1999; Osterloh and Frey, 2000; Gneezy et al., 2011) indicates that strong incentives may be perceived to have a controlling nature, which generally undermines intrinsic

motivation for the S task. The outcome on the intensive margin can be for demoralized I types to respond by allocating too much effort to the C task and too little to the S task.

The core of our argument is that this outcome does not necessarily arise when more modest high-powered financial incentives are used. These can generate a gentler shift among I types on the intensive margin, from strong effort concentration on the S task towards a more balanced allocation between the two tasks. For the I types predominant in social enterprises, monetary incentives imply a trade-off between the benefit of additional income from commercial activities and the lower satisfaction from reduced effort dedicated to the social mission. In principle, modest incentives can locate an intermediate position along the trade-off. Modest incentives are less likely to be perceived as controlling, and may even be presented as supportive of the social mission by promoting a balanced C-S perspective towards achieving the organization's goals.

There is another, subtler way that modest incentives may generate a more balanced effort allocation along the intensive margin. Brüggen and Moers (2007) show that exposure to a social norm promoting the social mission can mitigate the 'distorting' effect of financial incentives, where the distortion is associated with under-provision of S task effort. Both E and I types can be affected by a social norm, reducing their tendency to respond to incentives by making sharp shifts in their effort allocation away from S towards C, but other-oriented workers provide a more balanced effort allocation (Nellas and Reggiani, 2015; Benabou and Tirole, 2016). In social enterprises, the S task effectively elicits social norms, while monetary incentives are associated with a dangerous, agentic 'bonus culture', deeply embedded in a commercial logic (Dees, 2012; Benabou and Tirole, 2016). Hence, modest incentives reduce the risk that employees 'over-react' and devote excessive effort to the C task, compared with strong incentives.

Together, these arguments suggest that social enterprise employees respond to modest high-powered monetary incentives by shifting part of their effort towards the C task, while continuing to expend significant effort on the S task; as a result, their effort allocation is more balanced

between tasks. In contrast, strong high-powered incentives may lead to excessive effort devoted to the C task at the expense of the S task. Thus:

Hypothesis 1. *Relative to low-powered incentives which are associated with excessive effort devoted to the S task, modest high-powered incentives generate a more balanced effort allocation between C and S tasks along the intensive margin.*

Hypothesis 2. *Relative to modest high-powered incentives which are associated with more balanced effort between C and S tasks, strong high-powered incentives generate excessive effort devoted to the C task along the intensive margin.*

In addition to directing worker effort choices along the intensive margin, incentives may also perform a sorting function, whereby more financially-motivated E types are attracted to the organization (Lazear, 2000; Cadsby et al., 2007; Benabou and Tirole, 2016). Incentives may thus operate along the extensive margin as well. Attracting E types to the social enterprise can be unwelcome to social entrepreneurs (Bacchiega and Borzaga, 2001), especially if the perceived incongruence between incentives and social mission endangers employees' identification with organizational goals (Akerlof and Kranton, 2005; Besharov, 2014; Andersson et al., 2017) — or if incentives are perceived as 'unfair' (Tracey et al., 2011). Both founders and I-type employees may express concerns over practices whose origin lies in a commercial logic and which are prevalent in for-profit companies (Lazear and Shaw, 2007; Besharov, 2014; Gerhart and Fang, 2014); and I-type employees may refrain from joining a social enterprise espousing such profit-oriented practices.

A social enterprise offering strong high-powered incentives may attract E types, by allowing them to reap personal rewards from performing the C task while exerting a smaller share of S task effort. But by recruiting more E types through this mechanism, I types may feel that their other-oriented values are under-appreciated, or even devalued, by the social enterprise, which reduces their ability to identify with the organization and their intrinsic motivation (Akerlof and Kranton, 2005; Besharov, 2014). Hence, strong high-powered incentives may deter I types from joining a

social enterprise that uses such practices and encourage those currently working in ones that adopts such incentives to quit. The overall outcome is to change the composition of the social enterprise workforce, reducing the proportion of employees focusing more on the S task and increasing the proportion of employees focusing more on the C task.

By contrast, modest high-powered incentives place a limit on in-selection of E types into the social enterprise, as well as out-selection of I types. Loosely coupled pay-for-performance may be only weakly attractive for E types, if they can still do better working in a for-profit, where strong high-powered incentives are more common. This restricts the number of E types willing to join the social enterprise, diluting their impact on the organization's goal focus, and reducing the negative impact on I type selection (Jones et al., 2018). The overall effect of a small compositional change is a slight decrease in the proportion of employees focusing more on the S task, and a slight increase in the proportion of employees focusing more on the C task. We summarize our predictions thus:

Hypothesis 3. *Relative to low-powered incentives which are associated with excessive effort devoted to the S task, modest high-powered incentives generate a more balanced effort allocation between C and S tasks along the extensive margin.*

Hypothesis 4. *Relative to modest high-powered incentives which are associated with more balanced effort between C and S tasks, strong high-powered incentives generate excessive effort devoted to the C task along the extensive margin.*

This second set of hypotheses works in the same direction as the first. The difference lies in the mechanism by which effort is affected. Whereas hypotheses 1 and 2 relate to the intensive margin, hypotheses 3 and 4 relate to the extensive margin. Our experiment is designed to discriminate between these mechanisms and identify which one is more salient in practice.

4.3 Experimental Design

Our analysis uses an online, incentivized, real effort experiment with a labor market framing. Subjects allocate effort between a commercial and a social action as hypothetical employees of different fictional companies, whose descriptions match those of typical for-profits (FP), non-profits (NP), or social enterprises (SE). These firms, or 'contracts', provide similar services but have different objectives, corresponding to a realistic labor market choice. For instance, a workforce integration SE contract is described as follows: "Imagine you are working for a company aimed at reintegrating long term unemployed people into the workforce by hiring them to provide garbage collection services that are then sold on the market. It is in the best interest of the organization that both ensuring the professional development of the long term unemployed and generating revenue through the sale of services receive attention from employees". The equivalent FP and NP company descriptions emphasize revenue generation by providing services (the C task) and, respectively, the charitable goal of improving disadvantaged groups' welfare (the S task). Both commercial and social actions are available to subjects in each contract and are described in relation to the services the company provides, together with the payoffs they generate.

The experiment consists of four parts, summarized in Table 4.1. In Part 1, subjects choose the good cause they can earn money for throughout the experiment (i.e. the good cause payoff) and which provides the context for the fictional firms, ensuring the salience of the social task. In Part 2, all subjects perform the effort allocation task under each of the three different contracts (FP, NP, SE), displayed randomly to avoid order bias effects. Worker self-selection is not possible in this setting, allowing us to study intensive margin effects by varying the SE bonus between (randomly selected) subjects. In Part 3, subjects pick their preferred contract from those encountered in Part 2 and perform the effort allocation task again, allowing us to study the effect of incentives along the extensive margin. Finally, in Part 4 we collect information about demographics and social preferences. We describe our design below and provide the instructions in Appendix C.1.

Table 4.1: **Experimental Design**

	Actions and measures	Own payoff (C task)	Good cause payoff (S task)
Part 1	Choose good cause		
	Practice slider task		
	Comprehension check		
Part 2	FP contract	£1	£1
	NP contract	£0	£1
	SE contract	£0/£0.25/£0.50/£1	£1
Part 3	Choose preferred contract		
	FP contract (if selected)	£1	£1
	NP contract (if selected)	£0	£1
	SE contract (if selected)	£0/£0.25/£0.50/£1	£1
Part 4	Demographics		
	Social preferences		
	Compassion: sub-scale of public service motivation scale		
	Altruism: incentivized £10 dictator (giver)		
	Inequality aversion: hypothetical £10 ultimatum (receiver)		
	Hypothetical altruism: hypothetical £1,000 lottery		
	Willingness to share: without expecting anything in return		
	Prosocial behavior: observed prosocial behavior		
	Risk and time preferences		
	Attention check		
	Manipulation check		

Recruitment: Prolific Academic, a UK-based online platform (link)
Stratification: by gender, for comparison purposes
Target number of subjects: 800 in total, 200 per bonus level, 100 per gender × bonus level
Restrictions: UK resident, ages 18-64, active labor force, prior approval rate > 90%
Participation fee: £3 for 15-20 minutes
Bonuses: 5% or 1 in 20 subjects, up to £80, from slider task and dictator (giver/receiver)
Good causes: The Big Issue Foundation, Fairtrade Foundation, Water Aid
Preregistration: Open Science Framework (link)

We randomize the order in which contracts are displayed in Part 2 and the order in which the options are presented in Part 3. The choice of good cause determines the charity that the good cause payoffs will be donated to, but also the organizational mission that the fictional (social) enterprises will pursue, i.e. workforce reintegration, fair and equitable trade, and water quality and environment. We restrict subjects' prior approval rate on the platform to be larger than 90% to ensure high-quality answers. Note that subjects only see company descriptions, but not the FP, NP, or SE labels.

Task We use a real effort task adapted from the slider task introduced by Gill and Prowse (2012) to measure effort allocation. This task consists of 15 horizontal sliders that can be moved to positions equivalent to exerting effort on the commercial or social task. The sliders are labeled from 0 to 100 and initially positioned at 50. The commercial task, C, requires placing the slider

at 25 and carries a payoff for the subject, determined by the commercial task incentive level (or 'bonus') in a given contract. The social task, S, requires placing the slider at 75 and carries a payoff for the selected good cause.[6] Each slider has a number to its right, showing its current position. Subjects can move the mouse in any way they like to drag sliders, and can readjust the position of each slider as many times as they wish.

Most importantly, subjects do not simply state how they would like to allocate effort, but must drag sliders in a way that reflects their preferences, such that they expend real effort. While neither action has a monetary cost, both tasks require the subject to physically move a slider. In addition, the social task entails foregoing the monetary rewards of the commercial task. It is in this sense that the slider task allows us to capture real effort.

Before each slider task is carried out, we explain how payoffs are calculated. In addition, we inform subjects that each slider offers a commercial task bonus between £0 and £1 when we initially describe the task. As the maximum possible bonus per slider is thus known, subjects are aware when a social enterprise uses strong incentives (£1), allowing us to detect extensive margin effects. We measure *Commercial effort* and *Social effort* as counts of the number of sliders moved to the C and, respectively, S task. The more similar *Commercial effort* and *Social effort* are, the more balanced the effort allocation.

Treatments The experiment features two different treatments, designed to tackle i) the effect of incentives on effort allocation, and ii) the contributions of the extensive and intensive margins. First, beyond the participation fee (equivalent to a fixed wage), we vary the strength of the SE C task incentive between subjects. At one extreme, the SE contract offers 'low-powered incentives' (£0 bonus), in line with current social enterprise practice and identical to the NP contract; at the

[6] For example, in the workforce reintegration mission in the £0.50 treatment, the commercial task is described as follows: 'By placing the slider exactly at position 25 you can generate revenue for the company through the sale of services; each slider you position at 25 will give you a payoff of £0.50'. The equivalent social task is described as follows: 'By placing the slider exactly at position 75 you can contribute to the professional development of its employees; each slider you position at 75 will give a payoff of £1 to the good cause'.

other extreme, the SE contract offers the same 'strong high-powered incentives' (£1 bonus) as the FP contract; we refer to intermediate levels as 'modest high-powered incentives' (£0.25 or £0.50 bonus). Subjects are randomly assigned to a fixed incentive level, which they face throughout the experiment. Second, we allow subjects to choose their preferred contract from the previously encountered FP, NP, and SE contracts. That is, Part 3 incentives and company descriptions are the same as in Part 2, but we now allow for individual sorting across company types (captured by the binary variable *Sorting*). We fix the FP bonus at £1, the NP bonus at £0, and the good cause payoff in all contracts at £1 per slider throughout the experiment. The £1 FP bonus allows us to benchmark SE incentive strength (i.e. the SE bonus is 0%, 25%, 50% or 100% of the FP bonus) across the range of possible incentives. The £1 good cause payoff reflects the high social returns to S task effort and is kept constant across contracts to ensure that the only difference between the various contract terms is the extent to which commercial effort is financially rewarded (which is the main focus of our experiment).

Preferred Mission To ensure S task saliency, we allow subjects to choose their preferred good cause (Tonin and Vlassopoulos, 2015; Cassar, 2019). The options available – The Big Issue Foundation, Fairtrade Foundation, and Water Aid – are selected as charities whose goals match representative social enterprise missions, namely workforce reintegration, fair and equitable trade, and environmental protection (Mair et al., 2012; Eldar, 2017). Moreover, these missions have a clear multitasking component and can be pursued through a for-profit, non-profit, or social enterprise model, allowing us to construct realistic descriptions of the FP, NP, and SE contracts. Furthermore, the actual charity organizational form allows us to credibly commit to donating the good cause payoffs generated by subjects in the experiment.

Social Preferences As our theoretical framework suggests that individual social motivation affects both effort allocation and self-selection, we elicit subjects' social preferences in various

ways. Following the social entrepreneurship literature (Miller et al., 2012; Grimes et al., 2013), we measure *Compassion* using Perry's (1996) compassion scale, a sum of eight items on a five-point scale.[7] For example, one item asks subjects how strongly they agree with the following statement: "I seldom think about the welfare of people whom I don't know personally". We measure *Altruism* in a standard, incentivized dictator game, where subjects decide how to split a £10 endowment with another randomly paired subject (Galizzi and Navarro-Martinez, 2018). We also use a set of hypothetical and direct questions. We measure *Hypothetical altruism*, where subjects make a hypothetical donation after winning a £1,000 lottery, self-reported *Willingness to share* with others without expecting anything in return on a 0-10 scale (Falk et al., 2016), *Inequality aversion* as the recipient's minimum acceptable amount in hypothetical ultimatum game (Fehr and Schmidt, 1999), and *Prosocial behavior* through past social sector experience (Tonin and Vlassopoulos, 2015).

These measures capture different aspects of motivation, so we aggregate them into a composite *Social motivation* measure using principal component analysis. This approach produces a single factor with eigenvalue larger than 1, explaining more than 80% of variance, and on which *Compassion*, *Hypothetical altruism*, and *Willingness to share* load strongly (see Appendix C.5). For parsimony, we discuss *Compassion* and *Social motivation* in our main analysis, and report results for other measures as robustness checks. We also measure self-reported risk taking (*Risk*) and future discounting (*Time*) preferences on a 0-10 scale (Dohmen et al., 2011; Falk et al., 2016), as they may impact both individuals' perception of incentives and their effort allocation.[8]

Procedure We recruit subjects on Prolific Academic, a UK-based online platform geared towards researchers and startups and designed for surveys and experiments. This platform produces similar response times and data quality as Amazon Mturk, but gives access to more diverse and representative respondents (Peer et al., 2017). Social enterprises are an established organizational

[7] These items, with Cronbach's $\alpha = 0.75$, are available in Appendix C.1.

[8] The self-reported answers to the general risk, time, and willingness to share questions are reliable predictors of behavior and consistent with incentivized elicitations (Dohmen et al., 2011; Falk et al., 2016).

form in the UK (Tracey et al., 2011) and an online study allows us to reach a broad population of potential employees (Bitektine et al., 2018). We restrict the subject pool to UK residents aged 18-64 who are students or active in the labor force (i.e. not 'homemakers', disabled, or retired). To achieve gender balance and perform comparisons between men and women, we stratify the randomization by gender, using the gender variable Prolific previously required subjects to report. Finally, to ensure high-quality answers, we require participants to have a history of taking Prolific studies seriously (as evidenced by approved submissions in past studies) and consider eligible only those participants with prior approval rates higher than 90% on the platform; we also include attention and manipulation checks.

All subjects receive a flat £3 participation fee (around $4 or €3.50 at the time of the experiment). In addition, the sliders allow subjects to earn up to an additional £60 (15 sliders × 4 contracts × £1 per slider) and the dictator game produces own payoffs up to £20 (£10 as giver, £10 as receiver). To be able to use these rather large sums as incentives, we randomly select 40 out of the 796 subjects for bonus payment, with a maximum potential bonus of £80. Thus, we compensate some participants with larger sums but a smaller likelihood (about 1 in 20), a procedure equivalent to paying smaller sums with certainty (Charness et al., 2016).[9]

To arrive at our final sample, we impose several restrictions. First, we require subjects to have placed at most 10 sliders in an incorrect position, considering sliders placed at 23-27 and 73-77 as indicative of strong intentions to exert commercial or social effort, and therefore correct. Second, subjects must not have failed both attention and manipulation checks. Third, we require consistency between our gender variable and the Prolific variable used for stratification; in other words, a subject must have answered both questions in the same way. Finally, to ensure subjects paid attention, we require them to have completed the experiment in between 10 and 40 minutes.

[9] The expected total payoff is £7 for a duration of around 20 minutes. The maximum own payoff is attained when subjects exert only commercial effort in the £1 treatment. If subjects exert only social effort, the maximum good cause payoff is £60. In practice, the average own and good cause payoffs of selected subjects are £29 and, respectively, £33.

This leaves us with a sample of 708 subjects (out of 796 responses) for whom data quality is likely to be very high, distributed roughly evenly across treatments.[10]

4.4 Results

4.4.1 Descriptive statistics

Out of 708 subjects, 49.9% are female and 16.5% are students. Subjects are fairly well educated, cover the range of incomes, and tend to be young. The average *Compassion* score is 29.2 out of a maximum of 40 and subjects donate on average £4.2 in the dictator game; about 24.4%, 10.5%, and 16% of subjects have previous experience working in a non-profit, in a social enterprise, or with a social organization, respectively.[11] *Compassion* is positively correlated with most other social preference measures and loads strongly on *Social motivation*, together with *Willingness to share*, *Hypothetical altruism*, and *Prosocial behavior*. Subjects took on average 18.5 minutes to complete the study. 25%, 18.6%, and 56.4% of subjects chose the workforce reintegration, fair trade, and, respectively, environmental good causes, with similar choices across treatments.

4.4.2 Intensive margin: Social enterprise effort allocation

Figure 4.1 plots average social enterprise (SE) *Social effort* across incentive levels and sorting conditions, together with 95% confidence intervals. In the absence of pay for performance, the effort allocation is skewed towards the social task, as subjects exert more than two thirds of their effort on this task (i.e. more than 10 out of 15 sliders). However, subjects allocate effort roughly equally between the commercial and social tasks at all levels of pay for performance, with confidence intervals that include the level of social effort expected for a perfectly balanced effort allocation, plotted as a dashed line. Similarly, Panel A of Table 4.2 reports average *Social effort*

[10] To ensure transparency and commitment in our analysis, we preregistered our design and hypotheses within the Open Science Framework (link). Appendix C.2 provides descriptive statistics, social preference correlations, an analysis of good cause choice, and a randomization check. We find that the good cause chosen is not systematically related to individual traits and that our randomization procedure was successful. Appendix C.3 details the results of power calculations performed prior to running the experiment. The sample sizes we obtain allow us to detect relatively small changes in social effort (around half of a standard deviation) with more than 80% power at the 5% significance level. Appendix C.4 shows that results are robust to tightening or relaxing the sample restrictions.

[11] Social organizations include both non-profits and social enterprises. This variable captures professional work relationships with such an organization (e.g. as a joint venture with a for-profit).

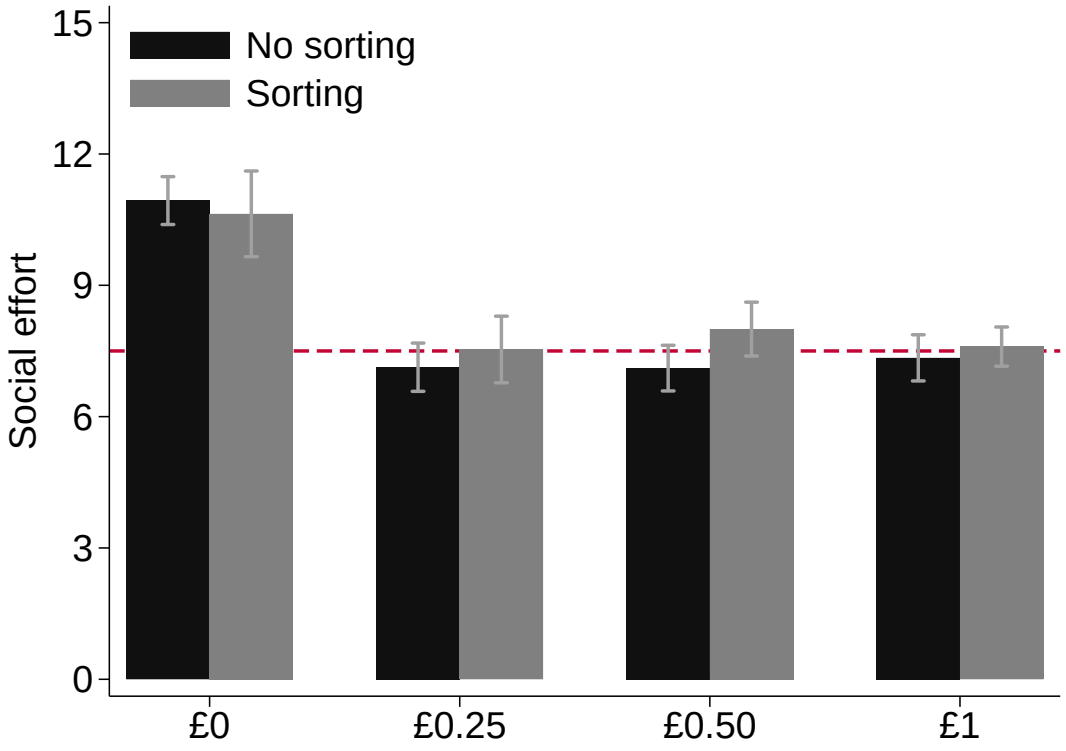

Figure 4.1: Social effort in social enterprises by treatment and sorting condition, with 95% confidence intervals; the dashed line represents a fully balanced effort allocation.

across contracts, together with the number of subjects in each condition. Using t-tests, the first row of Panel B shows that more balanced SE effort allocations are elicited regardless of incentive strength or whether self-selection is possible ($p < 0.001$). The remaining rows of Panel B show that differences in SE *Social effort* between the £0.25, £0.50, and £1 treatments are not statistically significant. These results provide initial evidence for intensive margin effects, equivalent to an attention-directing role of incentives in social enterprises.

Table 4.2 also allows us to compare behavior in different organizational forms. For profit (FP) and non-profit (NP) workers exert 30% and, respectively, 90% of their effort on the social task in each treatment; SE social effort levels are in between and significantly different from FP and NP levels ($p < 0.001$ in Panel C). To quantify where SEs lie on the FP/NP continuum, we use t-tests to compare SE social effort with the average of FP and NP social effort in Panel D, where a positive difference indicates SE is closer to NP. Indeed, we find a positive difference in the £0

Table 4.2: **Social Effort, by Contract and Treatment**

	No sorting				Sorting			
	£0	£0.25	£0.50	£1	£0	£0.25	£0.50	£1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Social effort levels across contracts								
FP	4.918	5.152	5.227	5.640	3.956	4.131	4.100	4.846
	(4.506)	(4.479)	(4.435)	(4.613)	(3.836)	(3.735)	(3.672)	(4.846)
N	170	178	185	175	90	84	70	26
SE	10.935	7.129	7.108	7.343	10.633	7.534	8.000	7.600
	(3.612)	(3.727)	(3.595)	(3.534)	(3.408)	(3.262)	(3.002)	(2.527)
N	170	178	185	175	49	73	94	125
NP	13.306	12.719	12.984	12.697	13.355	13.762	13.367	13.458
	(2.939)	(3.169)	(3.303)	(3.503)	(2.537)	(2.364)	(3.851)	(3.413)
N	170	178	185	175	31	21	21	24
B. SE effort *t*-tests of equality of means across treatments, *p*-values								
vs £0		0.000	0.000	0.000		0.000	0.000	0.000
vs £0.25			0.956	0.581			0.340	0.874
vs £0.50				0.533				0.286
C. SE effort *t*-tests of equality with FP and NP, *p*-values								
vs FP	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
vs NP	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
D. SE effort *t*-tests of equality with FP and NP average, sign and *p*-values								
	+0.000	−0.000	−0.000	−0.000	+0.001	−0.006	−0.118	−0.013

Standard deviations in parentheses. We use matched pair *t*-tests for the no sorting condition, since all individuals performed the slider task in all contract types. In the bottom row, we compare SE social effort with the average of FP and NP social effort to show where the SE lies on the continuum between FP and NP: '+' means SE is closer to NP than to FP, and '−' means SE is closer to FP than to NP. All *p*-values are two-sided.

treatment and a negative one (though not always significant) for steeper incentives. These results suggest that SEs occupy the middle ground between FPs and NPs with regards to effort allocation when monetary incentives are in place and highlight the adverse specialization problem that social enterprises face when they do not use monetary rewards.

Interestingly, company descriptions matter beyond the incentives offered, even when both commercial and social tasks are available in all contracts. For instance, the £0 bonus SE is equivalent to the NP in the contract terms offered, and the £1 bonus SE is equivalent to the FP. Nonetheless, behavior is significantly different in these organizational forms, indicating that the stated

organizational goal *per se* triggers shifts in the effort allocation.[12]

We can further probe the nature of adverse specialization: does imbalance result from a skewed effort allocation for all workers or only a minority of workers? Figure 4.2 plots the distribution of SE social effort in each treatment. Between 30% and 40% of subjects in the £0 treatment exert only social effort, whereas the distribution of social effort is roughly normal and centered around a fully balanced allocation for other subjects. Thus, adverse specialization only affects a fraction of workers, rather than shifting the entire distribution of social effort upwards. What could drive such behavior? Our theoretical framework suggests that subjects with higher other-regarding preferences are more likely to exert more social effort and, therefore, to exhibit adverse specialization. In Appendix C.6, we show that individuals in the top *Compassion* decile are indeed more likely to be in such a situation, especially if they select into the SE contract. The remaining panels of Figure 4.2 suggest that incentives, both modest and strong, successfully reduce the fraction of SE workers exerting only social effort. This results in significantly different distributions, centered more tightly around a fully balanced effort allocation.

As Hypothesis 1 predicts, modest incentives induce a more balanced effort allocation between the commercial and social tasks, alleviating the adverse specialization that occurs in the absence of monetary rewards. Surprisingly, contrary to Hypothesis 2 and a common view in social entrepreneurship, the commercial task does not gain prominence with stronger incentives: the effort allocation remains balanced even when the social enterprise pays as much as a for-profit, mitigating mission drift concerns. We now turn to analyzing the relationship between monetary rewards and the other-regarding preferences of workers attracted to the social enterprise and the potential for self-selection to influence effort allocation.

[12] Moreover, with a £0, £0.25, or £0.50 SE bonus, subjects would be better off financially by choosing the FP contract, as commercial effort is better remunerated in the FP and social effort produces the same good cause payoff (the social action in an FP could correspond, for instance, to corporate social responsibility (CSR) activities, see, e.g., Kitzmueller and Shimshack, 2012). Subjects would also be better off choosing the FP over the NP: they could choose to allocate, for example, 13 units of their effort to the social task, while still reaping personal rewards from 2 units of commercial effort in the FP contract (whereas the NP contract does not reward commercial action). Nonetheless, individuals choose organizations with a stated social mission over organizations without one, suggesting that labels matter.

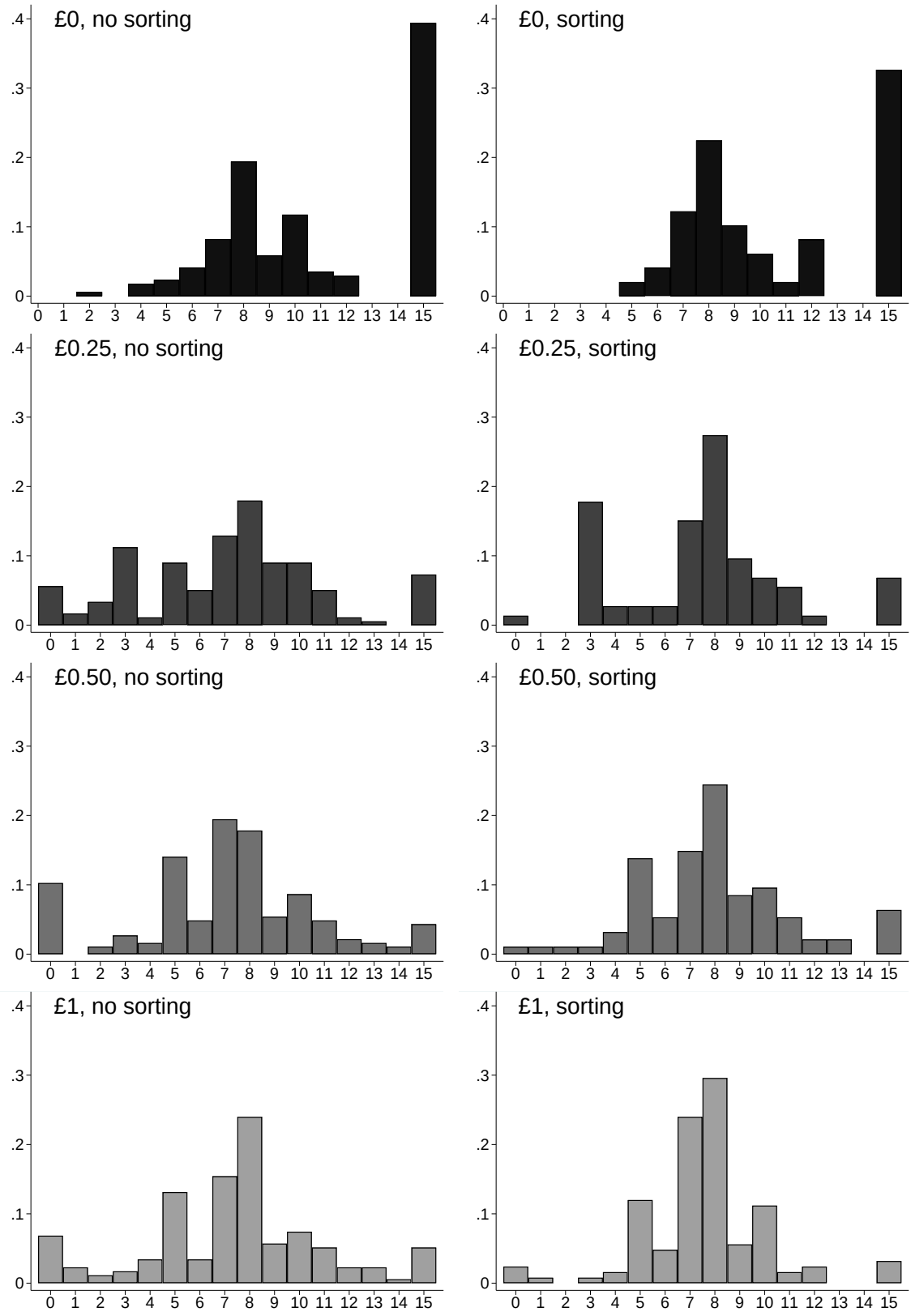

Figure 4.2: Distribution of social effort in social enterprises. The £0 distribution is different from the £0.25, £0.50, and £1 distributions (Kolmogorov-Smirnov $p < 0.001$, no sorting; $p < 0.01$, sorting). The latter distributions are not different from each other ($p > 0.1$).

4.4.3 Extensive margin: Worker self-selection

One of the reasons social entrepreneurs are reluctant to use pay for performance relates to their potential attraction of less socially motivated individuals. To assess the validity of this concern, Table 4.3 displays the average levels of *Compassion* and *Social motivation* across treatments.[13] In Panel A, we find minor and insignificant differences in mean social preferences across treatments ($p > 0.1$), suggesting our randomization was successful. Panel B then displays social preferences separately for subjects selecting into the FP, SE, and NP contracts. SE social preferences are similar for the £0, £0.25, and £0.50 treatments, but are significantly lower in the £1 treatment, as the t-tests in Panel C show ($p < 0.05$ relative to the £0 treatment).[14] This shift is consistent with our prediction that more extrinsically motivated employees are attracted to SEs when incentives in this organization are stronger. Nonetheless, high-powered incentives do not reduce the size of the pool of joiners. Instead, more workers join the SE as incentives become stronger: while a £0 SE bonus attracts 28.8% of subjects (i.e. 49 out of 170), the £0.25, £0.50, and £1 bonuses attract 41%, 50.8%, and, respectively, 71.4% of subjects, mainly at the expense of FPs.

We investigate extensive margin effects further by comparing social preferences across Part 3 contract choices in Panel D of Table 4.3. As already visible in Panel B, outside of the £1 treatment, self-selected SE workers' motivation is higher than that of FP workers ($p < 0.05$) and indistinguishable from that of NP workers ($p > 0.1$), supporting our central contention that the SE organizational form attracts highly motivated employees. However, SE workers' *Social motivation* in the £1 treatment is lower than that of NP workers ($p = 0.014$) and similar to that of FP workers ($p > 0.05$). As noted above, this is driven by a large shift in subjects choosing SE relative to FP when SE and FP contract terms are the same. In Panel E we inquire once again whether

[13] For ease of interpretation, we standardize *Compassion* to have a mean of 0 and a standard deviation of 1.

[14] F-tests for equality of variances do not indicate a wider dispersion of social preferences across treatments, but Kolmogorov-Smirnov tests for equality of distributions suggest a different distribution of *Social motivation* in the £1 treatment ($p < 0.1$). In Appendix C.5, we show that with a £1 bonus, the SE attracts more (fewer) individuals from the bottom (top) 25% of the *Social motivation* distribution, shifting the entire distribution downwards.

Table 4.3: **Motivation and Contract Choice**

	Compassion (standardized)				Social motivation (factor)			
	£0	£0.25	£0.50	£1	£0	£0.25	£0.50	£1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Motivation across contracts, without sorting								
All	0.038	-0.025	0.050	-0.065	0.020	-0.036	0.049	-0.035
	(0.953)	(1.015)	(1.086)	(0.937)	(0.752)	(0.768)	(0.827)	(0.670)
N	170	178	185	175	170	178	185	175
B. Motivation across contracts, with sorting								
FP	-0.185	-0.255	-0.266	-0.318	-0.237	-0.282	-0.254	-0.293
	(0.949)	(1.088)	(1.159)	(0.989)	(0.768)	(0.745)	(0.871)	(0.763)
N	90	84	70	26	90	84	70	26
SE	0.272	0.150	0.209	-0.031	0.292	0.196	0.222	-0.046
	(0.863)	(0.902)	(0.970)	(0.920)	(0.637)	(0.705)	(0.742)	(0.624)
N	49	73	94	125	49	73	94	125
NP	0.311	0.287	0.392	0.036	0.333	0.145	0.284	0.303
	(0.965)	(0.917)	(1.114)	(0.960)	(0.605)	(0.796)	(0.763)	(0.682)
N	31	21	21	24	31	21	21	24
C. SE motivation *t*-tests of equality of means across treatments, *p*-values								
vs £0		0.458	0.699	0.047		0.443	0.575	0.002
vs £0.25			0.692	0.177			0.815	0.012
vs £0.50				0.063				0.004
D. SE motivation *t*-tests of equality with FP and NP, *p*-values								
vs FP	0.006	0.013	0.005	0.156	0.000	0.000	0.000	0.080
vs NP	0.854	0.547	0.449	0.745	0.782	0.777	0.736	0.014
E. SE motivation *t*-tests of equality with NP and FP average, sign and *p*-values								
	+0.147	+0.320	+0.319	−0.506	+0.025	+0.025	+0.068	−0.653

Standard deviations in parentheses. We standardize *Compassion* to have a mean of 0 and a standard deviation of 1. In the bottom row, we compare SE motivation with the average of FP and NP motivation to show where the SE lies on the continuum between FP and NP: '+' means SE is closer to NP than FP, and '−' means SE is closer to FP than NP. All *p*-values are two-sided.

SEs are closer to FPs or NPs in social preferences by comparing the former with the mean of the latter. While SEs are indistinguishable from this average in terms of *Compassion*, they are significantly closer to NPs with regards to *Social motivation* when the bonus is £0, £0.25, or £0.50. Thus, the scope for extensive margin effects appears limited, with only strong incentives leading to a small, but significant decrease in SE employee social motivation. As Figure 4.1 shows, allowing for employee self-selection does not alter the relationship between monetary incentives

and *Social effort*, although self-selection leads to an effort allocation that appears closer to full balance. To formally test for differences across conditions, columns (1) and (2) of Table 4.4 regress SE *Social effort* on dummies for incentive levels, the sorting condition, and their interactions, with and without demographic and good cause controls. As some participants perform the SE contract twice, we cluster standard errors at the individual level. If the extensive margin is unimportant, we would expect the *Sorting* variable and its interaction with each treatment level to be jointly insignificant. The p-values for these tests, reported at the bottom of Table 4.4, show that sorting differentially affects effort allocation only in the £0.50 treatment, where subjects exert higher *Social effort*.

Table 4.4 shows the results for two other dependent variables. First, we account for potential differences in total effort – which may shift social effort downwards in absolute, but not necessarily relative terms – by computing social effort as a share of total effort. The results in columns (3) and (4) perfectly match those obtained using units of social effort, suggesting that total effort does not represent an important margin of adjustment, in line with our assumption in Section 4.2. Second, since one could be concerned with deviations from balance in the direction of either C or S task effort, we also consider the absolute value of the difference between social and commercial effort, where a lower value implies better balance. Columns (5) and (6) provide additional evidence against self-selection as the main mechanism by which incentives affect effort allocation; however, they suggest that sorting does contribute to better absolute balance in the £0.50 and £1 treatments.

Our analysis suggests that incentives operate similarly with and without worker self-selection and that strong incentives have a small negative effect on workers' social preferences. Thus, modest incentives have no extensive margin effects, contrary to Hypothesis 3; strong incentives may engender a loss of employee motivation, but do not skew effort towards the commercial task. While this result seemingly contradicts Hypothesis 4, our inability to confirm these hypotheses may relate to the incentive strength we use, an idea we return to when discussing limitations.

Table 4.4: **Intensive and Extensive Margin Effects on SE Effort Allocation**

	Social effort units		Social effort share		Absolute balance	
	(1)	(2)	(3)	(4)	(5)	(6)
£0.25	-3.806***	-3.934***	-0.252***	-0.260***	-2.197***	-2.389***
	(0.394)	(0.397)	(0.026)	(0.026)	(0.587)	(0.587)
£0.50	-3.827***	-3.934***	-0.254***	-0.261***	-2.570***	-2.823***
	(0.383)	(0.389)	(0.026)	(0.026)	(0.589)	(0.591)
£1	-3.592***	-3.670***	-0.237***	-0.241***	-2.717***	-2.885***
	(0.385)	(0.392)	(0.026)	(0.026)	(0.591)	(0.600)
Sorting	-0.303	-0.240	-0.020	-0.017	-0.935	-0.856
	(0.471)	(0.464)	(0.031)	(0.031)	(0.838)	(0.822)
Sorting × £0.25	0.708	0.639	0.047	0.043	0.005	0.177
	(0.588)	(0.586)	(0.039)	(0.039)	(0.972)	(0.948)
Sorting × £0.50	1.195**	1.128**	0.081**	0.077**	0.047	0.005
	(0.577)	(0.572)	(0.038)	(0.038)	(0.930)	(0.913)
Sorting × £1	0.560	0.489	0.034	0.030	-0.778	-0.837
	(0.537)	(0.531)	(0.036)	(0.035)	(0.904)	(0.886)
Constant	10.935***	11.545***	0.729***	0.770***	7.894***	7.899***
	(0.277)	(0.631)	(0.018)	(0.042)	(0.467)	(0.929)
Tests of joint significance of Sorting + Sorting × treatment, p-values:						
£0.25	0.250	0.266	0.251	0.274	0.059	0.153
£0.50	0.008	0.008	0.007	0.007	0.027	0.034
£1	0.321	0.344	0.408	0.436	0.000	0.000
Controls	No	Yes	No	Yes	No	Yes
N	1,049	1,049	1,049	1,049	1,049	1,049
R^2	0.152	0.177	0.151	0.175	0.066	0.113

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the subject level in parentheses. The baseline category is the £0 treatment when sorting is not possible. The dependent variable is *Social effort*, i.e. S task effort, in columns (1) and (2), social effort as a share of total effort, i.e. $S/(S+C)$, in columns (3) and (4), and absolute balance, i.e. $|S-C|$, in columns (5) and (6). Controls include age, gender, studentship, education, income, risk and time preferences, and choice of good cause.

In the social enterprise context, monetary rewards work mainly at the intensive, rather than the extensive margin, directing employee attention towards the remunerated commercial task in a way that generates a balanced effort allocation in absolute terms, as well as relative to other organizational forms.

Sensitivity and heterogeneity Our results are robust to a wide set of sensitivity analyses. These include alternative sampling restrictions (Appendix C.4), social preference measures (Appendix C.5), effort measures (Appendix C.6), and multiple hypothesis testing adjustments, reflect-

ing the number of outcomes and treatments we consider (Appendix C.7). As women are often found to be more other-oriented than men and more likely to engage with social, rather than commercial activities (Croson and Gneezy, 2009; Dimitriadis et al., 2017), we stratified our randomization by gender. However, we find no substantial differences between men and women in the effect of incentives on effort allocation (Appendix C.7). Our results paint a clear picture: modest incentives induce a balanced effort allocation without a reduction in social motivation.

4.5 Discussion

To achieve their economic and social value creation goals, social enterprises must allocate scarce employee effort between commercial and social tasks. Owing to their embeddedness in a social logic espousing an emphasis on social impact, a reluctance to employ practices stemming from a commercial logic, and high levels of other-regarding preferences among both founders and employees, social enterprises often pay insufficient attention to revenue generation. As purpose takes priority over profits, revenue drift hinders social enterprises' ability to deliver on their social mission and threatens their survival. Why then do so few social enterprises adopt pay for performance practices, and why does the scholarly literature pay relatively little attention to incentives in social enterprises?

We argue theoretically that in the absence of pecuniary rewards, social enterprise employees allocate most of their effort to social tasks due to their high social motivation and perceived social mission saliency. We hypothesize that monetary incentives elicit a balanced effort allocation by directing employee effort to the commercial task and by potentially attracting workers with lower levels of social motivation who are less prone to adverse specialization. While modest incentives are mainly expected to operate at the intensive margin by increasing the benefits of exerting effort on the commercial task, strong incentives may work on the extensive margin by attracting less motivated employees who could cause mission drift. Our experiment examines the effect of incentives on effort allocation in social enterprises, identifying the mechanisms through which this

effect propagates. Both modest and strong incentives are found to produce a balanced effort allocation, with employee effort split roughly equally between a commercial and a social task. While modest incentives do not affect the composition of the social enterprise workforce, strong incentives do lead to a small, but significant downwards shift in the distribution of workers' social motivation.

Theoretical implications The tension between social and economic value creation in social enterprises and its implications for firm performance are core questions in the hybrid organizations literature (Pache and Santos, 2010; Dacin et al., 2011; Smith et al., 2013; Battilana et al., 2015; Smith and Besharov, 2019). While the risk of mission drift engendered by the pursuit of commercial goals in social enterprises has received substantial attention (Ebrahim et al., 2014; Ramus and Vaccaro, 2017; Grimes et al., 2018), few studies address revenue drift beyond noting threats to economic performance when firms put purpose ahead of profits (Tracey et al., 2011; Battilana et al., 2015; Stevens et al., 2015; Staessens et al., 2018). Moreover, the usual incentive tools used by commercial ventures to guide employee effort are often viewed as inappropriate for social enterprises, due to their perceived incongruence with social impact and their potential to attract less motivated workers (Bacchiega and Borzaga, 2001; Austin et al., 2006; Tracey et al., 2011; Smith et al., 2013).

By applying incentive theory to social enterprises, we argue instead that monetary rewards can stave off revenue drift in this type of hybrid organization. Whereas existing studies show that firms often use social missions to elicit higher employee effort (Besley and Ghatak, 2005; Henderson and Van den Steen, 2015; Burbano, 2016; Cassar, 2019), the nature of that effort has been largely overlooked (Jones et al., 2018). Social enterprises relying solely on social mission to attract and motivate employees are vulnerable to revenue drift, which may create obstacles to growth and survival. By contrast, a combination of mission and monetary incentives succeeds in making workers balance their effort between commercial and social tasks.

Inasmuch as it engenders a deviation from social enterprises' core focus on social mission, an outcome of balance may still be considered a source of mission drift. However, we argue that balance will afford social enterprises the resources needed for growth, allowing them to achieve their social impact more reliably. Our results therefore reinforce the need to move away from viewing mission drift as unequivocally bad and towards appreciating the situations where it may be necessary (Grimes et al., 2018).

The experimental design developed in this article causally isolates the channels through which incentives affect effort allocation. On the one hand, monetary rewards signpost to employees what tasks are valuable for the organization; hence, they perform a normative function (Kaplan and Henderson, 2005; Ethiraj and Levinthal, 2009; Wolfolds, 2018) and are integral to firms' structural distribution of attention (Ocasio, 1997). On the other hand, incentives also perform a sorting function, potentially attracting employees with different motivations (Lazear, 2000; Cadsby et al., 2007); this can help achieve balance if motivation crowd-out is small, but may backfire if self-selected workers are extrinsically motivated and exert most of their effort on the incentivized commercial task (Deserranno, 2019). The evidence from our experiment is consistent with an intensive margin, attention-directing role of monetary rewards, with modest incentives being sufficient to balance the effort allocation without reducing employee social motivation. By conveying the importance of a given task to all employees, modest incentives act as a coordination device and are especially valuable either when firms face conflicting goals (Kogut and Zander, 1996), or when a dominant logic prescribes and legitimates non-optimal practices (Lounsbury, 2007). In social enterprises, the dominant logic tends to be marked by a deeply entrenched culture of social impact in a context where there are substantial gains to the joint pursuit of profits and purpose. This corresponds to a setting where a powerful informal organization must be countered with an 'inconsistent' formal organization in order to achieve multiple, incompatible objectives, a process Gulati and Puranam (2009) refer to as 'compensatory fit'. Under the threat of revenue

drift, social enterprises adopting performance-contingent rewards are indeed able to pursue such a compensatory fit strategy.

As hybrid organizations tackling social challenges through business means (Dees, 2001; Zahra et al., 2009), social enterprises need to guide employee effort to tasks that place conflicting demands on their attention by relying on both mission and monetary incentives. The need to make dual logic-consistent decisions leads social enterprises to 'selectively couple' practices prescribed by both commercial and social logics (Pache and Santos, 2013). The more consistent their message and practices, the more likely social enterprises are to achieve their dual goals. Given differences in social enterprise missions and ability to measure commercial and social outcomes, a multitasking framework may help us understand the practices social enterprises adopt, as well as the business models most exposed to a risk of revenue or mission drift.

Makadok and Coff (2009) show how competing actions lead organizations to adopt hybrid governance forms combining features of markets and hierarchies. However, they note that some tasks are actually synergistic for employees, such that levers which increase effort in one dimension have positive spillovers on effort in other dimensions. Indeed, social enterprise business models cover both competing and reinforcing social and commercial tasks (Besharov and Smith, 2014; Shepherd et al., 2019). Scholars may find it useful to adapt the multitasking framework to systematically study the relations between mission, services offered, the nature of tasks employees must perform, and the practices – drawn from both commercial and social logics, with elements of markets and hierarchy – enacted by social enterprises.

Our findings speak to other settings where complex organizations pursue multiple goals and workers are heterogeneous (Ethiraj and Levinthal, 2009). Innovative firms must balance exploration and exploitation (March, 1991), and employees vary in their pecuniary and non-pecuniary motives (Sauermann and Cohen, 2010). Inventors with strong preferences for intellectual challenge, novelty, or independence may then put exploration ahead of exploitation, with potentially

adverse consequences for firm performance when gains from ambidexterity are large (He and Wong, 2004). In this case, monetary rewards are expected to shift workers' attention towards exploitation (Manso, 2011; Ederer and Manso, 2013; Lee and Meyer-Doyle, 2017), and modest incentives may prove optimal (see also Baumann and Stieglitz, 2014).

Practical implications Facing growing competitive pressures, social enterprises are increasingly turning their attention to operational sustainability. Monetary rewards offer one way for social enterprises to overcome an excessive focus on social impact to the detriment of revenue generation. Modest incentives are particularly appealing, as they expand the potential employee pool and restore balance with minimal cost. Modest incentives are also unlikely to reduce employee social motivation and increase risk taking or myopic behavior, and may thus be expected to have minimal adverse effects on organizational activities outside the scope of our study. Rather than attracting less motivated workers or over-emphasizing the relevance of commercial tasks, modest incentives signal the value of certain actions to employees. Consequently, social entrepreneurs need not worry too much about the pool of potential applicants, but devote attention instead to internal effort allocations and compensation design. As our results show, a social enterprise that pays employees larger bonuses can attract more candidates, such that incentives may aid social enterprises in their search for talent. Moreover, social entrepreneurs who can overcome their aversion to performance-based pay may be able to credibly commit to market-based goods or service delivery. Redirecting a fraction of employees' attention towards revenue generation can help social enterprises become less dependent on outside finance and achieve the scale required for making a meaningful difference in beneficiaries' well-being and economic development. Of course, monetary rewards for commercial performance are but one tool available to social entrepreneurs. Additional means of guiding employee effort include governance mechanisms (Ebrahim et al., 2014), hiring and socialization practices (Battilana and Dorado, 2010), and, where quantifiable, rewards for achieving social impact and operational targets (Wolfolds, 2018). Assessing the complementarity

of such practices presents an interesting avenue for future research, with potentially important implications for social enterprise performance.

Limitations As with any experiment, there are several limitations to our study. Because we are interested in how high-powered incentives affect effort allocation, we study a wide range of incentive strengths in social enterprises, from as weak monetary incentives as in non-profits, to as strong monetary incentives as in for profits. Conceptually, we thus cover the entire range of relevant incentive strengths. Nevertheless, it might be that our choice of experimental parameters still limits this range. In particular, the good cause payoff is always £1 and the monetary bonus is at most £1 per slider, as well. The relatively high returns to the social task ensures that this task is on a level playing field with the commercial task, but one may be legitimately worried that monetary incentives could be perceived as weak. A wider range of bonus levels, exceeding the good cause payoff, could potentially lead to different findings for strong incentives and explain why we do not find evidence for hypotheses 2 and 4. However, this would not affect our main conclusion that modest incentives help social enterprises rather than hurt them.

To causally isolate if and how monetary incentives affect effort allocation, we purposely simplify the nature of social enterprises at the expense of their hybrid complexity. First, our experiment has limited external validity, given the multitude of tasks social enterprise employees may perform, the number of dimensions on which employees may differ, as well as the nuanced presentation of different types of firms' objectives in real life. Nonetheless, we believe our experiment captures the fundamental tension between commercial and social tasks in social enterprises and employee effort allocation in a simple and transparent manner. While we attempted to further alleviate external validity concerns by recruiting participants from a representative pool of potential employees and giving them realistic choices of company types and missions, field replications represent a natural and necessary extension of our study. Second, social enterprise effectiveness often depends on coordinated action and team performance. If so, are team incentives better than individual

incentives? Does their effectiveness vary with employee heterogeneity? Third, the introduction of pay for performance is often accompanied by a justification, helping the practice gain legitimacy. Does the communication of incentives matter for their success? How does goal clarity improve that communication? How do social enterprises justify and implement an incentive change? Fourth, the presence of monetary incentives in social enterprises is likely to affect a broad range of institutional referents, beyond employees. How do investors and communities perceive social enterprises using monetary rewards: are they more professional or are they courting mission drift? Fourth, financial incentives may have other, potentially negative effects on aspects of hybrid organizations that our work has not considered. For instance, do incentives create internal conflicts between employees with different social preferences? Do monetary rewards generate long run distortions? What are joiners' commitment and retention rates? For social enterprises to achieve their promise, answers to these questions are crucial, in both stylized experimental settings and in the field.

Finally, the nature of the experiment constrains our ability to describe social enterprises in detail; nonetheless, even when exactly the same available actions and reward structure (for both own and good cause payoffs) exist in social enterprises as in for-profits or non-profits, behavior differs markedly across organizational forms. Despite our avoidance of the explicit 'social enterprise' label, subjects clearly distinguished this organizational form from the alternatives. This result prompts the importance of future research on the perception of social enterprises as a potentially distinct category, whose prescriptions for action employees and wider audiences may ultimately internalize, thereby providing additional legitimacy for this organizational form (Zuckerman, 1999; Negro et al., 2010; Pontikes, 2012).

Conclusion We have argued theoretically and provided experimental evidence that social enterprises can address revenue drift, an excessive focus on social impact at the expense of generating revenue, by deploying monetary rewards. Modest incentives redirect employee attention to commercial tasks and reinforce social enterprises' commitment to achieving their social mission via

market-based mechanisms, without attracting less socially employees. Our findings question a common view in social entrepreneurship that incentives are incongruent with social impact and/or attract the wrong kind of employee. This study contributes to a growing literature on hybrid organizations' challenges of managing competing logics, and opens up a whole vista of interesting questions regarding social enterprises' compensation practices and organizational design.

Appendix C

C.1 Instructions

Welcome Thank you for taking part in this study designed to learn about how people make decisions.[1] The choices we ask you to make are based on methods and techniques from economics, business administration, and psychology. The experiment is expected to take around 20 minutes. At the top of your screen, you will be able to see what percentage of the questionnaire you have already completed.

Personal data will be kept confidential. Your answers will only be used for this research. The published results will not refer to a person by name and will not describe individual choices. We will not disclose information to third parties. Aside from the participation fee, this experiment allows you to earn additional money. Out of all respondents who completed the questionnaire, we will randomly select 40 respondents for payment; as we expect around 800 participants in this experiment, the chance of being chosen for payment is about 1/20. Depending on the choices made, those chosen for payment can earn up to £80. Your cooperation is greatly appreciated.

The task This experiment will consist of four parts. In Parts 2 and 3, you will perform a slider task. This consists of a screen with 15 horizontal sliders, labeled from 0 to 100. As shown below, each slider is initially positioned at 50 and can be moved towards 0 or 100. These labels carry no inherent value, and only provide an axis for the slider. Each slider has a number to the right of it showing its current position. You can use your mouse in any way you like to move each slider. You can readjust the position of each slider as many times as you wish. You will have a chance to practice moving sliders shortly.

The task involves making a choice between placing the slider at 25 or 75, each corresponding to an action and a payoff that will be made clear at the beginning of each slider round. Placing the slider at 25 will always generate a payoff to you. Placing the slider at 75 will always generate a payoff to a good cause (which you will select below). Each slider will carry a payoff between £0 and £1, stated explicitly whenever you perform the task. Note that a slider will be taken into consideration for your payoff only if positioned at *exactly* 25 or 75, as accuracy is valued.

Figure C.1.1: Slider example.

Payment You will receive a fixed fee of £3 for completing this experiment. In addition, you can earn money in each of the rounds where you will perform the slider task, with the exception of the practice round. Finally, you can earn money in some of the questions asked towards the end of the experiment. It will always be clearly indicated if and how (much) money can be earned. When the experiment has concluded, a number of participants will be randomly selected as winners and will receive payment. We expect around 800 participants, and only 40 will be selected for payment: we therefore expect around 1 in 20 participants to be selected for payment. Throughout the experiment, you will also have the chance to earn money for both yourself and a good cause. You will have a chance to select a good cause below. If you are selected for payment, any payoff you have generated for the good cause will be transferred to that particular good cause.

[1] A preview can be accessed here. Each subject is randomly allocated a social enterprise incentive level (£0, £0.25, £0.50, or £1) as they enter the experiment, and is exposed to only one of these levels throughout. In addition, subjects never see the labels used below (FP, SE, NP), and only infer the type of company.

Part 1 – Good cause and practice

This part of the experiment allows you to choose your preferred good cause and become familiar with the task. Please proceed to the next screen.[2]

Please choose one of the three organizations below corresponding to your preferred good cause:

- **Workforce reintegration**: The Big Issue Foundation seeks to promote the social and financial inclusion of its vendors by identifying and motivating individuals to engage with the services that will help them move forward and deal with their homelessness and health issues and achieve their own goals.
- **Fair and equitable trade**: Fairtrade Foundation seeks to connect disadvantaged producers and consumers, promote fairer trading conditions through standardization and certification and empower producers to combat poverty, strengthen their position, and take more control over their lives.
- **Water quality and environment**: Water Aid seeks to deliver clear water, improved sanitation, and proper hygiene to developing countries through a combination of technical solutions and hygiene education. They aim to ensure the effectiveness of their projects by using carbon-neutral, sustainable methods that preserve the environment.

You now have a chance to practice moving sliders. Please remember that a slider is considered correctly placed only if placed at exactly 25 or exactly 75. The numbers only represent positions that correspond to actions providing an own payoff and a payoff to the previously selected good cause. These 2 sliders are given for you to become familiar with the task. You will not be paid for this practice round. Please keep in mind that in the actual task you will position sets of 15 sliders. When you are sufficiently familiar with this task, please proceed to the next screen.

Comprehension check Before proceeding to the actual task, please answer the following questions.

If each slider placed at 25 produces an own payoff of 5 points, and each slider placed at 75 produces a good cause payoff of 20 points, what is the good cause payoff when 5 sliders are placed at 75?

- 100
- 25
- 40

If each slider placed at 25 produces an own payoff of 10 points, and each slider placed at 75 produces a good cause payoff of 20 points, what is the good cause payoff when 2 sliders are placed at 75?

- 100
- 25
- 40

If each slider placed at 25 produces an own payoff of 5 points, and each slider placed at 75 produces a good cause payoff of 20 points, what is your payoff when 5 sliders are placed at 25?

- 100
- 25
- 40

This is the end of Part 1. Please proceed to the next screen.

[2] Randomization is employed with regards to the order of: i) good causes, ii) attention check questions (and options), iii) contracts in Part 2, and iv) contracts in the choice question in Part 3.

Part 2 – All contracts

You will now be performing the slider task under a set of different contracts. Throughout the experiment you will behave as an employee of a set of companies. These companies provide similar services, but have different objectives, as explained at the beginning of each scenario. **Please read the company descriptions carefully**. Moving the slider will allow you to make choices as an employee of those companies. The actions described within each contract will correspond to potential actions of employees of such companies, and generate either a payoff to you or the good cause you selected earlier. The text will explain clearly how the payoffs are generated. Please proceed to the next screen.

Workforce reintegration (if selected)

For profit Imagine you are working for a company providing garbage collection services on the market. **The company only cares about generating revenue through the sale of services.**

- By placing the slider exactly at position 25 you can generate revenue for the company through the sale of goods and services; each slider you position at 25 will give you a payoff of £1.
- By placing the slider exactly at position 75 you can contribute to the professional development of its employees; each slider you position at 75 will give a payoff of £1 to the good cause.[3]

Please place the 15 sliders below as you see fit.

Non profit Imagine you are working for a company aimed at reintegrating long term unemployed people into the workforce by hiring them to provide garbage collection services. **The company only cares about workforce reintegration through the professional development of the long term unemployed.**

- By placing the slider exactly at position 25 you can generate revenue for the company through the sale of services; each slider you position at 25 will give you a payoff of £0.
- By placing the slider exactly at position 75 you can contribute to the professional development of its employees; each slider you position at 75 will give a payoff of £1 to the good cause.

Please place the 15 sliders below as you see fit.

Social enterprise Imagine you are working for a company aimed at reintegrating long term unemployed people into the workforce by hiring them to provide garbage collection services that are then sold on the market. **It is in the best interest of the organization that both ensuring the professional development of the long term unemployed and generating revenue through the sale of services receive attention from employees.**

- By placing the slider exactly at position 25 you can generate revenue for the company through the sale of services; each slider you position at 25 will give you a payoff of £0/£0.25/£0.50/£1.
- By placing the slider exactly at position 75 you can contribute to the professional development of its employees; each slider you position at 75 will give a payoff of £1 to the good cause.

Please place the 15 sliders below as you see fit.

[3] The availability of the social task in FP contracts approximates the possibility of CSR activities.

Fair and equitable trade (if selected)

For profit Imagine you are working for a company investing in new businesses. **The company only cares about generating returns for its investors by selecting the most promising ventures.**

- By placing the slider exactly at position 25 you can generate returns for the company by investing in the most promising ventures; each slider you position at 25 will give you a payoff of £1.
- By placing the slider exactly at position 75 you can make finance accessible to fair trade businesses in developing countries; each slider you position at 75 will give a payoff of £1 to the good cause.

Please place the 15 sliders below as you see fit.

Non profit Imagine you are working for a company dedicated to investing in new businesses. **The company only cares about alleviating poverty by supporting fair trade businesses in developing countries.**

- By placing the slider exactly at position 25 you can generate returns for the company by investing in the most promising ventures; each slider you position at 25 will give you a payoff of £0.
- By placing the slider exactly at position 75 you can make finance accessible to fair trade businesses in developing countries; each slider you position at 75 will give a payoff of £1 to the good cause.

Please place the 15 sliders below as you see fit.

Social enterprise Imagine you are working for a company investing in new businesses. **It is in the best interests of the organization that both offering fair trade businesses in developing countries access to loans and credit facilities and ensuring a positive rate of return on investments receive attention.**

- By placing the slider exactly at position 25 you can generate returns for the company by investing in the most promising ventures; each slider you position at 25 will give you a payoff of £0/£0.25/£0.50/£1.
- By placing the slider exactly at position 75 you can make finance accessible to fair trade businesses; each slider you position at 75 will give a payoff of £1 to the good cause.

Please place the 15 sliders below as you see fit.

Water quality and environment (if selected)

For profit Imagine you are working for a company providing water services to a variety of other organizations on the market. **The company only cares about generating revenue by expanding market access.**

- By placing the slider exactly at position 25 you can generate revenue for the company by expanding market access; each slider you position at 25 will give you a payoff of £1.
- By placing the slider exactly at position 75 you can reduce the carbon emissions resulting from product packaging and delivery; each slider you position at 75 will give a payoff of £1 to the good cause.

Please place the 15 sliders below as you see fit.

Non profit Imagine you are working for a company providing environmentally sustainable water services to a variety of other organizations. **The company only cares about having an environmentally friendly product, with minimal carbon emissions and fully recyclable packaging.**

- By placing the slider exactly at position 25 you can generate revenue for the company by expanding market access; each slider you position at 25 will give you a payoff of £0.
- By placing the slider exactly at position 75 you can reduce the carbon emissions resulting from product packaging and delivery; each slider you position at 75 will give a payoff of £1 to the good cause.

Please place the 15 sliders below as you see fit.

Social enterprise Imagine you are working for a company providing environmentally sustainable water services to a variety of other organizations on the market. **It is in the best interests of the organization that both ensuring that production and delivery are done with minimal environmental impact and increasing revenues by expanding market access receive attention.**

- By placing the slider exactly at position 25 you can generate revenue for the company by expanding market access; each slider you position at 25 will give you a payoff of £0/£0.25/ £0.50/£1.
- By placing the slider exactly at position 75 you can reduce the carbon emissions resulting from product packaging and delivery; each slider you position at 75 will give a payoff of £1 to the good cause.

Please place the 15 sliders below as you see fit.

This is the end of Part 2. Please proceed to the next screen.

Part 3 – Preferred contract

You will now perform the slider task once more. However, this time you can choose your preferred contract from the ones in Part 2. Please proceed to the next screen.

Workforce reintegration (if selected)

Which contract would you like to perform the slider task in?

- The company provides garbage collection services on the market and cares only about generating revenue. Placing the slider at exactly 25 produces an own payoff of £1. Placing the slider at exactly 75 produces a payoff of £1 for the good cause.
- The company reintegrates the unemployed into the workforce by hiring them to provide garbage collection services and cares only about the professional development of its employees. Placing the slider at exactly 25 produces an own payoff of £0. Placing the slider at exactly 75 produces a payoff of £1 for the good cause.
- The company reintegrates unemployed people into the workforce by hiring them to provide garbage collection services that are sold on the market. It is in the best interests of the organization that both generating revenue and aiding the professional development of its employees receive attention. Placing the slider at exactly 25 produces an own payoff of £0/£0.25/£0.50/£1. Placing the slider at exactly 75 produces a payoff of £1 for the good cause.

Fair and equitable trade (if selected)

Which contract would you like to perform the slider task in?

- The company invests in the most promising new businesses and cares only about generating returns for investors. Placing the slider at exactly 25 produces an own payoff of £1. Placing the slider at exactly 75 produces a payoff of £1 for the good cause.
- The company provides access to finance for fair trade businesses in developing countries and cares only about poverty alleviation. Placing the slider at exactly 25 produces an own payoff of £0. Placing the slider at exactly 75 produces a payoff of £1 for the good cause.
- The company provides access to finance for fair trade businesses in developing countries with a positive rate of return on investments. It is in the best interests of the company that both generating positive returns and poverty alleviation receive attention. Placing the slider at exactly 25 produces an own payoff of £0/£0.25/£0.50/£1. Placing the slider at exactly 75 produces a payoff of £1 for the good cause.

Water quality and environment (if selected)

Which contract would you like to perform the slider task in?

- The company provides water services to other organizations on the market and cares only about generating revenue. Placing the slider at exactly 25 produces an own payoff of £1. Placing the slider at exactly 75 produces a payoff of £1 for the good cause.
- The company provides water services to other organizations and cares only about environmental sustainability. Placing the slider at exactly 25 produces an own payoff of £0. Placing the slider at exactly 75 produces a payoff of £1 for the good cause.
- The company provides water services to other organizations on the market. It is in the best interest of the organization that both generating revenue and environmental sustainability receive attention. Placing the slider at exactly 25 produces an own payoff of £0/£0.25/£0.50/£1. Placing the slider at exactly 75 produces a payoff of £1 for the good cause.

[Subjects then perform slider task in chosen contract.]
This is the end of Part 3. Please proceed to the next screen.

Part 4 – Questionnaire

Before you complete the experiment, please answer the following questions.

What is your gender?

- Male
- Female

What is your age?

- Under 25
- 25-34
- 35-44
- 45-54
- 55 or older

What is the highest level of schooling you have completed?

- High school diploma

- Bachelor degree
- Master degree
- Other

Are you currently as student?

- Yes
- No

What is your educational background?

- Economics and business
- Arts, architecture, and design
- Science, technology, engineering, and mathematics
- Law, social sciences, and humanities
- Medicine, health, and care
- Other

What was your gross income (across all sources of income) in 2018? *We understand this information is sensitive. Therefore, if you want, you can keep it private. However, this information may help us understand differences in economic decision-making.*

- Less than £10,000
- Between £10,000 and £25,000
- Between £25,000 and £50,000
- Between £50,000 and £75,000
- More than £75,000
- I prefer not to answer this question

For this question, you will be randomly and anonymously paired with another participant. **This question allows you to earn some money**. You are endowed with £10 and have to decide how much of the £10 you would like to share with the other participant. Please enter the amount you would like to give to the other participant below (you will keep the remainder of the £10 for yourself). *Values between 0 and 10 are allowed, up to two decimals (e.g. 9.99 or 0.01).* **If selected for payment, you will receive the amount you chose to keep and the randomly selected participant will receive the amount you chose to give. At the same time, you will also be randomly paired with another participant deciding how to share £10 with you. If selected for payment, you will also receive this amount. Note that the person you give to and the person that gives to you will not be the same person.** [*Altruism*]

- ...

Imagine a similar situation to the one just described (i.e. sharing £10), with three differences. First, in this case, you are the recipient. Second, you can choose to refuse the amount received if you consider it inappropriate. However, if you refuse the amount, neither you or the other person would receive any payoff. **Note that this question will not earn you money**. What would be the minimum amount offered that you would accept? *Values between 0 and 10 are allowed, up to two decimals (e.g. 9.99 or 0.01).* [*Inequality aversion*]

- ...

For each of the statements below, please select the option that best describes you. *The options are: strongly disagree, somewhat disagree, neither agree nor disagree, somewhat agree, and strongly agree.* [*Compassion*]

- I am rarely moved by the plight of the underprivileged. (reverse coded)
- Most social programs are too vital to do without.
- It is difficult for me to contain my feelings when I see people in distress.
- To me, patriotism includes seeing to the welfare of others.
- I seldom think about the welfare of people whom I don't know personally. (reverse coded)
- I am often reminded by daily events about how dependent we are on one another.
- There are few public programs that I wholeheartedly support. (reverse coded)
- I have little compassion for people in need who are unwilling to take the first step to help themselves. (reverse coded)
- Please click on 'Somewhat disagree' [attention check]

Imagine you won £1,000 in a lottery. Considering your current situation, how much would you donate to a good cause? *Values between 0 and 1,000 are allowed, up to two decimals (e.g. 999.99 or 0.01).* [*Hypothetical altruism*]

- ...

How do you assess your willingness to share with others without expecting anything in return when it comes to a good cause? *Please use a scale from 0 to 10, where 0 means you are "completely unwilling to share" and 10 means you are "very willing to share". You can use values in between to indicate where you fall on the scale.* [*Willingness to share*]

- ...

Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks? *Please use a scale from 0 to 10, where 0 means you are "completely unwilling to take risks" and 10 means you are "fully prepared to take risks". You can use values in between to indicate where you fall on the scale.* [*Risk preferences*]

- ...

How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future? *Please use a scale from 0 to 10, where 0 means you are "completely unwilling to give up something today" and 10 means you are "fully prepared to give up something today". You can use values in between to indicate where you fall on the scale.*[*Time preferences*]

- ...

How would you label an organization with both economic and social value creation goals?

- ...

How would you perceive a social enterprise that introduces performance bonuses?

- ...

What was the own payoff per slider offered by the social enterprise contract?

- £0/£0.25/£0.50/£1 [manipulation check]

In the past, have you:

- Been employed by a non-profit? Yes/no
- Been employed by a social enterprise? Yes/no

How often do you:

- Volunteer? Rarely/often
- Donate to social organizations? Rarely/often
- Work professionally with social organizations? Rarely/often [*Prosocial behavior*]

C.2 Experimental Checks

Sample and summary statistics This appendix presents a set of basic checks on the data generated through our experiment, whose design we summarize in Table 4.1. We impose several restrictions on our main sample in order to ensure the highest quality of data. First, we consider sliders placed at 23-27 and 73-77 as correct, indicating a clear intention to exert commercial or social effort; we then require that subjects have placed at most 10 of the 60 sliders they perform overall outside of these ranges. Second, we require subjects to pass at least one of the attention and manipulation checks. Third, we require our gender variables and the one offered by Prolific to be in agreement. Fourth, we require subjects not to complete the experiment in less than 10 minutes or more than 40 minutes (potential signals of lack of attention). These criteria leave us with a sample of 708 subjects, although Appendix Table C.4.1 shows our findings are robust to tightening or relaxing these restrictions. Table C.2.1 provides summary statistics for our sample. Half of the participants are women, 16.5% are students, 40% have a bachelor degree, and 13% have a master degree. Respondents span the income and age ranges, although a large share are below age 44 and below £50,000 annual income. Average *Compassion* is 29 out of 40 and subjects share on average £4.2 in the dictator game (with a large fraction sharing exactly £5). Subjects report being willing to share without expecting anything in return (mean 6.4 on a 0-10 scale), and 24.4% and 10.5% report previous non-profit or social enterprise employment. Table C.2.2 also shows that most social preference measures are positively correlated. Subjects are moderately willing to take risks (mean 5.1 on a 0-10 scale), but are willing to give something up today in order to benefit in the future (mean 6.5 on a 0-10 scale). Finally, subjects completed the experiment in around 18.5 minutes on average, although substantial variation exists.

Good cause (mission) choice To ensure the saliency of the social task, we allowed subjects to choose their preferred good cause from the options: workforce reintegration, fair and equitable trade, and water quality and environment. This choice of good cause informs the company descriptions (mission) that subjects face. Subjects' choices are shown in Table C.2.3, by treatment. Table C.2.4 performs a multinomial logit estimation of the choice of mission on demographics, social preferences, and dummies for the treatment subjects were allocated to (i.e., £0.25, £0.50, and £1 incentive levels, against a £0 baseline). The results suggest that social preferences and demographics are largely uncorrelated with the choice of good cause. Subjects in the £0.25 and £0.50 treatments were less likely to select a workforce reintegration or fair trade mission, preferring an environmental mission instead, and the treatment dummies are jointly significant ($p = 0.023$). However, a χ^2 test cannot reject the independence of mission and treatment ($p = 0.111$). The latter is consistent with the structure of the experiment, as subjects were not aware of the treatment they were randomly allocated to (i.e., the SE bonus) when they chose the good cause. To alleviate any concerns regarding the endogenous nature of the mission choice, our robustness checks using regression analyses include mission choice dummies, essentially comparing within groups of individuals choosing the same good cause. Furthermore, our results are similar across these three mission choices, see Appendix C.7.

Randomization check We assess whether our randomization procedure has been successful by estimating a set of regressions of various demographics and social preferences on treatment dummies in Table C.2.5. The £0.25 treatment has a slightly larger share of individuals with income between £25,000 and £50,000, and subjects took longer to practice the slider task in the £0.50 and £1 treatments. These significant coefficients are within the bounds of the number of significant effects appearing by chance, and become insignificant with multiple hypothesis testing adjustments (Romano and Wolf, 2005; List et al., 2018). For other demographics and social preferences the dummies are jointly insignificant (all $p > 0.25$), and produce a poor fit of the data

(all $R^2 < 0.01$). This is true not only for the main analysis sample ($N = 708$), but also for all available observations ($N = 796$). Overall, our randomization has been successful.

Table C.2.1: **Descriptive Statistics**

		Mean	Std. dev.	N	Min	Max
A. Demographics						
Female		0.499	(0.500)	708	0	1
Student		0.165	(0.372)	708	0	1
Education:	High school	0.359				
	Bachelor degree	0.398				
	Master degree	0.127				
Income:	< £10,000	0.215				
	£10,000 – £25,000	0.329				
	£25,000 – £50,000	0.307				
	£50,000 – £75,000	0.077				
	> £75,000	0.025				
Age:	18 – 24	0.216				
	25 – 34	0.356				
	35 – 44	0.226				
	45 – 54	0.140				
	55 – 64	0.062				
B. Social preferences						
Compassion		29.195	(4.969)	708	12	40
Altruism		4.207	(2.181)	708	0	10
Inequality aversion		2.698	(2.275)	708	0	10
Hypothetical altruism		134.859	(159.739)	708	0	1,000
Willingness to share		6.404	(2.354)	708	0	10
Non-profit employment		0.244	(0.430)	708	0	1
Social enterprise employment		0.105	(0.306)	708	0	1
Volunteer		0.226	(0.419)	708	0	1
Donate		0.520	(0.500)	708	0	1
Work with social organization		0.160	(0.366)	708	0	1
Prosocial behavior		1.254	(1.159)	708	0	5
Social motivation (factor)		0.000	(0.757)	708	-2.450	2.307
Compassion (standardized)		0.000	(1.000)	708	-3.460	2.174
Risk taking		5.130	(2.474)	708	0	10
Time discounting		6.532	(2.193)	708	0	10
C. Experimental parameters						
Practice time		40.049	(31.011)	708	0.000	608.147
Comprehension check time		67.419	(37.004)	708	18.617	300.324
Questions time		28.359	(23.547)	708	8.341	280.372
Experiment time		1,111	(351.909)	708	600	2,399
Own payoff (£)		28.894	(12.342)	40	3	60
Good cause payoff (£)		33.025	(12.305)	40	6	60

Times given in seconds. Education and income coded as 'other' for 11.58% and 4.66% of subjects.

Table C.2.2: **Correlation Table: Social Preferences**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Compassion	1.0000							
(2) Altruism	0.1362	1.0000						
(3) Inequality aversion	-0.1353	0.0746	1.0000					
(4) Hypothetical altruism	0.2020	0.2650	0.0312	1.0000				
(5) Willingness to share	0.3994	0.1729	0.0050	0.3415	1.0000			
(6) Prosocial behavior	0.2337	0.1413	-0.0021	0.2570	0.3049	1.0000		
(7) Risk taking	0.0215	0.0515	0.0328	0.0880	0.1484	0.1108	1.0000	
(8) Time preferences	0.1190	0.0519	-0.0867	0.1530	0.3215	0.1403	0.1833	1.0000

Pairwise correlations between social preference measures. Compassion is measured through the compassion subscale of the public service motivation scale, altruism is measured through a dictator game, inequality aversion is measured through an ultimatum game, hypothetical altruism is measured as the donation in a hypothetical lottery, willingness to share is measured as subjects' willingness to share with other without expecting anything in return, and prosocial behavior is measured by combining questions on i) past non-profit employment, ii) past social enterprise employment, iii) volunteering, iv) donations, or v) professional relations with social organizations. Risk taking and time preferences are assessed with self-reflection questions on willingness to take risks and willingness to give something up today for a reward tomorrow. All correlations larger than 0.07 (in absolute terms) are significant at 5%.

Table C.2.3: **Choice of Good Cause**

Treatment	Workforce reintegration	Fair and equitable trade	Water quality and environment	N
£0	50	36	84	170
£0.25	41	24	113	178
£0.50	39	36	110	185
£1	47	36	92	175
Total	177	132	399	708

Number of participants in each treatment that selected the given good cause.

Table C.2.4: **Choice of Good Cause: Multinomial Logit**

	Workforce reintegration		Fair and equitable trade	
	Coefficient	(s.e.)	Coefficient	(s.e.)
Compassion	0.053*	(0.023)	0.040	(0.024)
Altruism	-0.037	(0.047)	-0.046	(0.050)
Inequality aversion	-0.010	(0.044)	0.102**	(0.046)
Hypothetical altruism	0.000	(0.001)	0.000	(0.001)
Willingness to share	-0.056	(0.053)	-0.036	(0.054)
Prosocial behavior	0.071	(0.089)	0.054	(0.099)
Risk taking	0.029	(0.040)	0.034	(0.044)
Time preferences	0.027	(0.047)	0.073	(0.052)
Treatment = £0.25	-0.628**	(0.269)	-0.830***	(0.315)
Treatment = £0.50	-0.627**	(0.274)	-0.391	(0.292)
Treatment = £1	-0.180	(0.268)	-0.110	(0.295)
Female	0.311	(0.206)	-0.031	(0.227)
Student	-0.843**	(0.363)	-0.220	(0.340)
High school diploma	0.249	(0.322)	-0.004	(0.348)
Bachelor degree	-0.494	(0.329)	-0.426	(0.356)
Master degree	-0.243	(0.396)	-0.238	(0.439)
< £10,000	1.108*	(0.566)	0.847	(0.566)
£10,000 – £25,000	0.956	(0.543)	0.725	(0.546)
£25,000 – £50,000	0.930	(0.551)	0.579	(0.560)
£50,000 – £75,000	0.907	(0.617)	-0.043	(0.695)
> £75,000	0.740	(0.773)	-0.226	(0.947)
Age 25-34	-0.302	(0.323)	-0.771**	(0.340)
Age 35-44	-0.114	(0.350)	-0.129	(0.359)
Age 45-54	0.413	(0.376)	0.067	(0.404)
Age > 55	-0.094	(0.465)	-1.015*	(0.595)
Constant	-2.523***	(1.035)	-2.599***	(1.003)
N	708			
LR χ^2 (p-value)	85.25 (0.018)			
Pseudo-R^2	0.061			
Social preferences: χ^2 (p-value)	17.18 (0.374)			
Treatment levels: χ^2 (p-value)	11.34 (0.023)			

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses. The baseline mission is the water quality and environment mission. For categorical variables the baselines are high school education, income < £10,000, age 18-25, and the £0 treatment. We do not report dummies for field of education for brevity, though none are significant. Although the mission was chosen in advance of subjects being aware of the bonus offered by the social enterprise contract (the treatment), a simple χ^2 test rejects the independence of mission and treatment with $p = 0.111$.

Table C.2.5: **Randomization Check**

	Treatment					
Characteristic	(1) £0.25	(2) £0.50	(3) £1	(4) p-value	(5) N	(6) R^2
Compassion	-0.309	0.061	-0.508	0.663	708	0.002
	(0.524)	(0.538)	(0.506)			
Altruism	0.027	-0.102	-0.007	0.946	708	0.001
	(0.248)	(0.241)	(0.237)			
Inequality aversion	0.007	0.000	-0.155	0.861	708	0.001
	(0.251)	(0.254)	(0.236)			
Hypothetical altruism	18.673	24.416	17.190	0.496	708	0.003
	(17.460)	(16.812)	(15.998)			
Willingness to share	-0.243	0.033	-0.272	0.476	708	0.003
	(0.262)	(0.257)	(0.247)			
Prosocial behavior	-0.155	0.033	-0.066	0.587	708	0.002
	(0.124)	(0.136)	(0.126)			
Social motivation	-0.055	0.029	-0.055	0.658	708	0.002
(factor)	(0.082)	(0.084)	(0.077)			
Risk taking	0.158	-0.009	-0.077	0.815	708	0.001
	(0.265)	(0.275)	(0.267)			
Time preferences	0.378	0.226	0.127	0.427	708	0.004
	(0.234)	(0.233)	(0.225)			
Age	0.073	-0.008	-0.163	0.269	708	0.005
	(0.124)	(0.123)	(0.124)			
Female	-0.011	-0.008	0.014	0.965	708	0.000
	(0.054)	(0.053)	(0.054)			
Student	-0.007	0.003	0.030	0.818	708	0.001
	(0.039)	(0.039)	(0.041)			
Bachelor degree	-0.052	-0.001	-0.023	0.717	708	0.002
	(0.052)	(0.053)	(0.053)			
Master degree	0.028	0.007	0.002	0.862	708	0.001
	(0.036)	(0.035)	(0.035)			
Low income	-0.048	-0.030	-0.028	0.843	708	0.001
	(0.053)	(0.053)	(0.054)			
Medium income	0.084*	0.077	0.050	0.291	708	0.005
	(0.049)	(0.048)	(0.048)			
High income	0.030	0.020	0.032	0.726	708	0.002
	(0.032)	(0.031)	(0.032)			
Practice time	2.677	5.863***	6.629*	0.028	708	0.007
	(2.408)	(2.097)	(3.881)			
Comprehension time	-4.894	-1.919	-5.440	0.499	708	0.004
	(4.136)	(4.293)	(4.110)			
Questions time	2.530	-0.294	1.246	0.675	708	0.002
	(2.767)	(2.041)	(2.532)			

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Robust standard errors in parentheses. Each row presents results from a separate model, regressing the given trait on treatment dummies, with joint significance test p-values. The omitted education categories are high school and 'other'; the omitted income category comprises those who prefer not to answer. Estimating a series of seemingly unrelated regressions produces similar results. Multiple hypothesis testing adjustments eliminate the significant coefficients.

C.3 Power Calculations

To ensure we are able to detect meaningful changes in social task effort, we performed a series of *ex ante* power calculations. In other words, given the sample sizes we target, the expected means and standard deviations, and the required significance level, what is the statistical power to detect a given effect? Conversely, what is the smallest effect we can detect while still maintaining statistical power above the conventional 80%? The purpose of this section, therefore, is to ensure that our experiment is able to maximize power and minimize the effect sizes it can detect.

Following List et al. (2011), for independent groups with means μ_a and μ_b, standard deviations $\sigma_a = \sigma_b = \sigma$, sample sizes N_a and N_b, significance level α, and detectable effect size δ, statistical power $1 - \beta$ for a two-sided test is calculated to satisfy:

$$\delta = (t_{\alpha/2} + t_\beta)\sqrt{\frac{\sigma_a^2}{N_a} + \frac{\sigma_b^2}{N_b}} \tag{C.3.1}$$

Equation C.3.1 shows that the effect size δ we can detect increases with the required significance level (i.e. we can detect larger effects at 5% than at 1% significance) and the standard deviations of the outcomes (i.e. the less noisy our estimates, the smaller the effect we can detect), but decreases with sample size (i.e. the more observations, the smaller the effect we can detect). The formula also shows that δ and t_β (and, as a result, $1-\beta$) are positively correlated, which implies that small effect sizes can only be detected when there is more statistical power. While statistical power rises with sample size, budget constraints limit this avenue, highlighting the trade-off between power and effect size. The results below provide a set of assumptions regarding sample sizes, means, standard deviations, and significance, in order to assess the relationship between effect size and power in our experiment.

In Panel A of Table C.3.1, we consider comparisons of SE social effort across treatments, with sample size 200 per group, fixing one sample mean to 7.5 as our expectation of a fully balanced effort allocation, and varying the other to achieve various δ levels and standardized effect sizes $0.2 < \delta/\sigma < 1$.[4] Based on a pilot experiment, we set $\sigma \in \{1, 1.5, 2, 2.5\}$. Results in column (8) suggest that we have substantial power (below the conventional 80% only in the most conservative settings) to detect small effect sizes in two-sided tests. For similar comparisons across treatments when sorting is permitted, we expect social enterprise sample sizes around 120-150, such that power is close to that in Panel A.

In Panels B and C, we consider comparisons between the largest group (SE) and smallest group (FP/NP) within the £0.50 and £1 treatments, as suggested by the pilot experiment.[5] The larger sample size we expect for the social enterprise group is in line with the higher expected variance in this group (List et al., 2011), although the ratio of variances is perhaps smaller. Small variations in sample size or variance (between groups) do not affect the main conclusions, namely that unless standard deviations are very large ($\sigma > 1.5$), our tests significantly detect a 1-unit change in effort allocation with power $1 - \beta > 80\%$.

For completeness, in column (10) we show the minimum effect size δ_{min} for a given standard deviation in two-sided tests with 80% power. In these two-sided tests, we are virtually always able to detect changes of $\delta/\sigma \geq 0.6$. Note that so far we have used two-sided tests in our power calculations in order to be conservative. As our hypotheses are mostly one-sided, we calculate power for such tests in column (9): as expected, these tests are even more powerful.

[4] There is no *ex ante* reason to expect different variance in social enterprise social effort across treatments when sorting is not allowed; therefore, we opted for equal samples across treatments (List et al., 2011). *F*-tests of equal variance based on the means and standard deviations of social effort reported in Table 4.2 show that, *ex post*, the assumption of equal variance is valid ($p > 0.1$ in all comparisons).

[5] The FP and NP groups are smaller, but their mean difference is expected to be large, so power is retained.

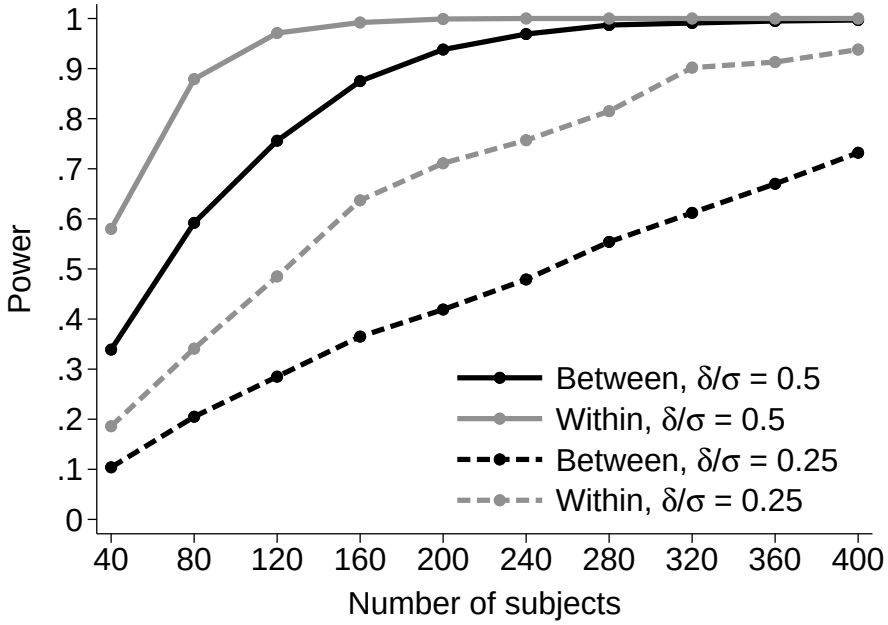

Figure C.3.1: Power levels for two-sided mean comparisons between- and within-individuals ($\alpha = 0.05, \sigma_a = \sigma_b = \sigma = 2, \delta = 1$ or $0.5, \delta/\sigma = 0.5$ or $0.25, H_0$: $\mu_a = \mu_b, H_a$: $\mu_a \neq \mu_b$).

Some of the comparisons we perform (for example, between SE and NP without sorting) represent dependent samples; in the case of such within-subject comparisons, power is expected to be at least as high (List et al., 2011). We confirm this result in Figure C.3.1, where we calculate the power achieved for between- and within- comparisons for $\sigma = 2$ and $\delta = 1$ (i.e, $\delta/\sigma = 0.5$) and, even more conservatively, $\delta = 0.5$ (i.e, $\delta/\sigma = 0.25$), following the simulation-based approach proposed by Bellemare et al. (2016). While we fail to achieve enough power to detect $\delta/\sigma = 0.25$, we obtain $1 - \beta > 80\%$ for $\delta/\sigma = 0.5$ whenever our groups have at least 80 subjects each.

Figure C.3.2 confirms the power calculations in Table C.3.1, showing the required sample size for detecting a given effect size δ with 80% power, when $\sigma \in \{1, 1.5, 2, 2.5\}$. Only in the most conservative settings (with high variance and small effect sizes) do we require samples larger than the ones we obtain; we are almost always able to detect 1-unit changes in effort.

Finally, since we are interested in testing a number of hypotheses, we must adjust ex ante for multiple hypothesis testing (List et al., 2018). As a conservative approach, we use a Bonferroni correction, requiring $\alpha = 0.05/k$, where k is the number of hypotheses. For instance, assuming $k = 5$, then the necessary significance level becomes $\alpha = 0.01$. In this case, the minimum effect sizes relative to the standard deviation, δ_{min}/σ, we are able to detect with 80% power are 0.342, 0.639, and 0.751 in panels A, B, and, respectively, C. Figure C.3.3 shows the required sample sizes for 80% power two-sided tests with significance $\alpha = 0.01$: our sample sizes are once again able to detect 1-unit changes in effort under all but the most conservative settings.

The conclusion of this section is that across a range of assumptions regarding sample sizes, means, standard deviation, and significance, our experiment is able to detect small changes in social effort – i.e. of at least half a standard deviations – even under the most conservative specifications. *Ex post*, it is important to note that while the standard errors resulting from our experiment were higher than the ones we used for power calculations, the materialized differences were also larger, such that power was maintained throughout.[6]

[6] List et al. (2011) warn against performing power calculations with the actual data from the experiment, which is why we emphasize the *ex ante* calculations.

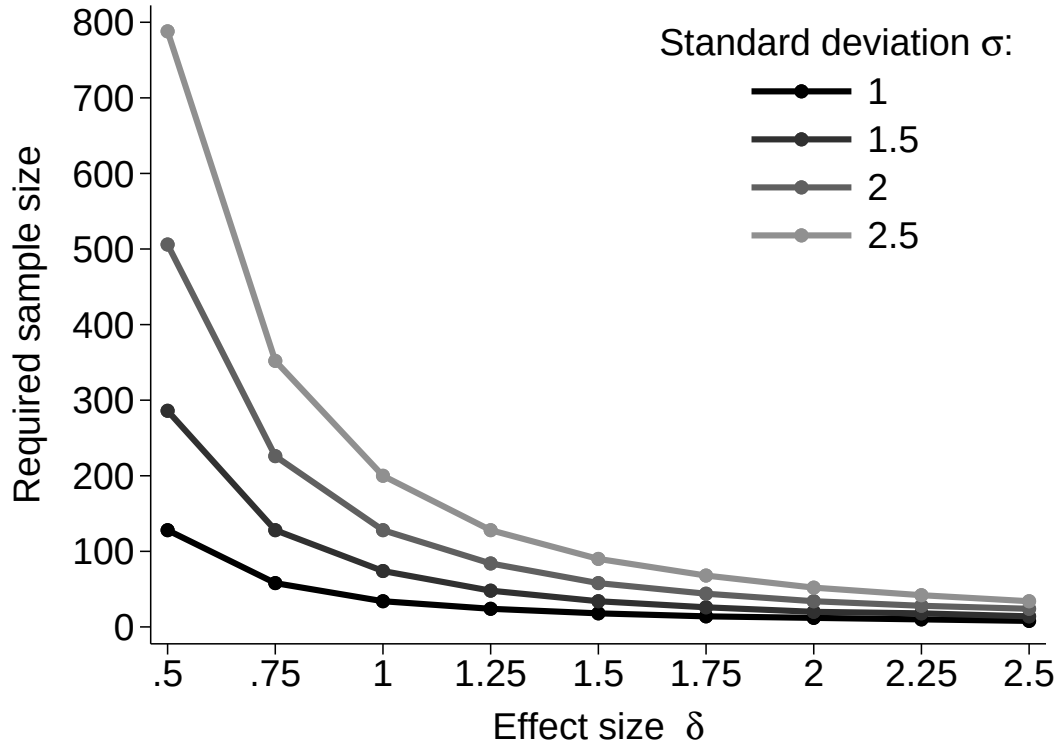

Figure C.3.2: Required sample size for detecting effect size δ with 80% power in two-sided mean comparison $(1-\beta=0.8, \alpha=0.05, \sigma_a=\sigma_b=\sigma, H_0\text{: } \mu_a=\mu_b, H_a\text{: } \mu_a\neq\mu_b)$.

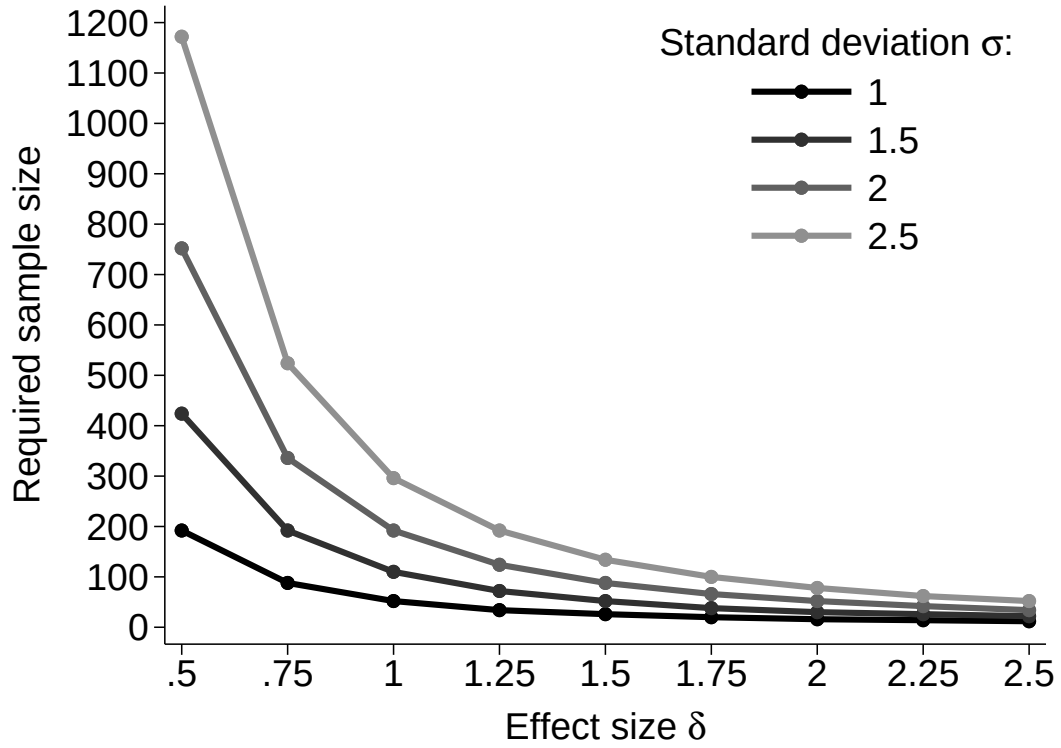

Figure C.3.3: Required sample size for detecting effect size δ with 80% power in two-sided mean comparison $(1-\beta=0.8, \alpha=0.01, \sigma_a=\sigma_b=\sigma, H_0\text{: } \mu_a=\mu_b, H_a\text{: } \mu_a\neq\mu_b)$.

Table C.3.1: **Power Calculations**

	N_a (1)	N_b (2)	μ_a (3)	μ_b (4)	σ (5)	δ/σ (6)	α (7)	2-sided $1-\beta$ (8)	1-sided $1-\beta$ (9)	$\delta^{80\%}_{min}$ (10)
A.	200	200	7.5	8	2.5	0.20	0.05	0.514	0.638	0.700
Without	200	200	7.5	8.5	2.5	0.40	0.05	0.979	0.991	
sorting	200	200	7.5	9	2.5	0.60	0.05	1.000	1.000	
	200	200	7.5	8	2	0.25	0.05	0.703	0.803	0.560
	200	200	7.5	8.5	2	0.50	0.05	0.999	1.000	
	200	200	7.5	9	2	0.75	0.05	1.000	1.000	
	200	200	7.5	8	1.5	0.33	0.05	0.914	0.954	0.420
	200	200	7.5	8.5	1.5	0.66	0.05	1.000	1.000	
	200	200	7.5	9	1.5	1.00	0.05	1.000	1.000	
	200	200	7.5	7.75	1	0.25	0.05	0.703	0.803	0.280
	200	200	7.5	8	1	0.50	0.05	0.999	1.000	
	200	200	7.5	8.5	1	1.00	0.05	1.000	1.000	
B.	100	40	7.5	8	2.5	0.20	0.05	0.186	0.281	1.310
With	100	40	7.5	8.5	2.5	0.40	0.05	0.565	0.685	
sorting	100	40	7.5	9	2.5	0.60	0.05	0.890	0.939	
(£0.50)	100	40	7.5	8	2	0.25	0.05	0.264	0.376	1.048
	100	40	7.5	8.5	2	0.50	0.05	0.756	0.845	
	100	40	7.5	9	2	0.75	0.05	0.978	0.991	
	100	40	7.5	8	1.5	0.33	0.05	0.425	0.551	0.786
	100	40	7.5	8.5	1.5	0.66	0.05	0.943	0.971	
	100	40	7.5	9	1.5	1.00	0.05	0.999	1.000	
	100	40	7.5	7.75	1	0.25	0.05	0.264	0.376	0.524
	100	40	7.5	8	1	0.50	0.05	0.756	0.845	
	100	40	7.5	8.5	1	1.00	0.05	0.999	1.000	
C.	120	25	7.5	8	2.5	0.20	0.05	0.148	0.230	1.540
With	120	25	7.5	8.5	2.5	0.40	0.05	0.439	0.566	
sorting	120	25	7.5	9	2.5	0.60	0.05	0.774	0.858	
(£1)	120	25	7.5	8	2	0.25	0.05	0.204	0.304	1.232
	120	25	7.5	8.5	2	0.50	0.05	0.618	0.732	
	120	25	7.5	9	2	0.75	0.05	0.923	0.960	
	120	25	7.5	8	1.5	0.33	0.05	0.325	0.446	0.924
	120	25	7.5	8.5	1.5	0.66	0.05	0.854	0.915	
	120	25	7.5	9	1.5	1.00	0.05	0.995	0.998	
	120	25	7.5	7.75	1	0.25	0.05	0.204	0.304	0.616
	120	25	7.5	8	1	0.50	0.05	0.618	0.732	
	120	25	7.5	8.5	1	1.00	0.05	0.995	0.998	

Power calculations for mean comparisons. In panel A, the comparison is for any pair of treatments, with equal variance and sample size; in panels B and C, we consider comparisons between the expected largest and smallest groups within each treatment. With 80% power, the equivalent standardized minimum effect sizes δ/σ in column (10) are 0.280, 0.524, and 0.616 in panels A, B, and, respectively, C.

C.4 Robustness Check: Different Samples

Attention and manipulation checks To examine the extent to which subjects pay attention to the experiment, we included an attention check in our compassion subscale, asking subjects to select a particular item (i.e., 'Somewhat disagree'). In addition, we included a manipulation check, asking subjects to recall the bonus offered by the social enterprise contract. 95.35% of the 796 subjects passed the attention check by clicking on the required option, but only 55.90% passed the manipulation check, correctly recalling the SE bonus. Rather than being due to poor understanding, the latter may be due to subjects not correctly assigning the social enterprise nomenclature to a particular contract, as the contracts subjects encountered in Parts 2 and 3 only included the company description and not a particular label. In other words, while the SE was described as a company for which both commercial and social tasks are important, the 'social enterprise' label was never used. The unfortunate choice of wording in the manipulation check may have thus created confusion and led to wrong answers. This question was also among the last asked, such that fatigue could have set in. Passing or failing the attention check is independent of treatment, such that attrition for this reason is random (and generally, those who pass either check are not different from those who fail on meaningful dimensions). Nonetheless, we exclude the 2.01% of subjects who failed *both* of these checks, although we have verified that including these subjects in our analyses does not affect our results. Table C.4.1 shows SE social effort for different samples, relaxing and tightening restrictions around i) slider placement, ii) attention and manipulation checks, iii) gender, and iv) time taken to complete the experiment.[7] Results are very similar across panels, including those where we require subjects to pass the attention check (Panels C and D), to pass at least one of the two checks (Panels A and E), and where we do not impose a restriction around attention and manipulation checks (Panels B, F, and G).

Definition of gender To stratify our randomization by gender, we used the pre-screening feature on the Prolific platform. This feature uses questions the platform previously asked its participants with regards to gender, allowing us to target our experiment at different subgroups. More specifically, we ran two identical experiments, restricting potential subjects to men in one and women in the other. To ensure subjects are not aware of this aspect of our experimental design and as a data quality check, we also ask subjects for their gender in Part 4. The pre-existing platform variable is consistent with the questionnaire answer, with an agreement rate of 99.26%. Throughout the analysis, we restrict the sample to observations where the two gender variables agree. Our results are robust to relaxing this restriction, as can be seen in Table C.4.1 by comparing Panels A, C, and E on the one hand, and Panels B, D, F, and G on the other.

Slider task placement In the experiment, we required subjects to position sliders exactly at 25 and 75 in order to produce a unit of real effort.[8] Indeed, 95.04% of the total 47,760 sliders were

[7] χ^2-tests cannot reject that subjects are uniformly distributed across treatments ($p > 0.75$ in all samples).

[8] The original task in Gill and Prowse (2012) is designed to measure total effort and consists of 48 sliders to be placed at exactly 50 in the space of two minutes. We use 15 slider per contract to reduce the likelihood of subjects becoming bored with the task. In addition, our use of positions 25 and 75 as focal points is purely a matter of labeling, which we make clear to our subjects. A pilot experiment confirmed that these labels did not affect the decisions made by subjects and that the task is neither trivial, as it is not immediately obvious where precisely positions 25 and 75 are found, nor prohibitively difficult. As the effort required to move the slider in each direction is identical, differences in individuals' cost of taking the commercial or social action are only driven by social motivation differences. Alternatively, subjects could have moved the slider to their preferred distribution between commercial and social effort on a 0-100 scale; however, this effort allocation measure is similar to the dictator game we employ to measure altruism. We believe that allowing individuals to allocate effort in a binary manner across 15 sliders carries less risk of introducing a purely mechanical relationship between social preferences and effort allocations, avoiding common method bias. Furthermore, we use 15 sliders – rather than the 10 we used in the pilot experiment – in order to limit any scale similarity between the slider task and the dictator game.

positioned correctly, and this does not differ by company type. However, despite an intention to exert commercial or social effort, there may be minor errors in positioning the slider. For instance, the slider could be positioned at 23, 24, 26, 27, 73, 74, 75, or 76, and these represent 0.86% of sliders. In our main results, we count minor deviations as units of effort under the assumption that they closely match an intention to place the slider precisely, but our results are unchanged when we only use precisely placed sliders, as Panels A and B of Table C.4.1 show. The remaining 4.1% of sliders are placed at other numbers, and in some observations more than half the sliders are inadequately placed. These subjects moved sliders more or less randomly and we drop them from the analysis; their inclusion attenuates our results only slightly, see Panels E, F, and G in Table C.4.1. Finally, a small number of participants placed sliders exclusively at 0 or 100, which indicate the direction of effort intended, but are clear deviations, such that they are not included in our main sample; recoding these observations (as 25 and 75) to count as units of effort leaves our results virtually identical.

Duration outliers There were several outliers with regards to the duration of the experiment, i.e. 2.1% of subjects took less than 10 minutes and 1.5% of subjects took more than 40 minutes. For the former, a short completion time may signal low attention paid to the task, reducing the quality of the data we obtain. The most likely reasons for the latter are that the session was left running while the subject was away temporarily or that a connection timed-out temporarily; either way, subjects may have paid less attention to the study. Our main sample excludes these observations, but the results are robust to including them, as can be seen in Table C.4.1 by comparing Panels A, C, and E on the one hand, and Panels B, D, F, and G on the other.

Answer consistency In the sorting condition, subjects choose their preferred contract and perform the slider task again. Consequently, subjects perform one contract (either FP, NP, or SE) twice, raising concerns about answer consistency. Reassuringly, the correlations between social effort levels with and without sorting are 0.681, 0.456, and 0.703 for individuals choosing the FP, NP, and respectively, SE contract. In addition, consistency in repeated contracts does not vary with treatment level ($\chi^2 = 0.634$, $p > 0.5$). Figure C.4.1 shows a scatter plot of social effort with and without sorting for individuals choosing the SE contract, weighted by number of observations. Most data points lie along the diagonal, suggesting no or minor deviations in repeated contracts. Overall, concerns about consistency do not threaten the validity of our results.

Pilot experiment Prior to completing the experiment we analyze in this paper, we conducted a pilot with 183 subjects, designed to guide our experimental design and power calculations. The main difference between the two lies in the SE contract description. Whereas we now write that "It is in the best interests of the company that both tasks receive attention", the pilot informed subjects that "The company cares equally about both tasks". We deemed this phrasing to provide too strong an anchor on a balanced effort allocation (a 50/50 split) and unrealistic to a certain extent. We preferred to give a more ambiguous description instead, allowing subjects to allocate their effort according to their perception of company needs. In the pilot, subjects were only required to move 10 sliders per contract, which we changed to 15 sliders per contract in order to remove any perceived similarity to the £10 dictator game. Moreover, the pilot did not include a £0.25 treatment and was not stratified by gender. Nonetheless, the pilot results – summarized in Figure C.4.2 – display a similar pattern as the results we present in Figure 4.1: adverse specialization on the social task arises in the absence of pay for performance, while bonuses induce a more balanced effort allocation without reducing social motivation levels, regardless of the incentive steepness (although there is an elevated risk of mission drift in the £1 treatment).

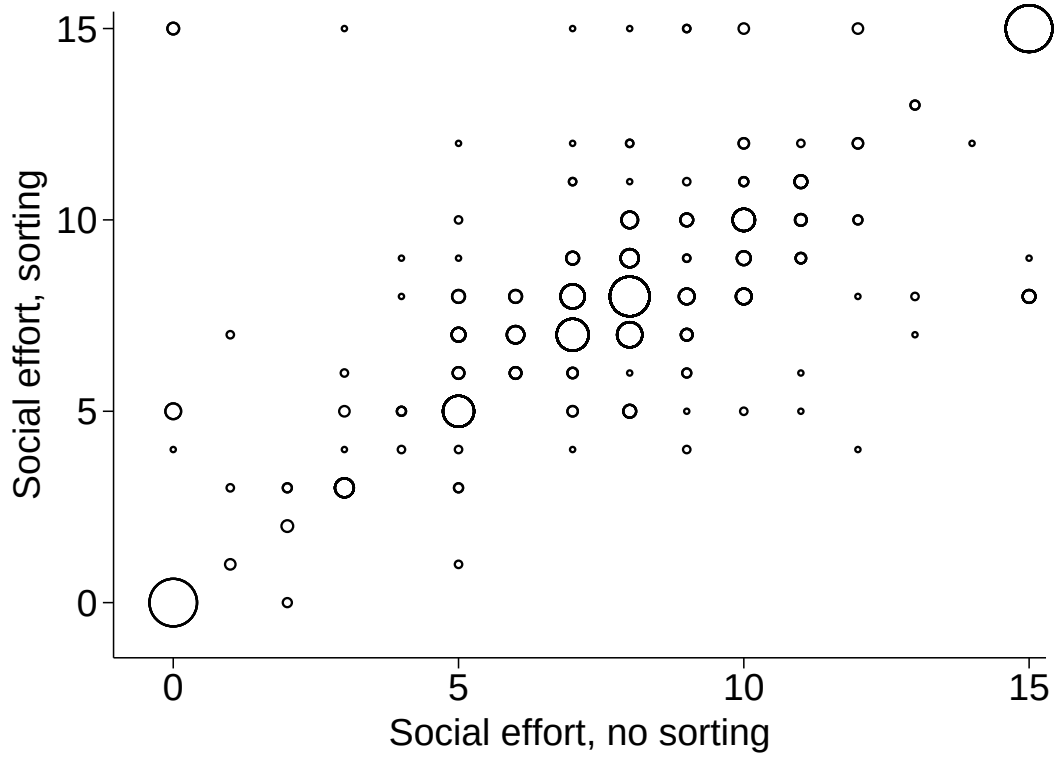

Figure C.4.1: Answer consistency across SE contracts with and without sorting, for subjects who performed the SE contract twice.

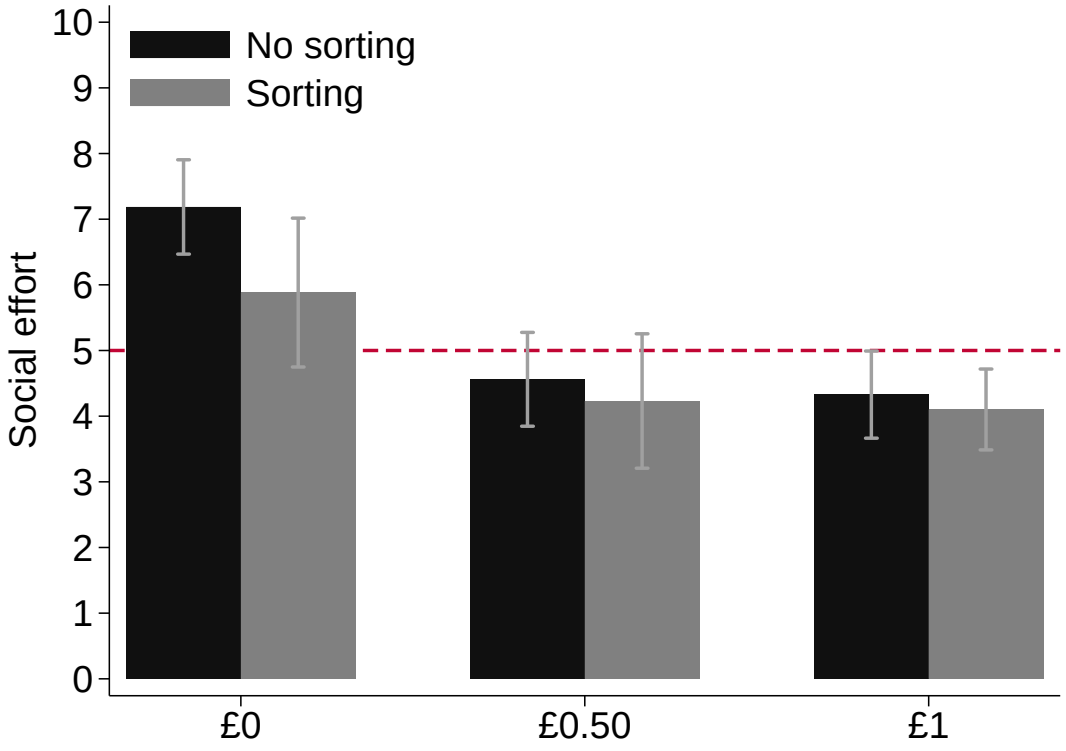

Figure C.4.2: Pilot data social effort in social enterprises by treatment, with 95% confidence intervals; the dashed line represents a fully balanced effort allocation.

Table C.4.1: **Social Enterprise Social Effort: Other Samples**

	No sorting				Sorting			
	(1) £0	(2) £0.25	(3) £0.50	(4) £1	(5) £0	(6) £0.25	(7) £0.50	(8) £1
A. $NS = 0, F = F_p, 10 \leq t \leq 40$, pass either check ($N = 594$, $N_{SE} = 291$)								
Units	11.000	7.099	7.088	7.338	10.350	7.390	7.951	7.504
	(3.599)	(3.672)	(3.762)	(3.741)	(3.416)	(3.140)	(3.054)	(2.572)
Share	0.733	0.473	0.472	0.489	0.690	0.492	0.530	0.500
	(0.239)	(0.244)	(0.250)	(0.249)	(0.227)	(0.209)	(0.203)	(0.171)
B. $NS = 0$ ($N = 623$, $N_{SE} = 307$)								
Units	10.913	7.238	7.141	7.463	10.302	7.405	7.940	7.459
	(3.676)	(3.703)	(3.776)	(3.774)	(3.447)	(3.025)	(3.019)	(2.620)
Share	0.727	0.482	0.476	0.497	0.686	0.493	0.529	0.497
	(0.245)	(0.246)	(0.251)	(0.251)	(0.229)	(0.201)	(0.201)	(0.174)
C. $NS \leq 10, F = F_p, 10 \leq t \leq 40$, pass attention check ($N = 686$, $N_{SE} = 332$)								
Units	10.928	7.088	7.165	7.323	10.687	7.362	8.000	7.590
	(3.612)	(3.760)	(3.587)	(3.526)	(3.421)	(3.180)	(3.018)	(2.557)
Share	0.728	0.474	0.478	0.490	0.712	0.491	0.535	0.506
	(0.240)	(0.249)	(0.239)	(0.234)	(0.228)	(0.210)	(0.200)	(0.170)
D. $IS \leq 10$, pass attention check ($N = 717$, $N_{SE} = 346$)								
Units	10.948	7.173	7.215	7.325	10.775	7.378	7.989	7.500
	(3.649)	(3.757)	(3.592)	(3.633)	(3.441)	(3.073)	(2.987)	(2.682)
Share	0.729	0.479	0.481	0.491	0.718	0.492	0.534	0.500
	(0.243)	(0.249)	(0.239)	(0.242)	(0.229)	(0.203)	(0.198)	(0.178)
E. $IS \leq 30, F = F_p, 10 \leq t \leq 40$, pass either check ($N = 722$, $N_{SE} = 349$)								
Units	10.794	7.088	7.112	7.222	10.274	7.426	7.978	7.500
	(3.714)	(3.743)	(3.659)	(3.599)	(3.800)	(3.333)	(2.992)	(2.632)
Share	0.727	0.480	0.474	0.484	0.706	0.496	0.539	0.503
	(0.239)	(0.250)	(0.243)	(0.239)	(0.225)	(0.222)	(0.204)	(0.170)
F. $IS \leq 30$ ($N = 767$, $N_{SE} = 372$)								
Units	10.700	7.149	7.158	7.272	10.321	7.566	7.959	7.407
	(3.827)	(3.775)	(3.635)	(3.664)	(3.785)	(3.298)	(2.949)	(2.735)
Share	0.724	0.488	0.478	0.487	0.709	0.505	0.538	0.497
	(0.242)	(0.254)	(0.242)	(0.244)	(0.226)	(0.219)	(0.201)	(0.177)
G. Full sample ($N = 796$, $N_{SE} = 388$)								
Units	10.239	6.984	6.901	7.040	9.419	7.388	7.774	7.208
	(4.296)	(3.868)	(3.790)	(3.802)	(4.550)	(3.457)	(3.101)	(2.935)
Share	0.717	0.493	0.479	0.489	0.717	0.505	0.547	0.500
	(0.252)	(0.258)	(0.246)	(0.245)	(0.247)	(0.247)	(0.209)	(0.182)

These sampling criteria, though not comprehensive, cover choices regarding slider placement, duration, gender, and attention checks. We display social effort as units and shares due to the larger number of imprecisely placed sliders not counted towards the total in some panels. Standard deviations in parentheses. N and N_{SE} = total and SE choice sample size, respectively. NS = incorrect sliders, including imprecise sliders (e.g. 23 is incorrect); IS = incorrect sliders, excluding imprecise sliders (e.g. 23 is correct); F, F_p = gender variables from experiment and Prolific, respectively; t = experiment time.

C.5 Robustness Check: Social Preferences

Composite social motivation measure Since social motivation may entail different aspects – altruism, compassion, reciprocity, etc. –, none of the individual measures of social preferences may perfectly capture this complex concept. To extract the maximum information from the various measures we collect, we perform a principal component analysis. We find that our six social preference variables load onto a single factor with Eigenvalue larger than 1 accounting for 80.78% of variance, which we label *Social motivation* (see Table C.5.1). *Inequality aversion* loads negatively on this factor and *Altruism* has a smaller loading than our other measures, suggesting that this game-theoretic measurement may be an imperfect proxy for social motivation (see also Figure C.5.1 for variable loadings on the first two factors). Due to its broader nature, we use *Social motivation* throughout the experiment, together with *Compassion*. Note that including *Risk preferences* and *Time preferences*, potentially correlated with social preferences, in the principal factor analysis produce similar results, as does using the individuals variables underlying *Prosocial behavior*.

Revealed preference social motivation Social task effort in the FP contract without sorting may also provide a measure of social motivation, because individuals renounce personal pay-offs in order to exert social task effort. This *revealed preference* measure is positively correlated with our other social preference measures, loads positively on the *Social motivation* factor, and produces similar results as the other measures (available upon request). However, due to the random order of Part 2 contracts, the SE bonus is revealed to some subjects before they perform the FP contract; it could thus be contaminated by the treatment in a way that is correlated with subsequent choices, such that *Compassion* and *Social motivation* provide cleaner measures.

Changes in social motivation In Table 4.3, we provide a series of tests for equality of means, variances, and distributions in *Compassion* and *Social motivation* (plotted in Figure C.5.2). We also estimate linear regressions of these social preference measures for individuals who select into social enterprises on the treatment dummies. The results in columns (1) and (2) of Table C.5.2 show some motivation crowd-out in the £1 treatment. To examine distributional changes, we create dummies for whether individuals are in the bottom or top 25% of individuals in a given measure, and estimate linear probability models for their presence in social enterprises. In the £1 treatment, we find an increase (decrease) in the number of individuals at the bottom (top) of the distribution of *Social Motivation*. Our measure of compassion registers no distributional shifts across treatments. Moreover, only the selection of low *Social motivation* individuals into social enterprises in the £1 treatment survives multiple hypotheses test adjustments ($p = 0.011$, without controls) (List et al., 2018). Another way to analyze such shifts is to perform quantile regressions of social preference variables on treatment dummies. Table C.5.3 suggests that the *Social motivation* of individuals who select into the SE contract is reduced across the distribution in the £1 treatment, although this is only weakly significant; *Compassion* is unaffected.

Alternative social motivation measures While incentivized measures are preferable to hypothetical ones, it is important to show how sensitive our results are to using different constructs. In addition, social preference games in the lab (e.g., dictator) may not accurately capture social motivation in the field (Levitt and List, 2007; Galizzi and Navarro-Martinez, 2018). We complement such measures with psychological scales and hypothetical questions to alleviate this external validity concern (and the main analysis focuses on *Compassion* and a composite *Social motivation* factor). The results using these alternative measures are shown in columns (1)-(5) of Table C.5.4. Increasing incentives are correlated with lower levels of social preferences in the SE contract, in particular altruism and willingness to share when the bonus is £1. However, the List et al. (2018)

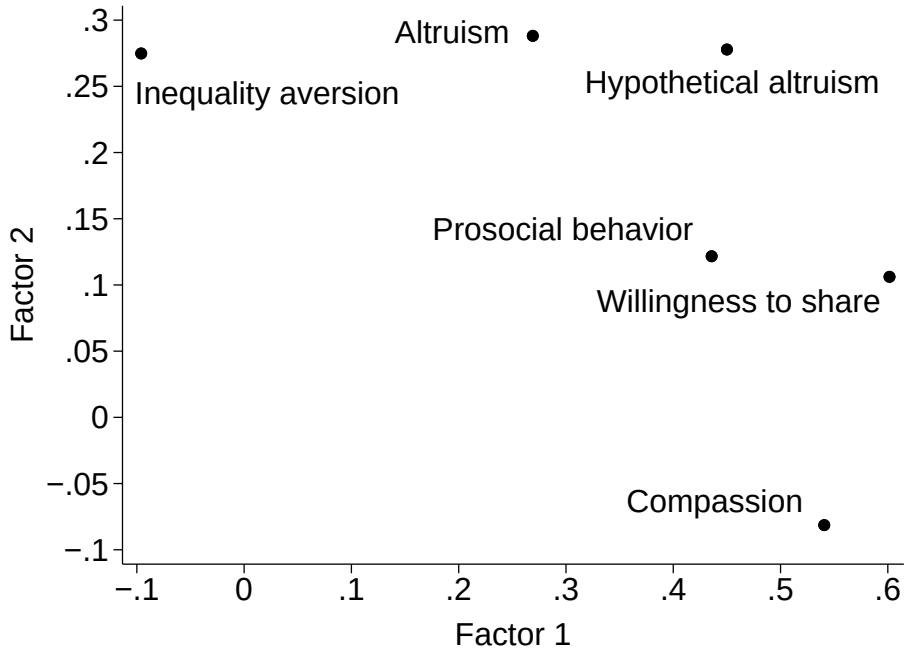

Figure C.5.1: Social preference loadings on the first two factors.

multiple hypothesis testing adjustment renders all coefficients statistically insignificant at conventional levels ($p > 0.1$), suggesting that strong incentives do not attract significantly less motivated workers. Interestingly, column (2) suggests that incentives' potential to widen the distribution of individual payoffs does not deter inequality-averse individuals; workers do not seem to perceive incentives as 'unfair' from a redistribution perspective, or at least do not anticipate this consequence. As columns (6) and (7) suggest, higher SE incentive levels do not attract individuals with a higher risk propensity or more myopic individuals.[9]

Social preferences by contract choice One argument for why adverse specialization occurs in social enterprises relates to the highly socially motivated individuals who join this organizational form. Regardless of the SE bonus, we expect that other-regarding preferences are lowest for self-selected FP workers and highest for self-selected NP workers; SE workers are in between, with some differences across treatments. To see this, Table C.5.5 presents a regression analysis counterpart to the comparisons in Table 4.3, considering subjects make a single choice between the three contracts: SE motivation is different from FP motivation but not NP motivation outside of the £1 treatment. To examine this possibility, we regress our social preference measures on dummies for Part 3 contract choices, controlling for treatment and choice of good cause (i.e. mission fixed effects). Table C.5.6 shows that our expectation is met for SE and FP worker comparisons, with the exception of *Inequality aversion*, *Risk preferences*, and *Time preferences*. It does not appear that more inequality averse or less risk tolerant individuals join SEs, although SE workers put more weight on the future relative to FP workers. While FP and NP workers are highly different in their social preferences, SE and NP workers are remarkably similar, with a statistical difference observed only for *Hypothetical altruism* (otherwise $p > 0.1$). This supports our argument that individuals selecting into SEs are highly socially motivated, which may result in adverse specialization when pay for performance is not used.

[9] We have also checked that stronger incentives do not attract individuals with higher education, or with higher or lower income levels. They appear to attract individuals who took longer to complete the comprehension check, but multiple hypothesis testing adjustments eliminate the significant coefficients.

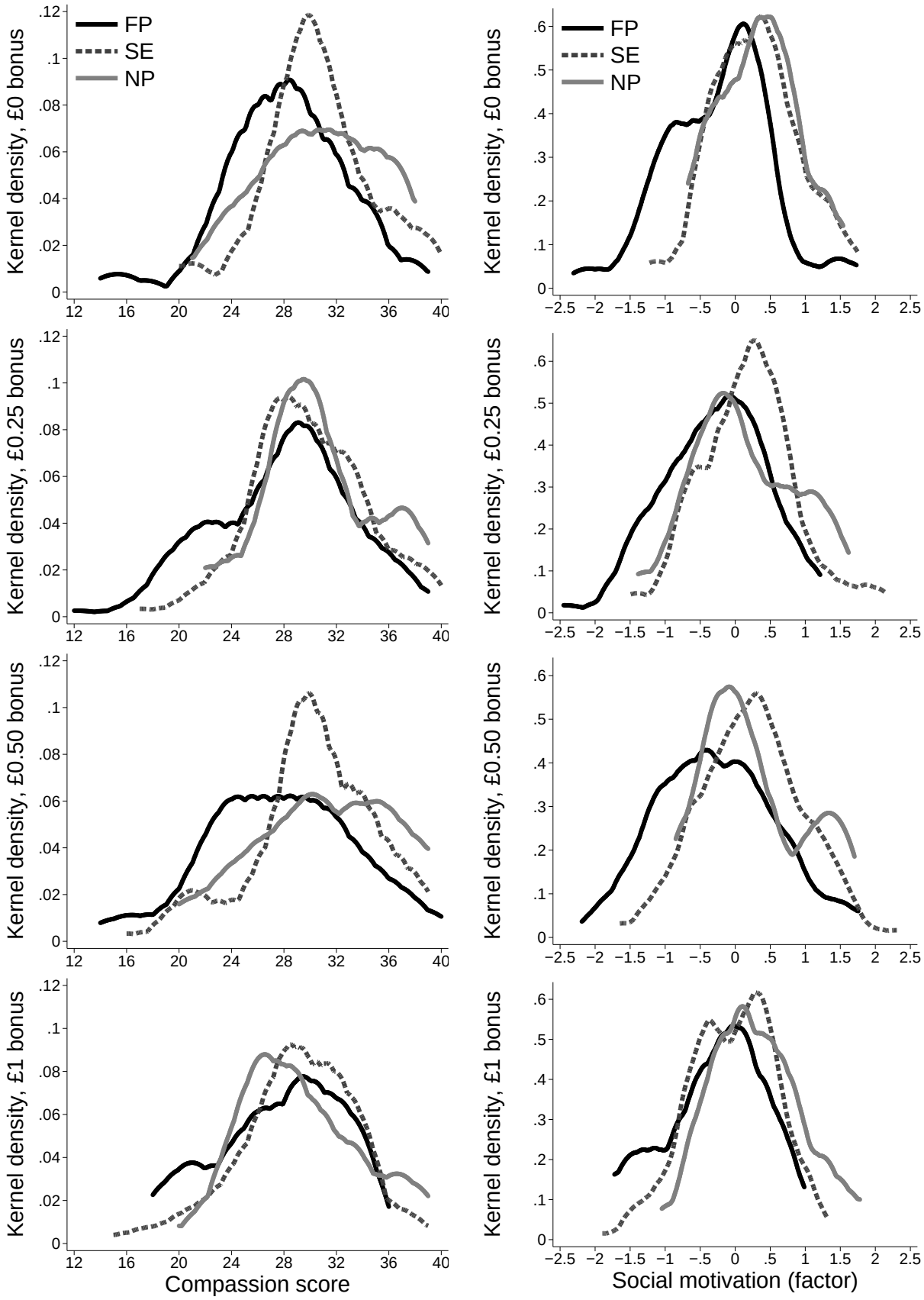

Figure C.5.2: Distribution of *Compassion* and *Social motivation*, by treatment and contract choice.

Table C.5.1: **Composite Social Preferences: Factor Loadings**

Variable	Factor 1	Factor 2	Uniqueness
Compassion	0.5407	-0.0814	0.7010
Altruism	0.2691	0.2881	0.8446
Inequality aversion	-0.0960	0.2748	0.9153
Hypothetical altruism	0.4501	0.2778	0.7203
Willingness to share	0.6016	0.1061	0.6268
Prosocial behavior	0.4358	0.1217	0.7953
Eigenvalue	1.1953	0.2015	
Variance explained	80.78%	19.22%	
Label	*Social motivation*		

Factor loadings for principal component analysis with an orthogonal varimax rotation; results are similar with oblique (non-orthogonal) rotations. As a social context may interact with risk and time preferences, we have also checked that including these variables in our measure of motivation does not impact the results. Reassuringly, the results are qualitatively similar. Risk and time preferences load more on Factor 2, so the Factor 1 has a slightly smaller, yet still dominant, explanatory power. Results are also similar when we include the revealed social preferences from the FP contract or use the 5 items that comprise *Prosocial behavior* individually.

Table C.5.2: **Social Preferences, Conditional on Social Enterprise Sorting**

	Mean		Bottom 25%		Top 25%	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Compassion						
£0.25	-0.122	-0.074	0.022	-0.005	-0.005	0.007
	(0.162)	(0.170)	(0.067)	(0.068)	(0.082)	(0.085)
£0.50	-0.064	0.030	0.027	0.001	0.064	0.106
	(0.158)	(0.153)	(0.064)	(0.062)	(0.080)	(0.080)
£1	-0.304**	-0.184	0.113*	0.087	-0.001	0.044
	(0.148)	(0.155)	(0.064)	(0.065)	(0.075)	(0.078)
R^2	0.016	0.143	0.013	0.112	0.004	0.110
B. Social motivation						
£0.25	-0.097	-0.073	0.144**	0.138**	-0.059	-0.039
	(0.123)	(0.123)	(0.059)	(0.062)	(0.089)	(0.089)
£0.50	-0.070	-0.025	0.098*	0.095*	-0.047	-0.012
	(0.119)	(0.117)	(0.051)	(0.054)	(0.086)	(0.085)
£1	-0.339***	-0.274***	0.187***	0.177***	-0.204***	-0.170**
	(0.107)	(0.112)	(0.052)	(0.058)	(0.078)	(0.081)
R^2	0.039	0.137	0.026	0.100	0.032	0.134
Controls	No	Yes	No	Yes	No	Yes

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. $N = 341$. Robust standard errors in parentheses. Columns present regressions of *Compassion* (standardized) and *Social motivation* (factor) on treatment dummies; the baseline category is the £0 treatment. Controls include age, gender, studentship, education, income, risk and time preferences, and choice of good cause.

Table C.5.3: **Social Preferences in Social Enterprises: Quantile Regressions**

	(1) 10^{th} pct.	(2) 25^{th} pct.	(3) 50^{th} pct.	(4) 75^{th} pct.	(5) 90^{th} pct.
A. Compassion					
£0.25	0.295	-0.006	-0.256	0.080	0.115
	(0.347)	(0.261)	(0.226)	(0.252)	(0.420)
£0.50	0.201	0.179	0.039	0.181	-0.101
	(0.302)	(0.232)	(0.212)	(0.222)	(0.323)
£1	-0.115	0.030	-0.260	-0.121	-0.374
	(0.301)	(0.226)	(0.229)	(0.190)	(0.318)
B. Social motivation					
£0.25	-0.106	-0.151	-0.060	-0.051	-0.045
	(0.205)	(0.181)	(0.156)	(0.172)	(0.242)
£0.50	-0.239	-0.067	0.036	0.043	-0.003
	(0.207)	(0.182)	(0.161)	(0.182)	(0.205)
£1	-0.317*	-0.297*	-0.260	-0.259	-0.359*
	(0.186)	(0.177)	(0.163)	(0.176)	(0.209)

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. $N = 341$. Bootstrapped standard errors in parentheses (1000 replications). Columns present quantile regressions of *Compassion* (standardized) and *Social motivation* (factor) on treatment dummies for the 10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th} quantiles; the baseline category is the £0 treatment. Controls include age, gender, income, studentship, education, risk and time preferences, and choice of good cause.

Table C.5.4: **Social Preferences by Treatment, Conditional on Social Enterprise Sorting**

	Altruism (1)	Inequality aversion (2)	Hypothetical Altruism (3)	Willing to share (4)	Prosocial behavior (5)	Risk (6)	Time (7)
£0.25	-0.389	-0.316	14.214	-0.150	-0.291	-0.136	-0.076
	(0.308)	(0.384)	(29.871)	(0.375)	(0.208)	(0.437)	(0.378)
£0.50	-0.497*	-0.149	2.896	-0.039	-0.189	0.167	-0.109
	(0.294)	(0.366)	(28.500)	(0.358)	(0.198)	(0.417)	(0.361)
£1	-0.762***	-0.199	-43.925	-0.810**	-0.295	-0.043	-0.555
	(0.281)	(0.350)	(0.262)	(0.343)	(0.189)	(0.399)	(0.345)
Test of joint significance p-value:							
	0.051	0.870	0.051	0.014	0.426	0.862	0.217
R^2	0.023	0.002	0.023	0.031	0.008	0.002	0.013

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. $N = 341$. Standard errors in parentheses. Results from regressions of social preference measures on treatment dummies. The baseline category comprises the £0 treatment. Controlling for age, gender, income, studentship, education, risk and time preferences, and choice of good cause does not alter the qualitative picture. Multiple hypothesis testing adjustments render all coefficients statistically insignificant at conventional levels ($p > 0.1$).

Table C.5.5: **Motivation across Contracts, with Sorting**

	Compassion (standardized)				Social motivation (factor)			
	(1) £0	(2) £0.25	(3) £0.50	(4) £1	(5) £0	(6) £0.25	(7) £0.50	(8) £1
FP	-0.472***	-0.418***	-0.523***	-0.289	-0.543***	-0.494***	-0.509***	-0.248
	(0.163)	(0.159)	(0.164)	(0.209)	(0.124)	(0.119)	(0.128)	(0.159)
NP	0.002	0.109	0.186	0.076	0.020	-0.078	0.063	0.353**
	(0.211)	(0.223)	(0.262)	(0.214)	(0.143)	(0.188)	(0.179)	(0.150)
R^2	0.071	0.052	0.107	0.018	0.137	0.099	0.124	0.061
N	170	178	185	175	170	178	185	175

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses. We regress *Compassion* or *Social Motivation* on subjects' choice of contract, using the SE motivation means in Panel B of Table 4.3 as a baseline.

Table C.5.6: **Social Preferences by Contract Choice**

	Compassion (1)	Altruism (2)	Inequality aversion (3)	Hypothetical Altruism (4)	Willing to share (5)	Prosocial behavior (6)	Social motivation (7)	Risk (8)	Time (9)
FP	28.873***	3.716***	2.695***	96.148***	6.015***	1.307**	-0.136*	5.086***	6.209***
	(0.496)	(0.248)	(0.236)	(13.888)	(0.264)	(0.137)	(0.081)	(0.264)	(0.216)
SE	30.977***	4.822***	2.496***	143.172***	7.169***	1.534***	0.307***	5.176***	6.643***
	(0.522)	(0.237)	(0.247)	(18.353)	(0.262)	(0.149)	(0.083)	(0.280)	(0.230)
NP	31.514***	4.913***	2.685***	192.515***	7.371***	1.527***	0.427***	5.044***	6.607***
	(0.647)	(0.251)	(0.313)	(22.131)	(0.314)	(0.153)	(0.094)	(0.341)	(0.305)
Test of equality p-values:									
FP vs SE	0.000	0.000	0.312	0.001	0.000	0.031	0.000	0.675	0.020
FP vs NP	0.000	0.000	0.971	0.000	0.000	0.086	0.000	0.894	0.157
NP vs SE	0.338	0.663	0.499	0.019	0.420	0.950	0.142	0.666	0.888
R^2	0.973	0.801	0.588	0.442	0.888	0.546	0.093	0.812	0.900

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. $N = 708$. Robust standard errors in parentheses. Results from regressions of social preferences on contract choice (suppressing the constant), with fixed effects for choice of treatment and mission.

C.6 Robustness Check: Effort Measures

Share of social effort Our main analysis focuses on sliders moved to the position equivalent to social effort, but our sampling restrictions include some observations where not all 15 sliders in a contract were placed correctly. This may create problems in interpreting results using social effort if total effort is not adjusted accordingly. For example, for one individual 6 units of social effort out of 15 correctly placed sliders result in 40% social effort, while for another 6 units of social effort out of 10 correctly placed sliders result in 60% social effort.[10] To address this, we assess the effects of treatment on the share of effort exerted in the social task, i.e. social effort as a fraction of total effort. The results for this dependent variable in Table C.6.1 completely mirror those in Table 4.2 for social effort units. Throughout this section, we show results for both dependent variables in order to ensure robustness (and our multiple hypothesis testing adjustments take this into account). Considering the different sampling restrictions with regards to slider placement in Table C.4.1, using units of social effort becomes more problematic when including subjects with more incorrectly placed sliders, which may attenuate our adverse specialization results for the £0 bonus SE. Compare, for instance, column (1) in Table C.4.1, where we progressively relax slider placement restrictions. The units of social effort go down from 11 in Panel A, where we restrict the sample to subjects who only placed sliders at 25 and 75, to 10.24 in Panel G, where all subjects are included, and sliders placed at 23-27 and 73-77 are considered correct. While units of social effort decrease, the share of social effort only varies between 0.733 and 0.717, suggesting that this measure captures effort allocation well regardless of slider placement restrictions. That results across both variables are very similar is encouraging, and we focus our analysis on units of social effort due to its higher transparency.

Fixed effects models Without sorting, all subjects perform the FP, NP, and SE contracts. Comparisons across contracts must then adjust for the paired nature of the test. More specifically, the results in Table 4.2 suggest the SE contract is always different from the NP and FP contracts in simple and paired t-tests. An alternative way to account for non-independence is to estimate individual fixed effects models. We regress social effort (as units or shares) on dummies for SE and NP contracts, using Part 2 observations only and the FP contract as a baseline. The results in Table C.6.2 confirm our results: social effort is higher in the SE and NP contracts relative to the FP contract; the former are different from each other, with $p < 0.0001$, although the gap is much smaller in the £0 treatment.

Adverse specialization The top panel of Figure 4.2 shows that 30%-40% of social enterprise workers in the £0 treatment only exert social effort. This bimodal distribution stands in contrast to a distribution centered around the SE social effort average, suggesting adverse specialization is driven by a subgroup of workers, rather than by higher social effort across the board. What drives this behavior? Our theoretical framework implies that a high level of social motivation should increase the likelihood that workers exert only social effort. To examine this, we regress a dummy for maximum social effort on *Compassion* and *Social motivation* in Table C.6.3. With or without sorting, there does not appear to a linear association between *Social motivation* and maximum social effort.[11] More compassionate workers are more likely to exert maximum effort, especially in the top decile of the distribution; the direction is the same for *Social motivation*, although these results are not significant (potentially due to small sample size). However, these individuals have

[10] Note that we do not find differences in SE total effort across treatments, regardless of the sample we use.

[11] Although the negative effects of *Social motivation* on adverse specialization in Panel B are not statistically significant, the quadratic results suggest that both the least and the most motivated individuals exert maximum social effort. The former may do so as a response to performing a contract they would not otherwise have chosen and may feel compelled to exert substantial social effort (see also Lazear et al., 2012).

self-selected into the SE contract and are more motivated than those who self-selected into the FP contract, as per Table C.5.5. Figure C.6.1 displays a scatter plot of SE *Social effort* in the £0 treatment and *Social motivation*. Those who self-select into the SE (the gray dots) are more likely to have higher motivation, as there are visibly fewer observations with *Social motivation* < 0. They are also more likely to exert maximum social effort, as the concentration of gray dots in the upper right-hand side suggests. Overall, adverse specialization in the absence of monetary incentives appears to be driven especially by individuals with very high levels of compassion. In additional checks, we verified that these individuals experience the largest changes in effort allocation once incentives are introduced (available upon request).

Measures of imbalance In our main analysis, we focus on units of *Social effort* as the most straightforward measure of effort allocation, and compare SE social effort with a fully balanced effort allocation (i.e., 7.5 units) and with FP and NP social effort (and their average). Moreover, results are similar when we perform comparisons using the share of social effort, with a fully balanced effort allocation as a reference (i.e., a 50% share). Because our theory is centered on the notion of balance, we can also capture effort allocation with more direct measures of (im)balance. These measures have a straightforward reference point (i.e., full balance implies a value of zero) and account for incorrectly placed sliders. The difference between social and commercial effort ($S - C$) provides a metric of how dominant the social task is relative to the commercial task, although this variable can become negative if commercial effort dominates; this variable allows for deviations from full balance to cancel each other out and can be considered a flexible measure of overall imbalance. Conversely, deviations from full balance can be considered as distortions regardless of their direction; thus, total imbalance can be conceptualized as the absolute value of the difference between social and commercial effort ($|S-C|$). Table C.6.4 presents the results from using both of these variables, for which a fully balanced effort allocation produces a value of zero. The £0 treatment shows a significant level of imbalance, while all other treatments are associated with significantly more balanced effort allocations (similar across incentive levels). In addition, as Panel C shows, subjects in the top decile of *Compassion* have higher levels of imbalance, consistent with a relationship between social preferences and adverse specialization.

Tobit models Table 4.4 shows the results of linear regressions of the different measures of social effort and balance on treatment dummies, a dummy for the sorting condition, and their interaction. While this represents the simplest and most transparent estimation method, all three dependent variables exhibit a certain degree of censoring. Social effort can only range between 0 and 15, as can the measure of absolute balance, while the share of social effort ranges from 0% to 100%, with around 16% of observations being censored in each case. Therefore, Tobit regressions are a more appropriate estimation technique. The Tobit results we show in Table C.6.5 are fully parallel to the ones obtained with linear regression, suggesting censoring in the dependent variables is not an important concern.

Absence and presence of bonus As our results suggest, SE social effort does not differ significantly between the £0.25, £0.50, and £1 treatments. To examine their joint impact in an analysis of extensive versus intensive margin effects, we aggregate these three treatments into a single *Bonus* dummy, whereas the £0 treatment corresponds to an SE that uses no bonus. Table C.6.6 replicates the analysis in Table 4.4 with this simple dummy for the presence or absence of incentives. Confirming our previous findings, allowing for sorting does not matter for the relationship between incentives and effort allocation.

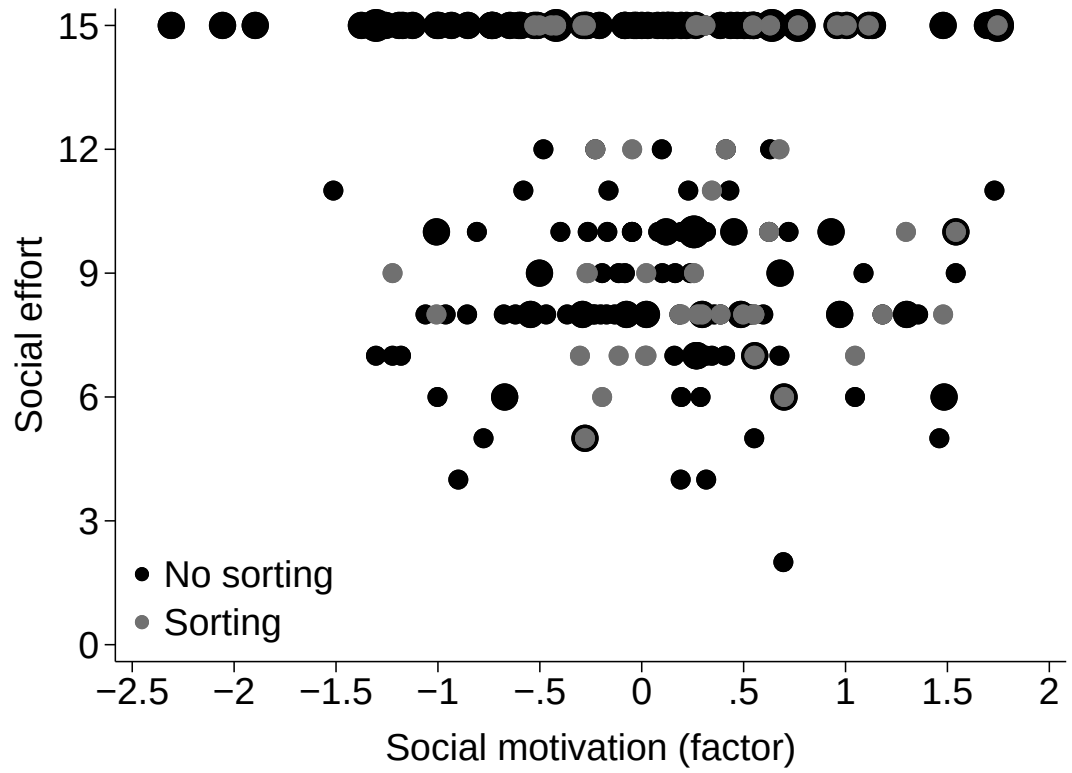

Figure C.6.1: Social effort in the SE contract, £0 treatment.

Table C.6.1: **Social Effort Share, by Contract and Treatment**

	No sorting				Sorting			
	£0 (1)	£0.25 (2)	£0.50 (3)	£1 (4)	£0 (5)	£0.25 (6)	£0.50 (7)	£1 (8)
FP	0.328	0.344	0.349	0.376	0.266	0.276	0.273	0.325
	(0.023)	(0.022)	(0.022)	(0.023)	(0.028)	(0.027)	(0.029)	(0.062)
SE	0.729	0.477	0.475	0.492	0.709	0.504	0.535	0.507
	(0.018)	(0.019)	(0.018)	(0.018)	(0.032)	(0.025)	(0.021)	(0.015)
NP	0.887	0.854	0.869	0.854	0.890	0.917	0.911	0.897
	(0.015)	(0.016)	(0.017)	(0.018)	(0.030)	(0.034)	(0.056)	(0.046)
SE effort t-tests of equality p-values:								
vs £0		0.000	0.000	0.000		0.000	0.000	0.000
vs £0.25			0.922	0.553			0.340	0.925
vs £0.50				0.479				0.256
SE effort t-tests of equality with NP and FP average, t-statistics and p-values:								
	+0.000	−0.000	−0.000	−0.000	+0.001	−0.006	−0.128	−0.012

Standard errors in parentheses. Within each column the FP, NP, and SE social effort levels are different from each other ($p < 0.0001$). We employ matched pair t-tests for the no sorting condition, acknowledging that all individuals performed the slider task in all contract types. In the bottom row, '+' means SE is closer to NP than FP, and '−' means SE is closer to FP than NP.

Table C.6.2: **Social Effort without Sorting: Fixed Effects Models**

	(1) £0	(2) £0.25	(3) £0.50	(4) £1
A. Units of social effort				
SE	6.018***	1.978***	1.881***	1.703***
	(0.426)	(0.331)	(0.319)	(0.336)
NP	8.388***	7.567***	7.757***	7.057***
	(0.455)	(0.433)	(0.425)	(0.443)
B. Share of social effort				
SE	0.401***	0.133***	0.126***	0.116***
	(0.028)	(0.022)	(0.021)	(0.022)
NP	0.559***	0.510***	0.521***	0.478***
	(0.030)	(0.028)	(0.028)	(0.029)
Observations	510	534	555	525
Subjects	170	178	185	175

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Robust standard errors in parentheses. The baseline category consists of social effort in the FP contract. Social effort is different in the SE and NP contracts across all treatments, with $p < 0.0001$.

Table C.6.3: **Adverse Specialization and Motivation**

	No sorting			Sorting		
DV: Maximum social effort	(1)	(2)	(3)	(4)	(5)	(6)
A. Compassion						
Compassion	0.027	0.037		0.235**	0.114*	
	(0.047)	(0.040)		(0.085)	(0.080)	
Compassion^2		0.095***			0.150***	
		(0.025)			(0.050)	
Compassion $\geq 90^{\text{th}}$ pct.			0.363***			0.946***
			(0.134)			(0.148)
R^2	0.093	0.154	0.136	0.579	0.657	0.740
N	170	170	170	49	49	49
B. Social motivation						
Social motivation	-0.186	-0.076		0.241	0.145	
	(0.057)	(0.056)		(0.176)	(0.183)	
Social motivation^2		0.085*			0.146	
		(0.047)			(0.163)	
Social motivation $\geq 90^{\text{th}}$ pct.			-0.021			0.573
			(0.139)			(0.372)
R^2	0.106	0.122	0.091	0.531	0.546	0.566
N	170	170	170	49	49	49

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses. Results from linear regressions of dummies for exerting only social effort in the SE contract with a £0 bonus on subjects' *Compassion* or *Social Motivation* (and their square terms, or a dummy for the top decile of the distribution). Controls include age, gender, income, studentship, education, risk and time preferences, and choice of good cause. Results are also qualitatively similar for the other social preference measures.

Table C.6.4: **Measures of imbalance**

	Absolute imbalance $\|S-C\|$		Absolute imbalance $S-C$	
	No sorting (1)	Sorting (2)	No sorting (3)	Sorting (4)
A. Differences in balance across treatments				
£0 (benchmark)	7.894	6.959	6.871	6.265
	(0.349)	(0.623)	(0.553)	(0.844)
£0.25	-2.197***	-2.192***	-7.522***	-6.128***
	(0.551)	(0.805)	(0.774)	(1.091)
£0.50	-2.570***	-2.523***	-7.632***	-5.212***
	(0.546)	(0.768)	(0.767)	(1.040)
£1	-2.717***	-3.495***	-7.111***	-6.065***
	(0.553)	(0.735)	(0.777)	(0.995)
R^2	0.043	0.064	0.163	0.111
N	708	341	708	341
B. t-tests of equality p-values				
£0.25 vs £0.50	0.490	0.627	0.884	0.321
£0.25 vs £1	0.343	0.043	0.592	0.942
£0.50 vs £1	0.786	0.103	0.493	0.291
C. Compassion				
$\geq 90^{\text{th}}$ pct.	4.590***	9.210***	5.805***	10.885***
	(1.449)	(1.986)	(1.710)	(2.432)
R^2	0.153	0.701	0.175	0.677
N	170	49	170	49
D. Social motivation				
$\geq 90^{\text{th}}$ pct.	-0.202	5.293	-0.461	6.849
	(1.706)	(3.885)	(2.007)	(4.522)
R^2	0.107	0.597	0.123	0.569
N	170	49	170	49

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses. Results from linear regressions of balance measures on treatment dummies (Panel A) and social preference measures (Panels C and D). The significant difference between the £0.25 and £1 treatments under the $|S-C|$ balance measure is eliminated when adjusting for multiple hypothesis testing.

Table C.6.5: **Tobit models: Intensive and Extensive Margin Effects**

	Social effort units		Social effort share		Absolute balance	
	(1)	(2)	(3)	(4)	(5)	(6)
£0.25	-4.683***	-4.822***	-0.310***	-0.319***	-3.074***	-3.284***
	(0.509)	(0.509)	(0.034)	(0.034)	(0.749)	(0.742)
£0.50	-4.825***	-4.936***	-0.321***	-0.328***	-3.395***	-3.672***
	(0.508)	(0.511)	(0.034)	(0.034)	(0.750)	(0.745)
£1	-4.519***	-4.594***	-0.298***	-0.303***	-3.627***	-3.803***
	(0.504)	(0.508)	(0.034)	(0.034)	(0.748)	(0.752)
Sorting	-0.491	-0.412	-0.033	-0.028	-1.210	-1.096
	(0.645)	(0.631)	(0.043)	(0.042)	(1.069)	(1.039)
Sorting × £0.25	0.962	0.876	0.064	0.059	0.169	0.346
	(0.757)	(0.748)	(0.050)	(0.050)	(1.207)	(1.171)
Sorting × £0.50	1.572**	1.485**	0.106**	0.100**	0.102	0.023
	(0.750)	(0.736)	(0.050)	(0.049)	(1.165)	(1.134)
Sorting × £1	0.791	0.704	0.050	0.045	-0.652	-0.734
	(0.709)	(0.695)	(0.047)	(0.046)	(1.136)	(1.103)
Constant	11.832***	12.434***	0.789***	0.829***	9.074***	9.156***
	(0.402)	(0.760)	(0.027)	(0.051)	(0.640)	(1.130)
Controls	No	Yes	No	Yes	No	Yes
N	1,049	1,049	1,049	1,049	1,049	1,049
Left-censored	46 (4.38%)		46 (4.38%)		3 (0.03%)	
Right-censored	128 (12.20%)		128 (12.20%)		174 (16.59%)	
Pseudo-R^2	0.034	0.039	0.240	0.277	0.013	0.022

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the subject level in parentheses. The baseline category is the £0 treatment when sorting is not possible. See Table 4.4 for details.

Table C.6.6: **Aggregating Treatments: Intensive and Extensive Margin Effects**

	Social effort units		Social effort share		Absolute balance	
	(1)	(2)	(3)	(4)	(5)	(6)
Bonus	-3.744***	-3.850***	-0.248***	-0.255***	-2.494***	-2.694***
	(0.318)	(0.322)	(0.021)	(0.021)	(0.510)	(0.513)
Sorting	-0.303	-0.238	-0.020	-0.016	-0.935	-0.875
	(0.470)	(0.463)	(0.031)	(0.031)	(0.837)	(0.822)
Sorting × Bonus	0.824	0.756	0.054	0.050	-0.362	-0.333
	(0.501)	(0.495)	(0.033)	(0.033)	(0.868)	(0.851)
Constant	10.935***	11.544***	0.729***	0.769***	7.894***	7.860***
	(0.277)	(0.630)	(0.018)	(0.042)	(0.466)	(0.923)
Controls	No	Yes	No	Yes	No	Yes
N	1,049	1,049	1,049	1,049	1,049	1,049
R^2	0.151	0.175	0.150	0.173	0.062	0.108

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors clustered at the subject level in parentheses. The baseline category is the £0 treatment when sorting is not possible (i.e. no bonus); *Bonus* captures all other treatments with a positive incentive. See Table 4.4 for details.

C.7 Multiple Hypothesis Testing and Heterogeneity

Multiple hypothesis testing Throughout the analysis we compare social effort across several treatments and outcomes, thus increasing the rate of false positive discoveries (Romano and Wolf, 2005). To alleviate this concern, we follow the procedure described by List et al. (2018) to account for multiple hypothesis testing in conducting pair-wise comparisons between the four treatments (£0, £0.25, £0.05, £1) and two outcomes (social enterprise social effort as units and shares). Table C.7.1 shows unadjusted p-values, List et al. (2018) multiplicity-adjusted p-values, and p-values from the application of conservative Bonferroni and Holm corrections. We perform the tests separately for the conditions with and without sorting. In comparing the no bonus group (£0) with the bonus groups (£0.25, £0.05, £1), significance is not affected: even with the strongest penalties for multiple hypothesis testing, the effort allocation is more balanced when a bonus is present. These results hold for multiple hypothesis testing adjustments accounting for comparisons by gender: with or without worker sorting, the effort allocation is more balanced when the bonus is positive.

Gender differences Women are often found to have stronger other-regarding preferences and to be more likely to engage with social, rather than commercial ventures (Croson and Gneezy, 2009; Dimitriadis et al., 2017). In our data women exhibit higher compassion, higher previous prosocial behavior, lower risk tolerance, and higher *Social motivation*, which survive multiple hypothesis testing adjustments (Romano and Wolf, 2005). This may imply that i) women exert more social effort and potentially exhibit stronger adverse specialization, and ii) the introduction and strength of social enterprise monetary incentives may lead to different sorting patterns and effort allocation for men and women. For these reasons, our randomization was stratified by gender, allowing us to perform comparisons across groups without loss of precision.[12] When we regress social effort on treatment dummies, gender, and their interactions in Table C.7.2, women's social effort is less crowded out by incentives, and significantly so in the £0.50 treatment; however, the differences in the share of effort devoted to the social task are not significant when sorting is allowed. Furthermore, multiple hypothesis testing adjustments suggest that gender differences in the effects of treatment on social effort are not significantly different for men and women, as also seen by plotting *Social effort* for the two groups in Figure C.7.1. In Table C.7.3 we regress our motivation measures on gender, treatment dummies, and their interaction. Women's motivation is crowded out to a smaller extent (as evidenced by the positive interaction coefficients), but not significantly so.

Previous social organization experience Individuals with previous social sector experience – working for or with non-profits or social enterprises – may differ from other individuals in two ways. Their work may have rendered them more socially motivated (Hockerts, 2017) or may have accustomed them to an institutional logic where revenue generation and commercial practices are the exception rather than the norm (Pache and Santos, 2010), so incentives may elicit different reactions from this subgroup. We create a dummy variable for individuals who have worked i) in a non-profit, ii) in a social enterprise, or iii) with a social organization and compare results across groups with and without such experience (results are similar if we also include volunteering and donations). Results for the subsamples of individuals with and without previous experience in the social sector are similar in both the sorting and non-sorting conditions. Individuals with a social sector background exert slightly less social effort, such that their effort allocation in the £0

[12] Bruhn and Mckenzie (2009) recommend controlling for strata dummies when assessing treatment effects in regression analyses. Our regressions with and without controls show that controlling for gender – our stratifying variable – does not affect our overall results.

treatment is slightly more balanced, although adverse specialization is still present. One speculative interpretation may be that, in contrast to the above expectation, over time social sector employees become attuned to organizations' financial issues and exert more effort on the commercial task to compensate for this perceived deficiency. Nonetheless, the differences between those with a social sector background and those without remain small.

Mission heterogeneity Because social preferences may be weakly correlated with good cause choice (Appendix Table C.2.4), we use good cause choice dummies in our regression analyses, effectively performing within-mission analyses. However, this approach does not necessarily imply that the effects do not differ by mission, another potentially important source of heterogeneity. For this reason, we analyze social enterprise *Social effort* separately for each mission, summarizing the results in Figure C.7.2. Despite the small samples in the sorting condition, the results are very similar to our pooled sample, with evidence of adverse specialization in the £0 treatment and effective balanced in the £0.25, £0.50, and £1 treatments, especially when individuals are allowed to select their preferred contract. Differences across chosen good causes are therefore limited and do not add much insight beyond our main conclusions. The uniform effects of monetary incentives across on social effort across these three representative social enterprise missions (which comprise more than 60% of issues tackled by SEs, Mair et al., 2012), also hints at the validity of our results for other types of missions.

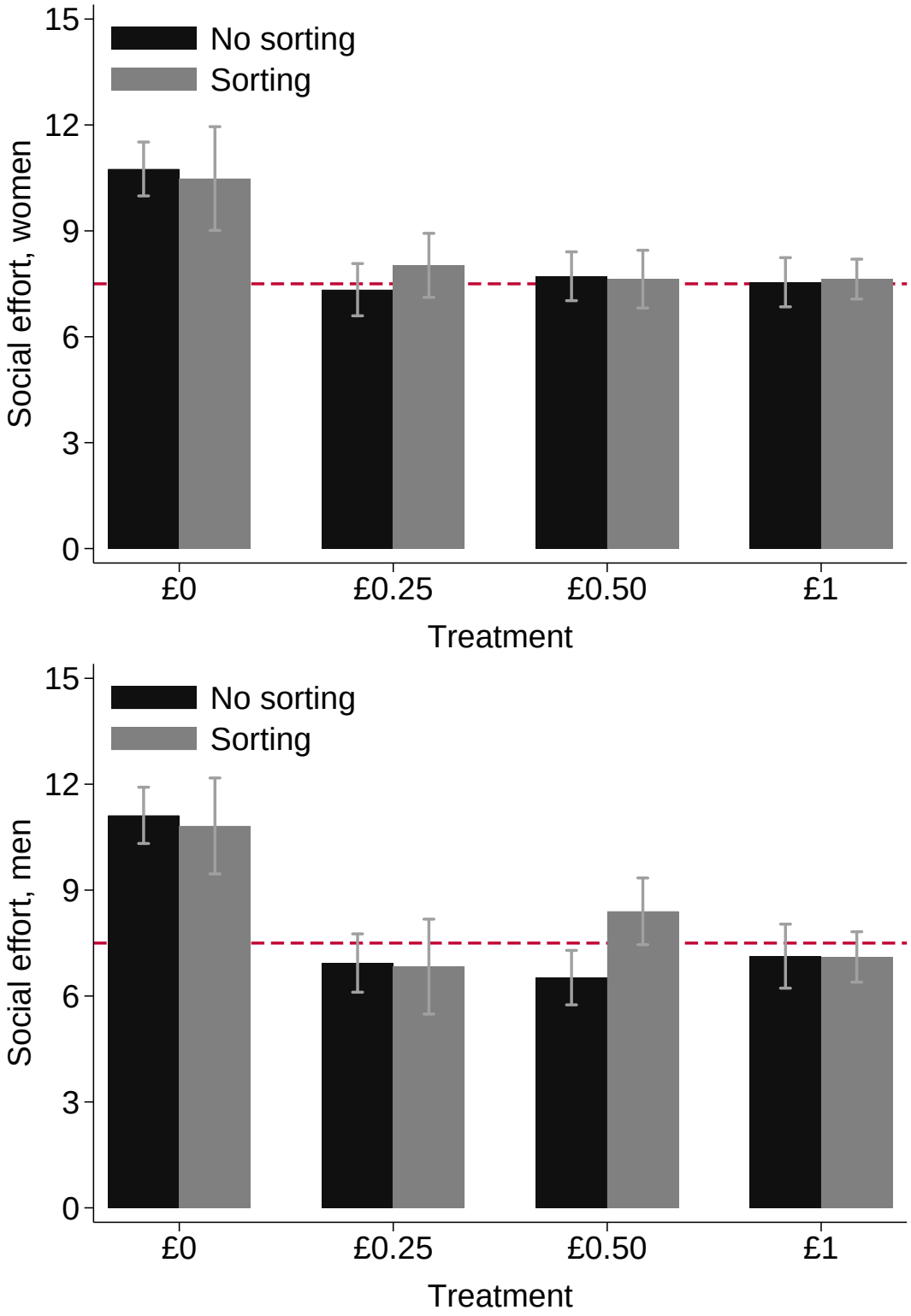

Figure C.7.1: Social effort in social enterprises by treatment for women (top) and men (bottom), with 95% confidence intervals; the dashed line represents a fully balanced effort allocation.

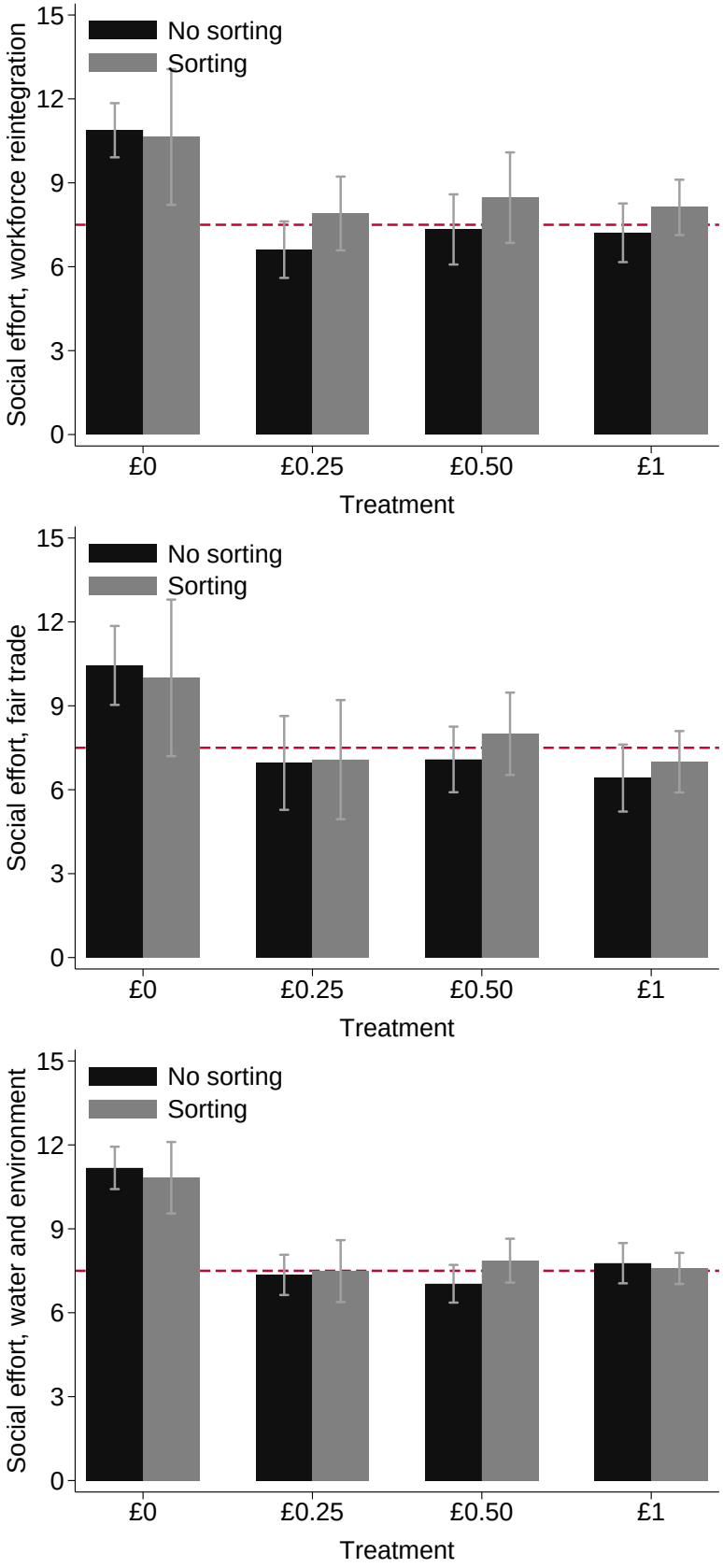

Figure C.7.2: Social effort in social enterprises by treatment and mission, with 95% confidence intervals; the dashed line represents a fully balanced effort allocation.

Table C.7.1: **Multiple Hypothesis Testing Adjustments**

Comparison			p-value			
Group 1 (1)	Group 2 (2)	Difference (3)	Unadjusted (4)	Adjusted (5)	Bonferroni (6)	Holm (7)
A1. Units of SE social effort, no sorting						
£0	£0.25	3.806	0.0003	0.0003	0.0040	0.0040
£0	£0.50	3.827	0.0003	0.0003	0.0040	0.0033
£0	£1	3.592	0.0003	0.0003	0.0040	0.0027
£0.25	£0.50	0.021	0.9553	0.9553	1.0000	0.9553
£0.25	£1	0.213	0.5843	0.8233	1.0000	1.0000
£0.50	£1	0.234	0.5503	0.8367	1.0000	1.0000
A2. Share of SE social effort, no sorting						
£0	£0.25	0.251	0.0003	0.0003	0.0040	0.0037
£0	£0.50	0.254	0.0003	0.0003	0.0040	0.0030
£0	£1	0.236	0.0003	0.0003	0.0040	0.0023
£0.25	£0.50	0.002	0.9210	0.9340	1.0000	1.0000
£0.25	£1	0.015	0.5590	0.8037	1.0000	1.0000
£0.50	£1	0.017	0.4933	0.7920	1.0000	1.0000
B1. Units of SE social effort, sorting						
£0	£0.25	3.098	0.0003	0.0003	0.0040	0.0040
£0	£0.50	2.632	0.0003	0.0003	0.0040	0.0030
£0	£1	3.032	0.0003	0.0003	0.0040	0.0023
£0.25	£0.50	0.465	0.3390	0.5513	1.0000	1.0000
£0.25	£1	0.065	0.8733	0.8873	1.0000	1.0000
£0.50	£1	0.400	0.3087	0.5563	1.0000	1.0000
B2. Share of SE social effort, sorting						
£0	£0.25	0.204	0.0003	0.0003	0.0040	0.0027
£0	£0.50	0.173	0.0003	0.0003	0.0040	0.0037
£0	£1	0.202	0.0003	0.0003	0.0040	0.0030
£0.25	£0.50	0.031	0.3400	0.5100	1.0000	1.0000
£0.25	£1	0.003	0.9210	0.9210	1.0000	0.9210
£0.50	£1	0.028	0.2833	0.5887	1.0000	1.0000

Results from pairwise comparisons of treatment groups using the multiple hypothesis testing p-value adjustments proposed by List et al. (2018), performed separately for the conditions with or without sorting. Each test considers two outcomes (social effort as units and share) and four treatments (£0, £0.25, £0.05, £1), and produces an estimate for the unadjusted p-value, the List et al. (2018) multiplicity-adjusted p-value, and p-values from the application of conservative Bonferroni and Holm corrections.

Table C.7.2: **Gender Differences in SE Effort Allocation**

	No sorting				Sorting			
	Units		Share		Units		Share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
£0.25	-4.184***	-4.296***	-0.276***	-0.284***	-3.985***	-4.539***	-0.263***	-0.300***
	(0.577)	(0.593)	(0.038)	(0.039)	(0.919)	(0.913)	(0.061)	(0.061)
£0.50	-4.596***	-4.651***	-0.306***	-0.309***	-2.418***	-2.924***	-0.161***	-0.195***
	(0.558)	(0.562)	(0.037)	(0.037)	(0.798)	(0.814)	(0.053)	(0.054)
£1	-3.988***	-3.984***	-0.264***	-0.263***	-3.711***	-4.114***	-0.247***	-0.274***
	(0.571)	(0.582)	(0.038)	(0.039)	(0.738)	(0.758)	(0.049)	(0.051)
Female	-0.365	-0.370	-0.024	-0.025	-0.337	-0.517	-0.022	-0.034
	(0.555)	(0.587)	(0.037)	(0.039)	(0.960)	(0.956)	(0.064)	(0.064)
Female × £0.25	0.764	0.814	0.049	0.053	1.527	1.816	0.101	0.119
	(0.787)	(0.806)	(0.052)	(0.054)	(1.246)	(1.211)	(0.083)	(0.081)
Female × £0.50	1.558**	1.572**	0.105**	0.106**	-0.431	-0.374	-0.025	-0.022
	(0.762)	(0.772)	(0.051)	(0.051)	(1.143)	(1.113)	(0.076)	(0.074)
Female × £1	0.780	0.741	0.054	0.051	1.230	1.176	0.082	0.078
	(0.772)	(0.787)	(0.051)	(0.052)	(1.063)	(1.054)	(0.071)	(0.070)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	708	708	708	708	341	341	341	341
R^2	0.173	0.186	0.171	0.185	0.131	0.240	0.129	0.237

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. $N = 341$. Robust standard errors in parentheses. The baseline category comprises the £0 treatment for men. Multiple hypothesis testing adjustments render all interaction coefficients statistically insignificant at conventional levels ($p > 0.1$).

Table C.7.3: **Gender Differences in Social Preferences**

	Compassion (standardized)		Social Motivation	
	(1)	(2)	(3)	(4)
£0.25	-0.334	-0.256	-0.189	-0.108
	(0.255)	(0.247)	(0.198)	(0.200)
£0.50	-0.276	-0.208	-0.134	-0.107
	(0.253)	(0.232)	(0.194)	(0.188)
£1	-0.549**	-0.423*	-0.464***	-0.383**
	(0.236)	(0.226)	(0.170)	(0.171)
Female	-0.064	-0.080	-0.035	-0.015
	(0.251)	(0.245)	(0.185)	(0.184)
Female × £0.25	0.365	0.327	0.159	0.066
	(0.329)	(0.328)	(0.252)	(0.252)
Female × £0.50	0.403	0.436	0.121	0.151
	(0.321)	(0.304)	(0.242)	(0.236)
Female × £1	0.444	0.433	0.226	0.199
	(0.301)	(0.293)	(0.217)	(0.214)
Controls	No	Yes	No	Yes
R^2	0.047	0.149	0.049	0.140

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. $N = 341$. Robust standard errors in parentheses. The baseline category is the £0 treatment for men.

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