

THE EFFECTS OF HUMAN CAPITAL INTERVENTIONS ON ENTREPRENEURIAL PERFORMANCE IN INDUSTRIALIZED COUNTRIES

Bram Hogendoorn* 

University of Amsterdam

Iryna Rud

CINOP

Wim Groot

Maastricht University

Henriëtte Maassen van den Brink

University of Amsterdam

Abstract. This paper provides a systematic review of studies on the effects of human capital interventions on entrepreneurial performance in industrialized countries. We identify 21 experimental and quasi-experimental studies published before September 2018. These studies examine the effects of business training, formal education, and entrepreneurship education. Their performance outcomes include firm profits, firm size, and entrepreneurial earnings. The main finding across these studies is that these interventions do not have statistically significant effects. Formal education is the only exception, showing positive effects on firm profits and entrepreneurial earnings, yet these effects are small in magnitude. Evidence is inconclusive regarding effect duration. These findings stand in stark contrast to correlational studies, which tend to find large positive correlations between human capital interventions and entrepreneurial performance. We therefore conclude that correlational studies tend to overestimate the benefits of human capital interventions. Moreover, our estimates show that the interventions are associated with moderately low additionality.

Keywords. Causal inference; Entrepreneurship; Firm performance; Returns to education; Systematic review

1. Introduction

In many countries, entrepreneurship is promoted and encouraged. Examples of programs include the Entrepreneurship Action Plan in the European Union (European Commission, 2013), publicly subsidized

*Corresponding author contact email: b.hogendoorn@uva.nl.

micro-enterprise programs in the United States (Schreiner, 1999), and loan schemes for start-up and scale-up companies in several industrialized countries (Young, 2012; Duruflé *et al.*, 2017). These programs aim at creating new jobs (Fölster, 2000; Thurik *et al.*, 2008), reducing welfare dependence (Michaelides and Benus, 2012), and helping to diffuse innovation (Shane and Venkataraman, 2000; Bhidé, 2005) through entrepreneurship.

Human capital investments form an integral part of programs targeted at entrepreneurs. By investing in human capital, entrepreneurs may be better able to identify business opportunities (DeTienne and Chandler, 2004), apply innovative practices (McGuirk *et al.*, 2015), deal with uncertainty and regulatory issues (Henry *et al.*, 2005), and adapt to stakeholders' demands (Gibb, 1997). Accordingly, it is commonly believed that human capital investment enhances entrepreneurial outcomes.

Despite increased scientific attention to the effects of human capital investment on entrepreneurial performance, existing evidence is inconclusive. This is because the relationship between human capital and entrepreneurship is not straightforward. Personal characteristics and other factors, which are often unobservable, can affect both the decision to invest in human capital and entrepreneurial performance (Shane, 2006). Failing to take these into account may bias estimations, and hence flaw policy recommendations. Causal studies that address unobserved heterogeneity in the relationship between human capital investments and entrepreneurial performance are therefore of great importance.

In this paper, we systematically review studies on the causal effects of human capital interventions on entrepreneurial performance. The population of interest are self-employed workers in industrialized countries. We focus on industrialized countries, since comprehensive overviews of causal evidence from these countries are still absent. Only studies that use an experimental or quasi-experimental design are included. The specific objectives are to (a) identify the strengths and weaknesses of these studies, (b) compare them in terms of characteristics and findings, (c) summarize what interventions do and do not work, (d) examine heterogeneity in intervention effects, and (e) assess the extent to which interventions realize additional human capital, over and above what would have occurred in the absence of these interventions.

Three main conclusions are drawn from this review. First, very few studies use an experimental or quasi-experimental design to estimate the effects of human capital interventions on entrepreneurial performance. Second, the few studies that do, find small positive effects, or find that the effects are not statistically significant. Formal education appears to be the only intervention to have a positive effect on firm profits and entrepreneurial earnings. Third, interventions do not result in much human capital gains.

The outline of this paper is as follows. Section 2 gives an overview of previous reviews and meta-analyses on the relationship between human capital and entrepreneurial performance. Section 3 provides a theoretical discussion on mechanisms underlying this relationship. Section 4 sets out the definitions, inclusion criteria, and search strategy. Section 5 describes the studies included for review in terms of their characteristics, as well as their substantive findings. The paper concludes with a discussion of the results, its limitations, and suggestions for future research.

2. Previous Reviews

Empirical studies generally find a positive relationship between human capital and entrepreneurial performance. Van der Sluis *et al.* (2008) review studies on formal education and various measures of entrepreneurial performance, including earnings, profits, and employment growth.¹ They show that two-thirds of these studies find positive and statistically significant correlations between formal education and entrepreneurial performance, and that the correlations tend to be moderately large, especially for earnings outcomes. Martin *et al.* (2013) look at entrepreneurship training and at entrepreneurship education as taught in college or university. They show that training has a small positive correlation with entrepreneurial earnings and profits, whereas academic entrepreneurship education has a larger positive correlation with

these outcomes. The authors theorize that this larger correlation is due to the context in which business decisions are made, which requires broad conceptual and theoretical knowledge as provided by academic education. Unger *et al.* (2011) take a broader view on human capital investment, ranging from management experience and business education to cognitive skills and formal education. Their outcomes include firm profit, sales, and size. The authors find a small positive correlation between human capital investment and entrepreneurial performance, which is somewhat larger when the human capital is more task-related. They argue that this is because higher task-related human capital can be more effectively utilized to exploit business opportunities and to facilitate further learning.

Although previous reviews point to a positive *relationship*, they also indicate that it is unclear whether human capital *causes* entrepreneurial performance. Human capital may be endogenous to entrepreneurial performance, if both are related to favorable personal traits or if those who will become entrepreneurs choose their education accordingly (Shane, 2006). Previous analyses suggest that education and entrepreneurial outcomes are indeed endogenous, and that this endogeneity may bias the estimated returns to education (Block *et al.*, 2012). To address this problem, Van der Sluis *et al.* (2008) examine four studies from industrialized countries that use instrumental variables (IVs) for education.² These quasi-experimental studies find significantly higher returns to education than correlational studies do. In contrast, two meta-analyses of experimental and quasi-experimental studies on entrepreneurship programs in developing countries come to much more cautious conclusions. Cho and Honorati (2014) find that most interventions have either no statistically significant effects or small effects on income or firm performance. Business training and vocational training have somewhat larger effects than financial training, and are moderately large only when combined with counseling. Similarly, Grimm and Paffhausen (2015) find that business training has small to moderate positive effects on small firm job creation. They further find that randomized controlled trials (RCTs) are generally less likely than quasi-experimental designs to detect statistically significant effects, and suggest that this is due to their smaller sample sizes. Martin *et al.* (2013), however, show that studies that use random assignment find smaller effects even when they have large sample sizes. Together, these findings suggest that correlational studies overestimate the effects of human capital on entrepreneurial performance, at least in developing countries.

Previous reviews also point to another potential problem with establishing the causal effects of human capital on entrepreneurial performance, namely, self-selection into entrepreneurship. Personal characteristics might correlate both with the decision to become an entrepreneur and with entrepreneurial performance. Schooling as one such characteristics has been examined most widely. In their meta-analysis, Van der Sluis *et al.* (2008) show that schooling is unrelated to selection into entrepreneurship. In addition, correlations between schooling and entrepreneurial performance are similar for studies that account for potential selection on schooling and studies that do not. Furthermore, among studies that use IVs for education, accounting for potential selection does not alter the direction of the (positive) effect of education on entrepreneurial performance (Van der Sluis *et al.*, 2007; Tokila and Tervo, 2011; Fossen and Büttner, 2013; Van Praag *et al.*, 2013). However, little is known about selection on characteristics other than schooling, although evidence on, for instance, noncognitive skills and personality traits in entrepreneurship is growing (e.g., Baron, 2000; Baron and Markman, 2003; Zhao and Seibert, 2006; Hartog *et al.*, 2010). It is important to keep this in mind when considering our results.

Besides the calls for more rigorous study designs, previous reviews provide several recommendations for future research.³ The main recommendation is to further investigate heterogeneity. Human capital interventions are likely to play out differently by type of intervention, target group, and outcome of interest. Another recommendation is to investigate long-term effects. It can be argued that some interventions require more time to take effect, because the effects accumulate over time or because the intervention itself is time-consuming for the entrepreneur. Furthermore, efficiency considerations deserve more attention. It remains unclear whether an intervention crowds out human capital investments that would have been realized in the absence of the intervention.

We take these recommendations into account, and add another feature that distinguishes this paper from previous reviews. Previous reviews of experimental and quasi-experimental studies have focused on developing countries only. Compared to industrialized countries, developing countries exhibit much higher rates of self-employment, of which the lion's share is informal and under poor working conditions. They also have higher rates of in-work poverty, lower levels of educational attainment in general and among own-account workers in particular, and lower growth perspectives for small firms (Nichter and Goldmark, 2009; Fields, 2011; Barro and Lee, 2013). Therefore, findings from developing countries may not be generalizable to industrialized countries. We fill this gap by looking at the causal effects of human capital interventions on entrepreneurial performance in industrialized countries.

3. Theoretical Considerations

Human capital theory predicts that investments in schooling and training increase labor productivity (Becker, 1964; Mincer, 1974). Among wage employees, empirical evidence for human capital theory is substantial. A meta-analysis of correlational and quasi-experimental studies shows that the returns to formal education are large at about 6.8% per year (Ashenfelter *et al.*, 1999). The returns to on-the-job training are around 2.6% per course, which translates to 30% if these trainings had been of the same duration as a year of formal education (Haelermans and Borghans, 2012). There are no compelling reasons to expect different processes at work among entrepreneurs. Hence, we expect that the average effect of human capital interventions on entrepreneurial performance is positive.

Effects may be heterogeneous by type of human capital. In his work on wage employees, Becker (1964) distinguishes between general and firm-specific human capital. General human capital enhances labor productivity within the firm that provides it as well as outside. Firm-specific human capital is useful only in the firm that provides it. Adjusting this distinction, it has been proposed to focus on task-specificity instead of firm-specificity, since the nature of performed tasks matters more than the firms in which they are performed (Gibbons and Waldman, 2004). Task-specific human capital is most fruitfully employed in jobs that require the performance of the specific tasks to which this human capital pertains. One consequence is that firms should allocate new workers to tasks that best match their skills (Prescott and Visscher, 1980). Yet, another consequence is that task-specific training should be tailored to the work at hand if it is to enhance performance. This idea easily extends to entrepreneurs. Hence, we expect that general human capital interventions always enhance entrepreneurial performance, whereas specific human capital interventions enhance entrepreneurial performance only if they are well tailored to the entrepreneur's work.

Another source of effect heterogeneity may be the human capital stock prior to additional investment. The technology of skill formation theory shows that the human capital production function is characterized by complementarities within and between skills (Cunha and Heckman, 2007; Aizer and Cunha, 2012). A higher skill stock in one period raises the skills stock in the next period, and skills gained during one stage raise the returns to skills investment during subsequent stages. While this theory builds mainly on evidence for children and adolescents, we believe that it extends to entrepreneurs as well. Human capital interventions may yield higher returns among entrepreneurs with higher human capital stock prior to the intervention. To measure this, we consider employment status prior to intervention as an (imperfect) proxy for human capital stock. Hence, we expect smaller effects of human capital interventions on entrepreneurial performance when they target previously unemployed individuals.

A final source of effect heterogeneity may be business size. While we stress that the current review examines entrepreneurs only, we were sometimes forced to include studies of both solo entrepreneurs and small businesses with employees when these studies did not separate the effect estimations (see Section 4). Solo entrepreneurs do not face the organizational constraints of businesses with employees in deciding how to employ their human capital assets most productively. This applies not only to constraints such as

job prescriptions and fixed procedures, but also to predetermined wage brackets (Douhan and Van Praag, 2009). Consequently, entrepreneurs should have higher returns to human capital than employees. This indeed appears to be the case, and the difference disappears after accounting for individual control over the working environment (Van Praag *et al.*, 2013). Hence, we expect larger effects of human capital interventions on entrepreneurial performance in studies that comprise only entrepreneurs than in studies that comprise both entrepreneurs and small businesses with employees.

4. Method

4.1 *Entrepreneurship Definition*

One of the main problems hampering entrepreneurship research is the lack of an agreed-upon definition of entrepreneurship (Shane and Venkataraman, 2000). Entrepreneurship has been defined in terms of business creation, business ownership, decision-making autonomy, risk bearing, risk seeking, industrial relations, innovative production, opportunity awareness, and skill set diversity, among other things (Gartner, 1988; Hébert and Link, 1989; Barrett and Rainnie, 2002; Lazear, 2004). The resulting measurements of these definitions are often at odds with one another. Self-employment may be a good proxy for entrepreneurs as business owners or as autonomous decision-makers, but not for entrepreneurs as individuals who learn fast and engage in nonnormative behavior (Levine and Rubinstein, 2017). As a result, policy implications of entrepreneurship research depend heavily on the definition used (Henrekson and Sanandaji, 2014). Notwithstanding this debate, in the current review, we define entrepreneurs as self-employed workers without employees. The rationale is threefold. First of all, this definition is widely used in entrepreneurship research (Van der Sluis *et al.*, 2008). Second, self-employment is easily observed, whereas more complex definitions based on behavioral or personality traits are infeasible since these traits usually go unobserved. Third, policies aimed at enhancing entrepreneurship often target the self-employed.

4.2 *Inclusion Criteria*

In order to be eligible for inclusion, studies should (a) evaluate the effects of human capital interventions, (b) analyze self-employed workers without employees, (c) focus on firm profitability, turnover or sales, firm size, or entrepreneurial earnings as outcomes, (d) use an experimental or quasi-experimental design, (e) take place in an industrialized country, and (f) be published in an English peer-reviewed journal before September 2018.

Some of these criteria require further explanation. Regarding (a), human capital interventions are broadly defined to include business-related education or training, coaching, entrepreneurship programs, formal education, and programs that foster informal or nonformal learning. Section 5.1 shows that human capital interventions are sometimes combined with other interventions such as loans. Studies that do not distinguish between these interventions cannot attribute the total intervention effect entirely to the human capital intervention, but are included nonetheless, as they do focus on the interventions and outcomes of interest. Regarding (b), we decided to also include studies that estimate effects among samples of both self-employed workers without employees and small firms with employees, because some studies do not separately report the effects for these two groups. Regarding (c), we do not consider survival rates and entrepreneurial skills, two outcomes that are often believed to indicate entrepreneurial performance. Survival is an ambiguous measure of performance. For some individuals, it truly indicates success, but for others, it is rather business closure that indicates success, through profitable business sale, exit into better remunerated salaried employment or retirement (Headd, 2003). Entrepreneurial skills are not immediately indicative of performance either, since they mediate between human capital interventions and ultimate entrepreneurial performance. Because we are interested in performance, we do not consider skills as such.

Regarding (d), the golden standard for measuring causal effects is the RCT. In an RCT, participants are randomly assigned to either receiving the human capital intervention or not. Under full compliance, this ensures comparability between the intervention recipients and the control group, as both groups should not differ systematically on any observed or unobserved characteristic. Since it is often infeasible to conduct such human capital experiments, we also include quasi-experimental studies. Quasi-experimental studies rely on a regression discontinuity, IVs, or difference-in-differences design (Imbens and Wooldridge, 2009). Instead of an experimenter they utilize another exogenous variable to randomly “assign” individuals into a treatment or control group, thus creating a quasi-experiment.⁴ Twin studies are eligible for the same reason, as the comparability of identical twins makes it plausible that differences in entrepreneurial performance between the twins are caused by differences in human capital investment (e.g., Ashenfelter and Krueger, 1994). Finally, we include studies relying on matching methods. Unlike other quasi-experimental designs, matching does not randomly assign individuals into a treatment or control group. However, it tries to achieve comparability of treatment and control groups by balancing them on observable characteristics (Rosenbaum and Rubin, 1983).

Regarding (e), this review is restricted to studies conducted in European Union (EU) and Organization for Economic Cooperation and Development (OECD) countries.⁵ Although we acknowledge that this group of countries is rather diverse, we believe that it captures the industrialized countries in the absence of a better definition.

4.3 Search Strategy

We systematically searched the electronic databases Academic Search Premier, Business Source Premier, EconLit, ERIC, RePEc, Sociological Abstracts, and Web of Science SSCI for publications that meet our inclusion criteria. Searches were performed combining intervention terms, subject terms, and outcome terms. Intervention terms were “coaching,” “education,” “informal learning,” “schooling,” “training,” and their morphological variations such as “coached,” “educated,” “educating,” etc. Subject terms were “entrepreneurship,” “microenterprise,” “own-account,” “self-employment,” “small business,” “small enterprise,” “small firm,” “small venture,” “start-up,” and variations. Outcome terms are “earnings,” “firm growth,” “income,” “job creation,” “performance,” “profits,” “returns,” “revenue,” “sales,” “success,” “turnover,” and variations. We also added the exclusion terms “developing countries,” “in-company,” and “on-the-job,” to limit the search to industrialized countries and nonemployees. Besides the database search, we looked for references in previous reviews. Lastly, we looked for references in the batch of included articles by applying backward citation searching.

The database search resulted in a total of 6864 hits (Figure 1). Browsing titles and abstracts, we selected 178 articles for further inspection. Another 20 articles were identified from the reference lists of other systematic reviews. Removing duplicates, this left us with 146 articles to be screened. We excluded studies that turned out to be correlational in nature (72 articles), had a different outcome of interest (26), applied a different intervention (14), were not empirical (13), or took place in a developing country (2), resulting in 19 articles. Through backward citation searching, two additional articles were identified. We accordingly included 21 articles in this review, 6 of which are experimental and 15 quasi-experimental.⁶

For these 21 studies, we report the authors, year of publication, country, treatment and control groups, intervention, duration, follow-up period, outcomes, study design, sample size, and effect sign, magnitude, and statistical significance ($p < 0.05$). When the statistical significance is not reported but can be calculated, we do this by hand. When multiple model specifications are used, the ones deemed least biased are reported.

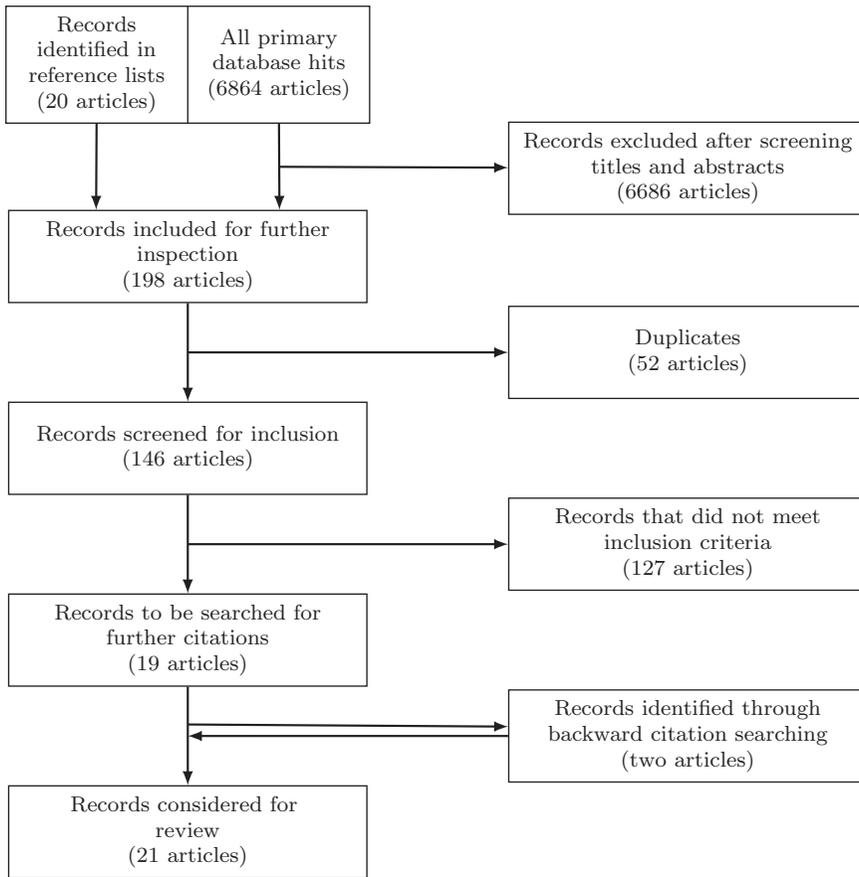


Figure 1. Search and Selection of Studies.

Two sets of effect estimates are reported. The first sets of estimates are the crude estimates as originally reported in the studies under review. These estimates b give the mean difference in entrepreneurial performance between treatment and control group. Since they are not comparable across studies, we convert them into a second set of standardized estimates. The conversion is initially conducted using Cohen's d estimator (1969), which is widely used in psychology and education studies and more recently also in economics (e.g., Cho and Honorati, 2014; Vooren *et al.*, 2019). Cohen's d gives the mean difference in entrepreneurial performance between treatment and control groups, expressed in (pooled) standard deviations rather than raw scores. As noted by Hedges (1981), this estimator is biased in small samples, which are abundant in this review. We therefore provide the bias-corrected estimator Hedges' g instead.⁷ Note that g (or d) is a measure of the effect magnitude rather than its statistical significance. Magnitude and significance do not necessarily overlap. A high g -value that is insignificant would indicate that the intervention has a large effect in the sample, but does not generalize to the population. Conversely, a low g -value that is significant would indicate that an intervention has a small effect only, even if this small effect exists in the population.

5. Results

5.1 Descriptive Results

Studies are very diverse in terms of target populations, interventions, and outcomes (Table 1). There appear to be three categories of target populations. The first category consists of owners of micro-, small-, and medium-sized enterprises (MSMEs) who are already in business ($n = 15$ articles). These studies should have self-employed workers in their sample in order to be included, but in some cases ($n = 2$), no minimum number of employees is reported. The second category consists of individuals who are either unemployed or underemployed ($n = 5$). These individuals are guided toward business assistance in order to successfully insert them into self-employment. In all of the studies in this category, the target group includes entrepreneurs, which sometimes ($n = 1$) contradicts initial intervention conditions. The third category consists of secondary school students who are prepared for entrepreneurship at a young age ($n = 1$).

Concerning interventions, again three types can be distinguished. There are studies evaluating business training and advice ($n = 12$), formal education ($n = 8$), and entrepreneurship education ($n = 1$). Business training and advice studies comprise various interventions, including group training in business practices, management skills training and individual business coaching. Sometimes, the human capital interventions ($n = 7$) are combined with nonhuman capital interventions, such as credit loans, start-up capital, or unemployment benefits.

Concerning outcomes, more than half of all studies ($n = 12$) analyze several. The most commonly analyzed outcomes are earnings or income ($n = 14$) and firm profits or sales ($n = 9$). A few studies ($n = 4$) also measure firm size in terms of newly created jobs. It sometimes remains unclear whether earnings or income refer to quantities at the personal or household level, whether they are derived merely from the enterprise or also from other sources, and whether they are net or gross of taxes and transfers.

The intensity and duration are important elements of the interventions. We observe a large variation among the studies that report on them, ranging from a single intervention of two to three hours (Coad *et al.*, 2016) to a one-year intervention of four hours per week (Bruhn *et al.*, 2018). Few authors explicitly report the intensity of the intervention ($n = 7$) or the time span during which the intervention took place ($n = 4$). This high prevalence of nonreporting prevents us from meaningfully relating intensity or duration to effectiveness.

Studies differ greatly with respect to methodology. First, follow-up periods range from 1 to 16 years after the intervention. Some studies have multiple follow-ups ($n = 8$). This is a positive finding, given that researchers and policy makers will be interested in whether effects wane or persist over time. Second, sample sizes differ enormously across studies, ranging from 176 to 211,754 individuals. This has important consequences for the probability of detecting statistically significant effects, which should be borne in mind when interpreting the results. Finally, when it comes to completeness and transparency, we believe that there is room for improvement. Some studies report on complex model estimates but leave out descriptive statistics ($n = 4$). In a similar vein, many studies ($n = 8$) do not make a minimal attempt to confront their findings with previous theoretical or empirical knowledge. This makes it difficult to assess whether their findings corroborate or contradict previous findings in the field, as well as to understand the reasons for (non-)congruence.

The quality of the designs also varies. Most studies rely on IVs ($n = 8$), followed by matching ($n = 6$), RCTs ($n = 5$), and a regression discontinuity design ($n = 1$). None of the studies included in this review uses a twin design. IV-studies are conducted using the two-stage least squares estimator ($n = 7$) or the Hausman–Taylor estimator for panel data with endogenous time-invariant covariates ($n = 1$). They instrument educational attainment with father's education (Fossen and Büttner, 2013; Iversen *et al.*, 2016), with both parents' education (Williams, 2003; Tokila and Haapanen, 2012), with parental and spousal education (Iversen *et al.*, 2011), with parental education and number of siblings (Parker and Van Praag,

Table 1. Description of included records.

Publication	Country	Target Group	Intervention	Duration/ Intensity	Follow-Up Period	Design	Sample Size	Crude Effect <i>b</i>	Hedges' <i>g</i>
Brunh <i>et al.</i> (2018)	Mexico	Business owners of nonagricultural micro-, small-, and medium-sized enterprises in Puebla	Subsidized access to consulting and mentoring services	4 hours per week during one year	Five years	Stratified RCT and DD	T1: 150 C1: 282 T2: 89 C2: 164 T3: 89 C3: 164	Size: +5.76 Profits: +5.80 Sales: -5.11	+0.54 +0.12 -0.04
Coad <i>et al.</i> (2016)	United Kingdom	Business owners in England or Wales who recently started a business and opened a business account at a large UK bank	Start-up training provided free of charge	2 to 3 hours	5 years	PSM	TC1: 3,338 TC2: 1,905 TC3: 1,554 TC4: 982 TC5: 44	Sales: +0.21	+0.23
Elerf <i>et al.</i> (2015)	Sweden	High school students aged 17–19	Entrepreneurship education (Junior Achievement Company Program)	×	11–16 years (dep. on cohort)	PSM	T: 9,731 C: 202,023	Income: +0.10	+ 0.09
Fairlie <i>et al.</i> (2015) ★	United States	Individuals interested in starting or growing a business in Philadelphia, Pittsburgh, Minneapolis-St.Paul, rural Minnesota, rural Maine	Training courses and business counseling, only business counseling, or only training courses (Project Growing America Through Entrepreneurship)	×	5 years	RCT	T1: 1,753 C1: 1,691 T2: 1,563 C2: 1,457 T3: 1,278 C3: 1,176	Size: -0.09 Income: +0.09 Sales: -2.55	-0.07 +0.03 -0.05
Fajnzylber <i>et al.</i> (2009)	Mexico	Self-employed individuals working in microfirms (National Survey of Microfirms)	Formal credit loan, informal credit loan, training, tax payments, union membership, or business associate	×	1 year	PSM	4,480	Income: -0.12	-0.04
Fossen and Büttner (2013)	Germany	Self-employed or salaried individuals aged 19–65 (German Socio-Economic Panel)	Formal education	n/a	13-year long panel	IV	68,004	Earnings: +0.07 (average over entrepreneurs)	×

(Continued)

Table 1. Continued.

Publication	Country	Target Group	Intervention	Duration/ Intensity	Follow-Up Period	Design	Sample Size	Crude Effect b	Hedges' g
García-Maímar and Montuenga-Gómez (2005)	Portugal, Spain	Self-employed or salaried individuals	Formal education (European Community Household Panel)	n/a	7-year long panel	Hausman-Taylor IV	Portugal: 7,181 Spain: 6,652	Earnings: +0.40 (average over countries)	+0.00
Georgiadis and Pielis (2016)	United Kingdom	Business owners and employees of small- and medium-sized enterprises	General, managerial, or human resources skills training.	8-20 hours in 8-10 weeks	2 years	Stratified RCT	430	Profits: +0.10 Sales: +0.18 (average over training types)	+0.12 +0.14
Gonzalez-Uribe and Leatherbee (2018)	Chile	Business owners of microenterprises aged 19–84 who participate in a start-up accelerator and applied to entrepreneurship schooling	Entrepreneurship education, advertisement on the accelerator's web page, mentoring and network events	6 months	2.75 to 4.75 years	Fuzzy RD	T: 59 C: 217	Profits: +0.04	+0.12
Iversen <i>et al.</i> (2011)•	Denmark	Self-employed individuals aged 33–42, resident in Denmark since 1980, active in the nonagricultural private sector (Integrated Database for Labor Market Research)	Formal education	n/a	22-year long panel	IV	49,598	Profits: +0.04	+0.05
Iversen <i>et al.</i> (2016)•	Denmark	Self-employed individuals aged 33 to 42, resident in Denmark since 1980, active in the non-agricultural sector (Integrated Database for Labor Market Research)	Formal education	n/a	22-year long panel	IV	26,116	Profits: -0.01 (at interaction set to mean)	×

(Continued)

Table 1. Continued.

Publication	Country	Target Group	Intervention	Duration/ Intensity	Follow-Up Period	Design	Sample Size	Crude Effect b	Hedges' g
Martínez <i>et al.</i> (2018)	Chile	Individuals aged 18 and above who are either unemployed or underemployed and claim poverty benefits	Business classes and in-kind capital transfers	60 hours during one month	3 years	Stratified RCT	T1: 689 C1: 566 TC2: ×	Size: +0.02 Income: +6.59 Profits: -7.60 Sales: -0.00	×
Meager <i>et al.</i> (2003)	United Kingdom	Treatment: Individuals aged 18–30 interested in starting a business and receiving support from The Prince's Trust. Control: individuals from a survey among randomly selected individuals aged 18–30 from a 5% random sample of all unemployment benefits claimants.	Start-up loan, start-up grant, test-marketing grant, and/or business advice	×	3.5 years	Exact matching	T1: 2002 C1: 1600 T2: 1332 C2: 1600 T3: 872 C3: 925	Earnings: -1.19 (average over weekly/monthly)	-0.16
Michaélides and Benus (2012) ★	United States	Individuals interested in starting or growing a business in Philadelphia, Pittsburgh, Minneapolis-St.Paul, rural Minnesota, rural Maine	Training courses and business counseling, only business counseling, or only training courses (Growing America Through Entrepreneurship)	On average 1.2 hours for individual assessment, 10.5 hours for various training courses, 1.3 hours for business counseling	5 years	RCT	T1: 1759 C1: 1690 T2: 1549 C2: 1489 T3: 1274 C3: 1176	Earnings: +337	+0.21

(Continued)

Table 1. Continued.

Publication	Country	Target Group	Intervention	Duration/ Intensity	Follow-Up Period	Design	Sample Size	Crude Effect <i>b</i>	Hedges' <i>g</i>
Parker and Van Praag (2006)	Netherlands	Business owners of enterprises with and without employee aged 14–60	Formal education	n/a	n/a	IV	370	Profits: +0.14	+0.21
Rodríguez- Planas (2010) ◀	Romania	Individuals aged 25–55 registered at the local employment office, with an income below half the minimum wage, and employed for at least six months in the past year or recently graduated	Self-employment skills assessment, advice, capital loans and entrepreneurial training	12 months	3 years	PSM	T: 350 C: 961	Earnings: +37.58	+0.10
Rodríguez- Planas and Benus (2010) ◀	Romania	Individuals aged 25–55 registered at the local employment office, with an income below half the minimum wage, and employed for at least six months in the past year or recently graduated	Small business skills assessment, advice, capital loans and entrepreneurial training	12 months	3 years	PSM	T: 350 C: 961	Earnings: +37.58	+0.10
Schreiner (1999)	United States	Individuals aged 18 and above claiming unemployment benefits in Washington or Massachusetts, except interstate or standby claimants	Business classes, business counseling, benefits without work search and contingent lump-sum payment (Unemployment Insurance Self-Employment Demonstration)	Washing- ton: 4–7 classes in 1–12 weeks, 1.5–7.5 hours counsel- ing, 10–24 weeks benefits	31–33 months	RCT	T: 1369 C: 1360	Earnings: +1427	×

(Continued)

Table 1. Continued.

Publication	Country	Target Group	Intervention	Duration/ Intensity	Follow-Up Period	Design	Sample Size	Crude Effect b	Hedges' g
Tokila and Tervo (2011)	Finland	Individuals aged 18 to 64 with a self-employment pension insurance and positive non-agricultural entrepreneurial incomes exceeding a threshold, out of a 7% random sample from the statistics registry	Formal education	n/a	n/a	IV	11,359	Income: +0.68 (average over years/dummies)	+0.14
Van Praag et al. (2013)	United States	Self-employed individuals active in the non-agricultural sector, with or without employees, aged 14 to 22 at the start, who are not in education and who work at least 300h per year in their business (National Longitudinal Survey of Youth)	Formal education	n/a	22-year long panel	IV	1052	Earnings: +0.21	+0.24
Williams (2003)	West Germany	Self-employed individuals aged 25 to 60 (German Socio-Economic Panel)	Formal education	n/a	14-year long panel	IV	176	Income: +0.11	+0.20

★ ◀ ◀ indicate usage of the same data sets.
 DD difference-in-differences, IV instrumental variable, PSM propensity score matching, RCT randomized controlled trial, RD regression discontinuity design.
 T treatment group, C control group, numbers indicate moment of follow-up.
 The crude effects b and Hedges' g give the effect magnitude at latest follow-up.
 Age reported for moment of intervention.
 × not reported, n/a not applicable.

2006), or with magazine subscription, library membership, presence of a stepparent, and number of siblings in the household at age fourteen (Van Praag *et al.*, 2013). The use of parental education has been criticized in the literature because it might be an invalid instrument (Trostel *et al.*, 2002). However, it appears that bias occurs not so much due to invalid instrumentation as such (Hoogerheide *et al.*, 2012), but rather due to a combination of invalid and weak instrumentation (Bound *et al.*, 1995). Simulations moreover show that the direction of this bias cannot be determined *a priori* (Crown *et al.*, 2011). This is problematic particularly for the studies of Tokila and Tervo (2012) and Williams (2003), who do not assess the strength of their instruments. Their findings must therefore be viewed with caution. A better instrument is found in the study applying a fuzzy regression discontinuity design (Gonzalez-Urbe and Leatherbee, 2018). In this study, entrepreneurs pitch before a jury the need for business training. The jury assigns each pitch a score and only grants entrepreneurs who score above a certain threshold access to business training. Using the discontinuity around the threshold score as an instrument of business training receipt, the authors are able to relate exogenous variation in business training to entrepreneurial performance. A final interesting finding of IV-studies is that years of education appear to be successfully instrumented as a continuous variable, but not as a set of dummy variables (Iversen *et al.*, 2011). This may be relevant for researchers who wish to instrument educational attainment in future research.

Studies relying on matching techniques vary in quality too. In one study, matching is conducted on three covariates only, in others, propensity score model specifications are not shown ($n = 2$), and in yet others, they are shown but not motivated ($n = 3$). Sometimes, the matching successfully reduces differences between the treatment and control groups (Rodríguez-Planas and Benus, 2010; Elert *et al.*, 2015), sometimes moderate differences remain (Meager *et al.*, 2003; Fajnzylber *et al.*, 2009), and sometimes the imbalance reduction is not shown, impeding assessment of the quality (Rodríguez-Planas, 2010; Coad *et al.*, 2016). One reason for the unsuccessful balancing of treatment and control groups may lie in the way that propensity scores are modeled. All studies specify them as zero-order linear probit models, while simulations show that this often results in underfitting. Specifying these models with the inclusion of higher order terms and interactions is more effective in reducing imbalance bias, even if this results in overfitting (Millimet and Tchernis, 2009). In addition to observed differences between treatment and control groups, unobserved differences must also be balanced to identify the causal effects of human capital interventions. Although matching cannot correct for unobservables, it can try to capture some of them by including appropriate proxies. It is encouraging that all matching studies indeed include general labor market experience, business experience, or high school grades as covariates, thereby partially correcting for (unobserved) ability differences.

RCT studies appear to be more robust than quasi-experimental studies, although they too vary in quality. Three of them are designed as classical RCTs (Schreiner, 1999; Michaelides and Benus, 2012; Fairlie *et al.*, 2015). The others are designed as stratified RCTs with random assignment within predefined strata, namely, sector (Bruhn *et al.*, 2018), region (Georgiadis and Pitelis, 2016), or region and socioeconomic status (Martínez *et al.*, 2018). In contrast to matching, the randomized assignment ensures comparability of treatment and control groups on both observed and unobserved characteristics. However, noncompliance with assigned treatment status may put this in jeopardy. Only one RCT accounts for this by instrumenting training receipt with treatment assignment status (Bruhn *et al.*, 2018). Another note on the RCT studies concerns sample size. Concurring with Grimm and Paffhausen (2015), we find that RCTs dispose of smaller samples than quasi-experiments. Somewhat related is the combination of data sources. One RCT study combines its survey data with individual-level administrative data to lengthen the window of observation, without problems of response bias and selective attrition that are typical of survey data (Martínez *et al.*, 2018). This may be a fruitful strategy even when authorities only grant anonymized data that do not allow individual identification on the treatment and control group levels. In such cases, a difference-in-differences design can still identify the long-term effects of the human capital intervention on entrepreneurial performance (Bruhn *et al.*, 2018).

A final remark regards the data sets used by these studies. Three pairs of studies make use of equivalent or similar data sets. Fairlie *et al.* (2015) use data from the same entrepreneurship program as Michaelides and Benus (2012). Iversen *et al.* (2016) use a (more restricted) selection of the data set used by Iversen *et al.* (2011). Rodríguez-Planas (2010) is based on the exact same data set and same model specifications as Rodríguez-Planas and Benus (2010). For completeness, and because we do not perform a statistical meta-analysis, all publications are included in this review.

5.2 Substantive Results

Table 2 presents the main findings from the studies under review. It shows whether the intervention has a significant effect ($p < 0.05$) on the outcome, and if so, whether the effect is positive or negative. To maintain overview, the interventions and outcomes are grouped in the categories set out in Section 5.1. Some studies report effects on several outcomes in the same category, such as individual and household income, but the effects are always of the same sign and significance level across those outcomes. Effects are reported at latest follow-up for studies that have multiple measurement moments.

The first and foremost finding is that most studies find no statistically significant effects. Only a minority of the human capital interventions ($n = 8$) appear to have a statistically significant effect on at least one measure of entrepreneurial performance. At the very least, it is unlikely that these interventions do harm, as all of them are positive in sign. It is important to note that statistically significant effects are found even in large sample sizes, and are not the mere consequence of limited statistical power.

When significant effects are found, they are small in magnitude. Although for a number of studies ($n = 5$) we are not able to calculate Hedges g , the numbers for the remaining studies are illustrative. Figure 2 shows the distribution of g across all interventions and outcomes. The average effect is small in magnitude ($\bar{g} = 0.10$), even when split between negative ($\bar{g} = -0.05$) and positive ($\bar{g} = 0.16$) effects. The only study to find a moderately large effect ($g = 0.54$) is conducted by Bruhn *et al.* (2018). They find that five years after intervention, Mexican MSME owners who received subsidized business consultation hire more employees than owners in the control group. A possible explanation is that the intervention is very well tailored to the demands of the entrepreneurs. It starts with a daylong consultation in which entrepreneurs diagnose enterprise problems with their individual mentor, to jointly decide on the focus and scope of the following trajectory, consisting in personalized consulting services for 4 hours per week during an entire year. This degree of tailoring is not found in the other interventions.

Zooming in on the specific interventions-outcome combinations, we find the following. There is one publication about the effect of early *entrepreneurship education on entrepreneurial income* (Elert *et al.*, 2015). The authors match Swedish high school students aged 17–19 years who voluntarily participated in an entrepreneurship program, to their counterparts who did not participate. The matching is conducted using a plethora of covariates. However, because participation was voluntary, unobservable ability and motivation differences may persist. The authors try to partially correct for this, by including preprogram high school grades in the matching covariates as a proxy for ability. After matching, they find that the program does not significantly affect entrepreneurial income.

Business training interventions provide mixed results. Regarding the effects of *business training on entrepreneurial income* ($n = 8$), none of the studies find a statistically significant effect. Regarding the effects of *business training on firm profits* ($n = 5$), the majority of studies find no statistically significant effects either. The only study to find a positive effect of business training on firm profits and sales is Martínez *et al.* (2018). A reason for this exception may lie in the nature of the intervention, which includes not only business classes but also a relatively large asset transfer. Regarding the effects of *business training on firm size* ($n = 4$), lastly, again the majority of studies find no statistically significant effects. Only Bruhn *et al.* (2018) find that the owners of MSMEs who participate in training see that their firms grow in terms of employment. This exception is little surprising, as most of the individuals

Table 2. Interventions and outcomes

Authors/Year	Intervention	Entrepreneurial Earnings/Income	Firm Profits/Sales	Firm Size
Elert <i>et al.</i> (2015)	Entrepreneurship education	o		
Fajnzylber <i>et al.</i> (2009)	Business training/advice	o		
Meager <i>et al.</i> (2003)	Business training/advice	o		
Michaelides and Benus (2012) ★	Business training/advice	o		
Rodríguez-Planas (2010) ◀	Business training/advice	o		
Rodríguez-Planas and Benus (2010) ◀	Business training/advice	o		
Schreiner (1999)	Business training/advice	o		
Fairlie <i>et al.</i> (2015) ★	Business training/advice	o	o	o
Martínez <i>et al.</i> (2018)	Business training/advice	o	+	o
Coad <i>et al.</i> (2016)	Business training/advice	o	o	
Georgiadis and Pitelis (2016)	Business training/advice	o	o	
Bruhn <i>et al.</i> (2018)	Business training/advice	o	o	+
Gonzalez-Uribe and Leatherbee (2018)	Business training/advice	o	o	o
Fossen and Büttner (2013)	Formal education	+		
García-Mainar and Montuenga-Gómez (2005)	Formal education	+		
Tokila and Tervo (2011)	Formal education	+		
Van Praag <i>et al.</i> (2013)	Formal education	+		
Williams (2003)	Formal education	o		
Iversen <i>et al.</i> (2011) ●	Formal education		+	
Iversen <i>et al.</i> (2016) ●	Formal education		+	
Parker and Van Praag (2006)	Formal education		+	

★ ◀ ● indicate usage of the same data sets.

+ positive effect ($p < .05$), o not statistically significant.

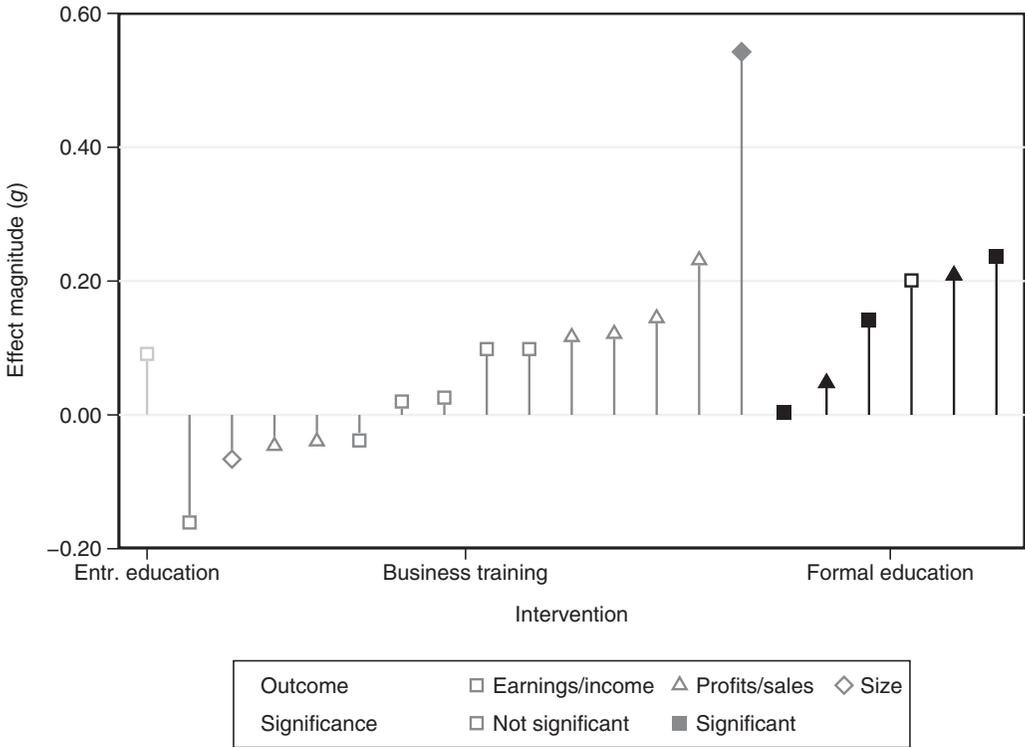


Figure 2. Effect Magnitude.

participating in other interventions only recently set up their firms (Fairlie *et al.*, 2015; Gonzalez-Uribe and Leatherbee, 2018; Martínez *et al.*, 2018).

Formal education is the only intervention that appears to be effective. Regarding the effects of *formal education on entrepreneurial income* ($n = 4$), all studies but one find positive effects. A potential reason that Williams (2003) finds no statistically significant returns to education is its small sample size of 176 individuals, which is the smallest of all studies under review and which substantially reduces its statistical power. In this category, we also find a study with one of the larger effects, though still moderately small. Van Praag *et al.* (2013), using the US National Longitudinal Survey of Youth, apply a wide variety of modeling techniques and specifications to a panel of entrepreneurs. They consistently find positive returns to education, which are larger ($g = 0.24$) than those found in the other studies under review. Results remain statistically significant and positive even after including general ability as a control in the second stage, on top of a credible set of instruments, which strengthens confidence in the results. Regarding the effects of *formal education on firm profits* ($n = 3$), in fact, all studies find positive effects. Parker and Van Praag (2006) set forth an interesting explanation as to why this is the case. Formal education provides the entrepreneur with a certificate that can be screened by capital investors. As a result, one pathway by which formal education increases business income is a reduction in capital constraints. This might also explain why formal education yields higher entrepreneurial benefits than noncertified human capital obtained by business training.

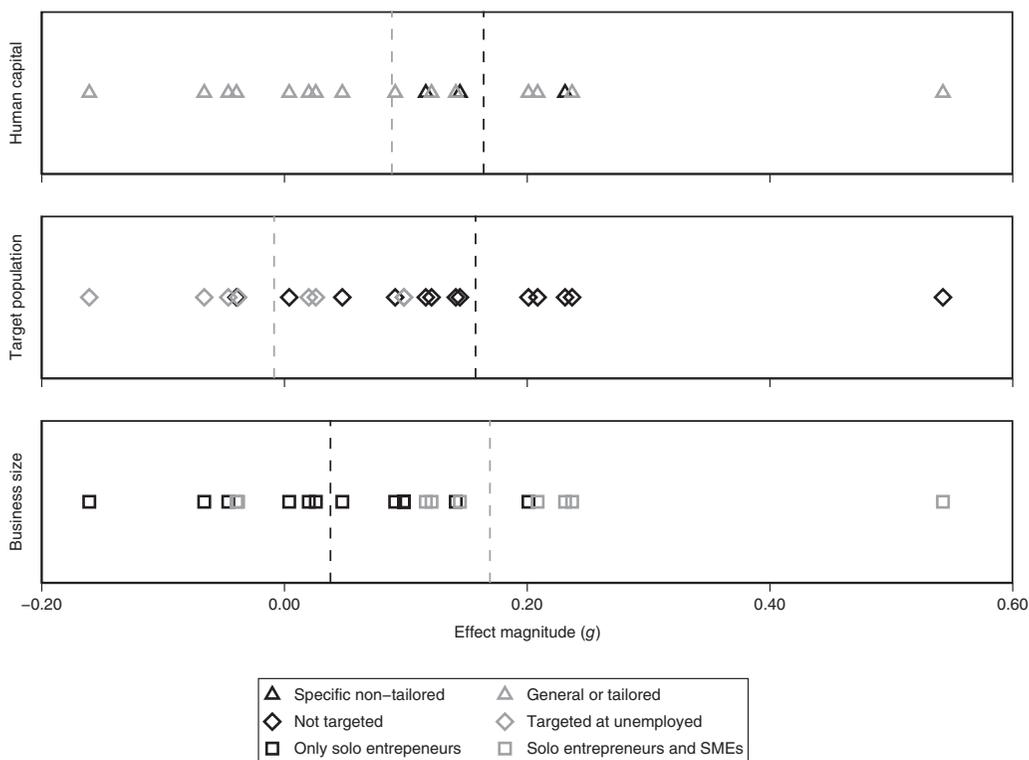


Figure 3. Effect Heterogeneity.

Note: Hedges' g gives the effect magnitude at latest follow-up. In case of multiple outcomes, multiple estimates are included. Dashed lines represent the mean values of g .

In Section 3, we laid out several expectations regarding effect heterogeneity. Our first expectation was that interventions that offer general human capital or well-tailored specific human capital have larger effects than interventions that offer ill-tailored specific human capital. The upper panel of Figure 3 shows that this is not the case. Effect magnitudes are similar across these categories. Nonetheless, this review only includes three estimates of specific nontailored interventions, which is insufficient for any conclusive statement. Our second expectation was that interventions targeted at low human capital individuals have smaller effects than untargeted interventions. The middle panel of Figure 3 suggests that this expectation is borne out. Interventions targeted at unemployed or underemployed individuals, our proxy for low human capital stock, have smaller effects than generic interventions. The difference is relatively large and there is little overlap between the effect magnitudes of both categories. Our third expectation was that interventions targeted only at entrepreneurs without employees have larger effects than interventions that also include entrepreneurs with employees. The lower panel of Figure 3 shows no support for this expectation. If anything, studies including both entrepreneurs with and without employees display larger effects. Note that all these comparisons are *between* studies and could hence be driven by other study characteristics instead. This is particularly the case concerning our first and third expectations. Specific human capital interventions that were well tailored are often combined with various other (nonhuman capital) interventions. An example is Schreiner (1999), where business

counseling is combined with monetary payments and exemptions from work search. In a similar fashion, small businesses with up to six employees (Fajnzylber *et al.*, 2009) are hardly comparable to businesses with up to 250 employees (Georgiadis and Pitelis, 2016). Therefore, it may be more informative to look at heterogeneity *within* studies.

Within-study comparisons show the following regarding effect heterogeneity. Gender does not appear as a source of heterogeneity in the effects of business training on sales revenue and firm size (Fairlie *et al.*, 2015), nor in the effects of formal education on entrepreneurial earnings (Iversen *et al.*, 2011). The role of urbanization is unclear. One study finds no heterogeneity by urbanization in the effects of training on earnings (Rodríguez-Planas, 2010), whereas another study finds moderate rural–urban differences in the effects of formal education on earnings (Tokila and Tervo, 2011). No information is provided as to whether these differences are statistically significant. Wealth and perceived credit constraints do not appear to condition the effects of business training on entrepreneurial income (Martínez *et al.*, 2018), although variation in this study is limited, as the intervention targeted extremely poor individuals. Differences by educational attainment appear to be small, at least regarding the effects of training on sales revenue (Rodríguez-Planas, 2010; Fairlie *et al.*, 2015). As for labor market experience, Fairlie *et al.* (2015) find no variation by employment status in the effects of training on sales revenue and firm size. Michaelides and Benus (2012) find no long-term variation by employment status either in the effects of training on entrepreneurial earnings. In contrast, labor market experience appears to play an important role in the effects of formal education. Iversen *et al.* (2016) show that formal education is more effective among those workers with more years of experience in wage employment prior to self-employment. Fossen and Büttner (2013) distinguish between entrepreneurs who were unemployed prior to entering self-employment, and other entrepreneurs. In line with our expectations as set out in Section 3, the authors find that the returns to education are lower for the former than the latter, and attribute it to the fact that previously unemployed entrepreneurs may be unsuccessful in fully employing their human capital in the labor market, whereas other entrepreneurs choose self-employment exactly because it enables them to better exploit their human capital. Also, in line with our expectations is the role of firm size. Van Praag *et al.* (2013) show that entrepreneurs without employees benefit significantly more from formal education than do small businesses with employees. They attribute this to the fact that entrepreneurs face fewer organizational constraints, and are hence better able to employ their human capital productively. As an additional piece of evidence, they show that entrepreneurs with more (perceived) control over their working environment have higher returns to education. Lastly, the effects of human capital interventions may be conditioned by the institutional context. Preliminary support is given by García-Mainar and Montuenga-Gómez (2005), who find that the effects of formal education on entrepreneurial earnings differ substantially between countries. However, they only offer ad-hoc explanations that are not further elaborated upon.

Regarding time effects, findings of the publications that include repeated posttests over time ($n = 7$) are mixed. Bruhn *et al.* (2018) find that subsidized access to management consulting and mentoring services has no effect on firm size one year after the intervention, yet does have a positive and statistically significant effect three to five years later. Coad *et al.* (2016), on the other hand, find no changes over time. A short business training appears to have no effect on turnover growth in any of the five years after the training took place. Fairlie *et al.* (2015), too, find that business training has no significant effects on sales revenue, firm size, or household income at any of the follow-up measurements. Martínez *et al.* (2018) find that business classes in combination with capital transfers have a positive and statistically significant effect on entrepreneurial income, sales, and profits one year after the intervention. However, the effects turn insignificant three years later. Michaelides and Benus (2012) rely on the same data set but apply slightly different model specifications. They find that business training negatively affects entrepreneurial income six months after the intervention, after which the coefficients turn insignificant. The studies published by Rodríguez-Planas (2010) and Rodríguez-Planas and Benus (2010) find no effects of self-employment assistance on monthly earnings during the first three years after intervention. Likewise, Schreiner (2003)

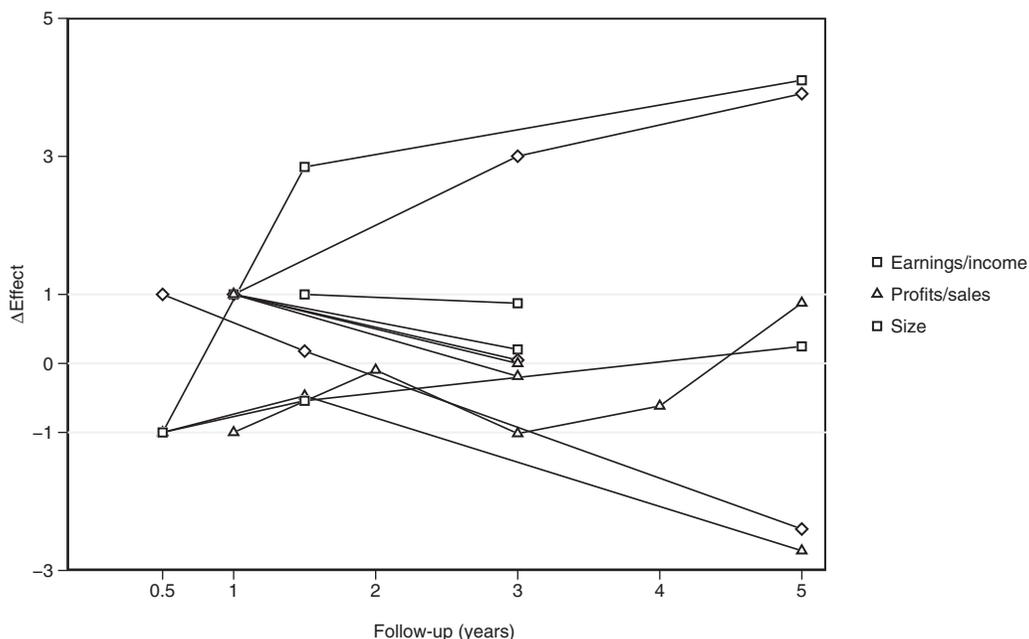


Figure 4. Changes in Effect Magnitude of Business Training over Time.

Note: For each study, the effect magnitude at first follow-up is set at 1 (positive effects) or -1 (negative effects). Values reflect the relative change of the effects over time. Rodríguez-Planas (2010) and Rodríguez-Planas and Benus (2010) report the average effect over the first two years, this datapoint is plotted at $t = 1.5$ years. Schreiner (1999) reports follow-ups at, respectively, 19 and 31, and 21 and 33 months, which have been averaged and plotted at 20 and 32 months.

finds no effect of microenterprise services on self-employment wage during the first 30 months after the program ended. If we were to ignore the statistical significance of the findings and would just focus on the estimate sign and its relative change over time, the picture would look like the one sketched in Figure 4. Again, there appears to be no clear trend toward increasing or decreasing effects.

Another time effect that would be interesting to elaborate on is the duration of the intervention itself. However, as mentioned before, there are not enough studies explicitly reporting program duration to come to meaningful inference.

5.3 Additionality

One of the premises underlying human capital interventions is that prevailing levels of human capital investment by entrepreneurs are suboptimal. Interventions are then necessary to correct this market failure. When such interventions affect large populations, they are likely to bring about changes in the general labor market equilibrium (Heckman *et al.*, 1999; Vooren *et al.*, 2019). The interventions under review tend to have relatively small groups of participants, reducing the need for general equilibrium models. Nonetheless, it may be informative to assess whether the interventions truly increase human capital over and above what would have occurred in the absence of these interventions.

Table 3. Training Receipt by Assignment Status

Publication		Treatment (%)	Control (%)	Additionality (%)
Bruhn <i>et al.</i> (2018)	($t = 1$)	×	×	×
	($t = 2$)	53.3	10.0	81.2
Coad <i>et al.</i> (2016)	τ	×	×	×
Elert <i>et al.</i> (2015)		×	×	×
Fairlie <i>et al.</i> (2015) ★	($t = 1$)	81.2	44.0	45.8
	($t = 2$)	86.4	57.3	33.7
	($t = 3$)	88.7	65.4	26.3
Fajnzylber <i>et al.</i> (2009)		×	×	×
Georgiadis and Pitelis (2016)		100.0	×	×
Gonzalez-Uribe and Leatherbee (2018)		×	×	×
Martínez <i>et al.</i> (2018)	τ	×	×	×
Meager <i>et al.</i> (2003)		×	×	×
Michaelides and Benus (2012) ★	($t = 1$)	90.0	35.0	61.1
	($t = 2$)	×	×	×
	($t = 3$)	×	×	×
Rodríguez-Planas (2010) ◀	τ	×	×	×
Rodríguez-Planas and Benus (2010) ◀	τ	×	×	×
Schreiner (1999)	($t = 1$)	74.8	×	×
	($t = 2$)	×	×	×

★ ◀ indicate usage of the same data sets.

t multiple follow-up moments and at least measured once, τ , multiple follow-up moments but all unknown.

× not reported and cannot be calculated.

We use the concept of additionality to assess whether interventions truly increase human capital. This concept is widely used in environmental economics (e.g., Chomitz, 1998; Zhang and Wang, 2011) and development economics (e.g., Luukkonen, 2000; Clarysse *et al.*, 2009). It indicates the degree to which changes in human capital occur as a result of an intervention and would not have occurred in its absence, and is thus related to the concept of crowding out (David *et al.*, 2000). Measuring additionality requires comparability of treatment and control groups, so that human capital changes in the control group constitute a counterfactual situation of what would have happened in the absence of the intervention. Experimental and quasi-experimental studies strive to ensure high comparability of treatment and control groups, and are therefore well-equipped to measure additionality.

Additionality is estimated consistently across studies using information about training receipt in the treatment and control groups. For each study, the estimate is calculated as the total increase in training in the treatment group, reduced by the proportion of that increase that also took place in the control group. In other words, the estimate indicates the degree to which the increase in human capital is attributable to the intervention.⁸ Note that this objective estimate differs from subjective estimates, which measure intervention perceptions instead. One of the studies under review provides such a subjective estimate, with 11% of the participants stating that the intervention made absolutely no difference yet only 27% stating that the intervention was crucial for the decision to start a business (Meager *et al.*, 2003).

Three conclusions can be drawn from our attempts to estimate additionality (Table 3). First, most studies provide insufficient information to calculate additionality. Authors do generally not investigate whether the control group seeks human capital investment elsewhere. The statement that “no service was provided to any firm not initially selected to receive a service” (Georgiadis and Pitelis, 2016,

p. 413) is illustrative in this respect. Most studies do not report compliance of the treatment group either. This is also problematic, for noncompliance is a potential cause of treatment ineffectiveness. Second, the few studies that provide sufficient information show moderately low additionality. It is thus possible that their interventions crowd out alternative human capital investments. The only exception is Bruhn *et al.* (2018), with an intervention that realizes relatively much additional human capital. Third, one study reports human capital investments for multiple follow-up moments (Fairlie *et al.*, 2015). It shows how additionality decreases over time, from 46% half a year after the intervention to 26% five years later, mainly as a result of the control group receiving training elsewhere. If similar patterns were to be observed in the other studies, then this finding casts doubt on the efficiency of the interventions, as many individuals seek training even in the absence of an intervention.

6. Discussion and Conclusion

In this paper, we provide a systematic literature review on the causal effects of human capital interventions on entrepreneurial performance in industrialized countries. We have identified 21 peer-reviewed studies published in English before September 2018. These studies examine interventions targeted at entrepreneurs already in business, unemployed individuals, or secondary school students. The interventions include business training or advice, formal education, and entrepreneurship education. The outcomes include entrepreneurial income or earnings, firm profits or sales, and firm size.

Five conclusions can be drawn from our analysis. First, there are few experimental or quasi-experimental studies on the effects of human capital interventions on entrepreneurial performance. Second, most interventions find no statistically significant effects. Third, statistically significant estimates vary from very small to moderately small in magnitude. The only intervention with a consistently positive effect on entrepreneurial earnings and firm profits is formal education, although the effects are small. Fourth, heterogeneity in treatment effects suggests that interventions targeted at unemployed individuals are less effective than untargeted interventions. This finding, however, is based on very few studies and must be considered with caution. Fifth, evidence is inconclusive as to whether effects persist over time.

Our review of quasi-experimental and experimental studies suggests that the effects are smaller in magnitude than those suggested by previous reviews of correlational studies. Martin *et al.* (2013) find that entrepreneurship education and training are related to entrepreneurial performance with an average magnitude of $\bar{r} = 0.17$, which in large sample equals $\bar{g} = 0.34$. Unger *et al.* (2011) find that human capital stock and investment relate to firm growth with magnitude $\bar{r} = 0.07$ ($\bar{g} \approx 0.14$) and to firm profits with $\bar{r} = 0.06$ ($\bar{g} \approx 0.11$). This is larger than the average effect magnitude of $\bar{g} = 0.10$ found in our selection of studies. Our estimates are more in line with experimental and quasi-experimental estimates from developing countries. For instance, Cho and Honorati (2014) find small effects of training alone ($\bar{g} = 0.06$) and of training and counseling ($\bar{g} = 0.09$) on entrepreneurial performance. This suggests that correlational studies overestimate the effect of human capital interventions on entrepreneurial performance.

Our review further shows that the interventions generate fairly little additional human capital over and above what would have been realized in the absence of the interventions. The few studies that provide sufficient information yield additionality estimates ranging from 26% to 81%. This is in line with estimates found elsewhere, such as an additionality of 12% for a subsidized training for British employers (Abramovsky *et al.*, 2005), 41% for a randomized education voucher experiment among Dutch low-skilled employees (Hidalgo *et al.*, 2014), 70% for a similar experiment among Swiss employed and unemployed workers (Schwerdt *et al.*, 2012), and 76% for a training program targeted at British SME workers at risk of losing their jobs (Devins and Johnson, 2003). These low additionality estimates suggest either that the interventions are poorly implemented with low take-up rates among participants, or that they crowd out alternative human capital investment. This is particularly worrisome if human capital interventions involve high public expenditure.

There may be several explanations for the small effects of human capital interventions on entrepreneurial performance. First of all, interventions may be too short to have an effect. We were unable to examine this as most studies did not report intervention intensity or duration, although the single most intense intervention did yield the largest effect. Another explanation is a lock-in effect (Card *et al.*, 2010; Vooren *et al.*, 2019). Participants reduce their entrepreneurial activity during an intervention, which may have negative short-term effects on entrepreneurial performance. Some interventions also had loan schemes embedded, which may lengthen the duration of lock-in effects due to a repayment period (Meager *et al.*, 2003). Examining this possibility requires more information about intervention intensity and duration as well as longer follow-up periods. A related explanation is that forgone experience or other investments may be better rewarded than human capital investments. Lastly, entrepreneurship may come about by other factors than mere human capital interventions. Previous studies have found a large genetic component in the probability of being an entrepreneur (Nicolaou *et al.*, 2008) as well as a sizable influence of parental on child entrepreneurship (Lindquist *et al.*, 2015). While these studies examine entrepreneurial activity rather than performance, it is conceivable that similar mechanisms are at work for performance. This could somewhat limit the scope for learning entrepreneurship through human capital interventions.

This review may be relevant for policy, since many industrialized countries pursue policies to encourage entrepreneurship. Our evidence suggests that policy makers should be cautious when designing human capital interventions other than formal education. One point to consider is their implementation, which should be sensitive to needs and to the environments in which they take place (Schreiner, 1999), and the corresponding take-up rates. Another point is to allow for the possibility that alternative policies might be more effective in enhancing entrepreneurial performance. These include policies aimed at other types of entrepreneurship. For example, solo entrepreneurship may be less beneficial economically than scale-up entrepreneurship (Isenberg, 2012; Duruflé *et al.*, 2017). At the same time, findings about solo entrepreneurship do matter as successful solo entrepreneurship can provide a sound foundation for successful scale-up entrepreneurship. In this respect, we concur with Duruflé *et al.* (2017) that policy makers should appreciate the entire “entrepreneurial ecosystem.” This ecosystem extends beyond human capital investments to include the scientific infrastructure, financial regulations, and funding policies.

We propose the following for future research. First and foremost, we call for the application of causal research designs in evaluating human capital interventions. Studies applying an experimental or quasi-experimental design are better equipped to find the true effects of interventions on the performance. Nonetheless, matching and IV studies are not immune to endogeneity, when there are unobservables involved or when instruments are not strictly exogenous. Greater exploitation of experiments and regression discontinuity designs could be promising (Lee, 2008). Second, future studies could pay more attention to imprecision in self-reported outcomes such as earnings. This could be aided by using additional validation data and bounding analysis (Bound *et al.*, 2001). Third, there is a need for longer follow-ups, since it remains unclear how human capital effects unfold over time (McKenzie and Woodruff, 2013; Grimm and Paffhausen, 2015). Fourth, studies could examine the mechanisms underlying human capital effects. This is especially important for studies that combine multiple treatments, as it helps to uncover why a particular intervention is effective and to what extent. A related suggestion is to broaden the set of outcomes. Even if interventions have little effect on entrepreneurial performance, they could still enhance welfare through increased autonomy, flexibility, or job satisfaction (Carter, 2011). Finally, studies could pay more attention to heterogeneity, as interventions probably play out differently across industries, between start-up and scale-up companies, or by economic and institutional context (Block *et al.*, 2017). Future research could explore these suggestions, taking into account the findings of this review.

Notes

1. All systematic reviews and meta-analyses mentioned here study a wide array of interventions and outcomes. For the sake of brevity, we only discuss those interventions and outcomes relevant to this paper.
2. These studies instrumented education with father's years of education and number of siblings, father's profession and religious affiliation, or variation in compulsory minimum school leaving ages across region and time.
3. Other recommendations pertain to differences between industries, types of entrepreneurs, learning processes and task requirements, and the role of government policies (see discussion by, for example, Block *et al.*, 2017). These are not addressed in the current review, due to the small number of studies that meet the inclusion criteria.
4. Sometimes, these studies are referred to as quasi-causal rather than quasi-experimental, since they do not involve a real experiment but instead use certain statistical techniques to approximate causal inference.
5. As of 2018, the following countries are member of the OECD and/or the EU: Australia, Austria, Belgium, Bulgaria, Canada, Chile, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Israël, Italy, Japan, Latvia, Lithuania, Luxembourg, Malta, Mexico, the Netherlands, New Zealand, Norway, Poland, Portugal, Romania, Slovakia, Slovenia, South Korea, Spain, Switzerland, Turkey, the United Kingdom, and the United States. All of these are high-income or middle-income countries, with relatively high 2015 Human Development Index scores ranging from 0.77 (Mexico) to 0.95 (Norway).
6. Two of these studies have been included in previous surveys. The study by Parker and Van Praag (2006) was included in the review of Van der Sluis *et al.* (2008), and the study of Michaelides and Benus (2012) was included in the meta-analysis of Martin *et al.* (2013).
7. The standardized effect estimator d is calculated by taking the mean difference in outcomes Y between treatment and control groups, and dividing it by the group size-weighted pooled standard deviation s_{pooled}

$$d = \frac{\bar{Y}_{\text{treatment}} - \bar{Y}_{\text{control}}}{s_{\text{pooled}}}$$

$$s_{\text{pooled}} = \sqrt{\frac{(n_{\text{treatment}} - 1)s_{\text{treatment}}^2 + (n_{\text{control}} - 1)s_{\text{control}}^2}{n_{\text{treatment}} + n_{\text{control}} - 2}}$$

Some studies do not report standard deviations and sample sizes by treatment and control group, and in IV regression, the distinction between treatment and control groups does not apply. In these cases, d can be approximated using the t -statistic of the test for mean differences and the total sample size N_{total}

$$d \approx 2t / \sqrt{N_{\text{total}}}$$

Correcting for small-sample bias, we obtain the estimator g

$$g = d \times \left(1 - \frac{3}{4(n_{\text{treatment}} + n_{\text{control}} - 9)} \right)$$

Note that in large samples, the difference between d and g is negligible (Lakens, 2013). Hence, the g estimates of this review pertaining to large sample studies can easily be compared to the d estimates of other reviews.

8. Following the voucher studies of Hidalgo *et al.* (2014) and Schwerd *et al.* (2012), we calculate additionality as the difference in the proportion of training receipt among the treatment groups and the

proportion of training receipt among the control groups, divided by the proportion of the treatment group that made use of intervention resources.

$$Add = \frac{p_{\text{training}}^{\text{treatment}} - p_{\text{training}}^{\text{control}}}{p_{\text{resources}}^{\text{treatment}}} \times 100\%$$

In contrast to voucher studies, the studies in this review measure training receipt among the treatment group as participation in the intervention program. We can therefore assume that all training receipt in the treatment group occurs at the cost of the intervention program. Additionality can then be calculated as

$$Add = \frac{p_{\text{training}}^{\text{treatment}} - p_{\text{training}}^{\text{control}}}{p_{\text{training}}^{\text{treatment}}} \times 100\% = 1 - \frac{p_{\text{training}}^{\text{control}}}{p_{\text{training}}^{\text{treatment}}} \times 100\%$$

The resulting indicator ranges from zero to one, with higher values indicating more additionality. Only in case that the treatment group receives *less* training than the control group, the indicator turns negative.

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