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Article — Accepted Manuscript (Postprint)

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Research in Social Stratification and Mobility

Provided in Cooperation with:

WZB Berlin Social Science Center

Suggested Citation: Pietrzyk, Irena; Erdmann, Melinda (2020) : Investigating the impact of interventions on educational disparities: Estimating average treatment effects (ATEs) is not sufficient, Research in Social Stratification and Mobility, ISSN 1878-5654, Elsevier, Amsterdam, Vol. 65, pp. --, <https://doi.org/10.1016/j.rssm.2019.100471>

This Version is available at:

<https://hdl.handle.net/10419/232505>

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Investigating the impact of interventions on educational disparities: Estimating average treatment effects (ATEs) is not sufficient

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In most national education systems, educational attainment depends heavily on parental socioeconomic status, which challenges the notion of equal opportunity. Educational interventions have increasingly become the focus of public and scientific debate as a tool for mitigating these disparities. Establishing appropriate methods for investigating the impact of these programs on inequality is of great interest to researchers, policymakers and practitioners. We help make methodological refinements in this research area. In defining educational disparities as unequal chances for educational attainment of different social groups on the macro level, we suggest considering not only how *strongly* participating persons from distinct social groups benefit from a program (i.e., group-specific ATEs) but also how *many* persons from distinct social groups take part in the program under real world conditions (i.e., group-specific participation rates (PRs)). As we define the PR as the share of a subpopulation participating in a program under real world conditions, PRs link the real world and macro level to the intervention level, and therefore, its consideration confers external validity. We develop a formula of how group-specific ATEs and PRs jointly contribute to an intervention's effect on disparities, and we simulate their joint contributions to disparities in university enrollment within reasonable limits and by presenting a fictitious yet realistic example. Because the contributions of group-specific ATEs and PRs to disparities have not been formalized yet, our results underscore the essential importance of PRs for understanding the impact of educational interventions on disparities. More specifically, interventions that appear to mitigate disparities based on a comparison of group-specific ATEs might in fact exacerbate inequality if low-SES persons are underrepresented in the program. Furthermore, the illustration helps to correct the misconception that focusing exclusively on group-specific ATEs (or on group-specific PRs) is sensible when drawing conclusions about a program's effect on disparities. We close with recommendations for future research and a review of the study's limitations.

Keywords: Educational inequality; Randomized controlled trials; External validity; Average treatment effects; Participation rates

1. Introduction

In most national educational systems, educational attainment depends heavily on parental socioeconomic status (e.g., Bar-Haim & Shavit, 2013; Blossfeld, Kulic, Skopek, Triventi, Kilpi-Jakonen, Vono de Vilhena et al., 2019), which contradicts the notion of equal opportunity. Furthermore, a stated shortage of skilled labor in some national economies has called for greater participation from socially disadvantaged persons in high-level tracks of the educational system (e.g., CEDEFOP, 2016; Hays, 2018). Based on these normative and economic concerns, policy makers and researchers have developed different instruments for changing educational trajectories. Educational interventions have increasingly moved into the focus of public and scientific debates as a tool for fostering the educational success of less privileged students and for mitigating educational disparities. Because the question of whether educational programs actually reduce inequality is of central importance for politicians, practitioners and researchers, it is crucial to develop a rigorous methodology that satisfies the demands of research on disparities. We here contribute to methodological development in this research area.

Against the definition of educational disparities as reflecting unequal chances between groups of different social origin for educational attainment at the macro level, we expand current methodological standards. Rather, in line with the current debate, we acknowledge the importance of randomized controlled trials (RCTs) in drawing internally valid conclusions about a program's effect on the educational attainment of distinct social groups. However, the external validity of conclusions, which is understood as the validity of conclusions on the impact of a program under real world conditions, should be considered carefully when dealing with disparities. In expanding the current standard, we argue that it is not only important to establish externally valid estimates of the average treatment effect (ATE) for each social group, but also that the participation rates (PRs) of different groups under real world conditions contribute considerably to a program's effect on inequality. We develop a formula for how group-specific ATEs and PRs influence inequality and illustrate the importance of PRs by simulating their impact on social disparities in university enrollment within reasonable limits and by presenting a fictitious yet reasonable example. Thus far, participation under real world conditions has mainly been discussed as a threat to the externally valid estimation of ATEs. We move beyond this focus by stressing the importance of group-specific PRs in shaping disparities, given externally valid estimates of ATEs. Even though the influence of group-specific PRs on educational disparities has occasionally been mentioned in the methodological literature, the joint contributions of group-specific ATEs and PRs have not yet been formalized. We thus contribute to a methodological standard that more precisely captures the impact of interventions on inequality in relation to the theoretical definition of educational disparities and with a higher degree of external validity than methods applied thus far.

The importance of an appropriate methodological standard for investigating the impact of educational programs on inequality arises from political, practical and scientific considerations: (i) On the policy-making level, the introduction of educational programs is frequently accompanied by high expectations for reducing disparities, which justifies the costs resulting from the implementation of educational programs. However, the political arguments only hold true if the

interventions in fact prove to be effective in mitigating inequalities. (ii) Practitioners developing educational interventions can become aware of previously neglected factors that drive disparities based on a methodological standard. Group-specific PRs, to which we call attention, are such a factor that until now has not been considered comprehensively in the development of interventions. (iii) For social researchers interested in describing mechanisms that drive inequality, it becomes more and more important to determine the impact of educational programs on disparities, since interventions increasingly shape various national educational systems. Whereas school guidance counseling, which aims at developing students educational and professional success, has already been quite common in the USA during the last decades (e.g., Gysbers, 2005), guidance programs have recently gained more importance in Europe as well. In 2003, OECD, the European Commission and the World Bank reviewed national guidance policies of 37 countries (Sultana, 2004; Watts & Sultana, 2004). In the review, guidance refers to services that aim at assisting participants to make educational, training and occupational choices, thereby intending to foster lifelong learning, labor market and economic development, and/or social equity. Most countries involved in the report stated an increase in the range and reach of such services. The authors conclude that guidance is “higher on the public policy agenda than ever before” (Watts & Sultana, 2004: 106).

There is a wide variety of educational interventions: Some programs aim at fostering academic achievement in different fields, such as mathematics or language (e.g., Gersten, Haymond, Newman-Gonchar, Dimino, & Jayanthi, 2019; Jitendra, Lein, Im, Alghamdi, Hefte, & Mouanoutoua, 2017), and might therefore help reduce the achievement gap between social groups, that is, the so-called primary effects of social origin (Boudon 1974). Additionally, a growing number of programs seeks to raise transition rates into ambitious educational tracks. Hence, these interventions might help reduce the social gap in educational decisions that typically exists net of academic performance differences, that is, the so-called secondary effects of social origin (Boudon, 1974; Breen & Goldthorpe, 1997; Erikson & Jonsson, 1996). Interventions of the latter kind range from delivering general information on the costs and benefits of educational pathways (e.g., Barone, Schizzerotto, Abbiati, & Argentin, 2017; Hastings, Neilson, & Zimmermann, 2015; Kerr, Pekkarinen, Sarvimäki, & Uusitalo, 2014; McGuigan, McNally, & Wyness, 2016), offering practical support for different tasks, such as filling out blanks or reminding students of their initial plans via short messages (Bettinger, Long, Oreopoulos, & Sanbonmatsu, 2012; Castleman, Page, & Schooley, 2014), to providing extensive and individual guidance counseling (e.g., Avery 2013; Meyers, Olsen, Seftor, Young, & Tuttle, 2004).

In fact, many programs enhance the educational success of students in general, as has been shown by means of rigorous RCTs (Connolly, Keenan, & Urbanska, 2018). Some studies especially focus on the effects for low-SES students (for academic achievement: e.g., Dietrichson, Bøgg, Filges, & Klint Jørgensen, 2017; for the transition to higher education: e.g., Herbaut & Geven, 2019). However, educational interventions do not automatically mitigate educational disparities by fostering the educational success of low-SES students. A positive impact on the educational attainment of these students is a promising result but not a sufficient condition for reducing disparities. Since the relation of social groups defines inequalities, it is also necessary to determine the ATEs for socially privileged students if the program under investigation is also

delivered to this group. We will show that the additional consideration of group-specific PRs is essential.

To expand upon RCT methodologies used in research on educational disparities, we define educational inequality (chapter 2). Against this background, we present our suggestions in terms of investigating the effect of interventions on educational disparities (chapter 3). To strengthen our claims about the essential importance of PRs, we develop a formula of how group-specific ATEs and PRs jointly influence educational inequality and simulate their contributions to disparities in university enrollment within reasonable limits and by presenting a fictitious yet reasonable example. Furthermore, simplifications and assumptions of the derived formula are discussed (chapter 4). The paper ends with recommendations for future research and a review of the study's limitations (chapter 5).

2. A definition of educational inequality

Before presenting our methodological considerations, we first provide a definition of educational inequalities that is widely accepted in social research. We focus on inequalities based on social origin, even though we believe that our definition is transferable to educational inequalities based on, for example, gender or migration background.

The influential work of Boudon (1974) can serve as a starting point for defining educational disparities, according to which educational disparities should be understood as inequality of educational opportunity. In more specific terms, this refers to differences in the level of educational attainment between groups of different social origin (Boudon, 1974). This definition includes the relation of different social strata to each other. While investigating the impact of educational expansion on inequalities, Mare (1980: 297) systematically drew attention to the fact that an increase in overall educational attainment should be distinguished from the chances of specific social strata to achieve educational success. This idea was picked up, among others, by Blossfeld and Shavit (1991). In their influential book *Persistent Inequality*, they argue that educational opportunity “means the chance to attain a specific educational level, rather than its attainment. It is a relative not an absolute concept. As a consequence of educational expansion societies can produce a higher average level of educational attainment from one birth cohort to the next, without changing the educational opportunities of children from different social strata.” (Blossfeld & Shavit, 1991: 28) The authors illustrate this idea using the example of Great Britain during the interwar period: During this time, the chance of the working class to gain a place in a selective secondary school rose from 20 % to 26 %, while the chance of the service class increased from 70 % to 77 % (Halsey, Heath, & Ridge, 1980; cited by Blossfeld & Shavit, 1991). Thus, the chance difference between working class and service class had increased by one percentage point in the course of educational expansion. Educational disparities had increased.

Aside from the authors' relational perspective, their considerations point out the following aspects: First, a distinction is made between an overall increase in educational level and the opportunities of specific social strata. When analyzing the change in inequalities over time, the focus should be on the change in the chance difference. Second, chance is defined as the share of a social stratum that enrolls in a specific type of school. Therefore, the outcome is ultimately situated at the macro level, even though enrollment naturally is the result of individual behavior on the micro level against the background of institutional restrictions and opportunities. Consistent with these explanations, we define educational inequalities as the unequal chance for educational success at the macro level dependent on social origin.¹

3. A discussion of the methodology to evaluate interventions

In the following paragraphs we suggest how the effect of interventions on educational inequality should be investigated within the framework of RCTs. In line with current standards, we believe that *internally valid* conclusions on the causal effects of an intervention should be obtained by means of randomized controlled trials (RCTs) that provide estimates for the ATE on educational attainment for different social groups. However, as the definition of educational inequalities is ultimately situated at the macro level, we stress the importance of *external validity*. The methodological literature has mainly explored which factors threaten the external validity of ATEs. Although it is indisputably important to consider these general threats, we extend the methodological debate by arguing that ATEs, even if they are externally valid, are not sufficient to draw externally valid conclusions about an intervention's impact on disparities. We suggest that in addition to externally valid ATEs, the consideration of PRs under real world conditions is crucial to drawing conclusions about the impact of interventions on disparities.

3.1 Internal validity: Conclusions about a causal effect

Internal validity of causal inferences refers to the validity of inferences about whether the observed association between a treatment and outcome reflects a causal relationship (e.g., Shadish, Cook, & Campbell, 2002: 38). Internally valid conclusions on a causal effect are best obtained through randomized controlled trials (RCTs) as noted in the methodological literature (e.g., Shadish et al., 2002).

¹ This definition does not allow for mistaking the increase of the educational success of low-SES students for a reduction of inequalities. Narrowing the definition down to the educational attainment of socially disadvantaged students would ultimately neglect the fact that educational disparities transfer to inequalities on the labor market, where individuals compete for valuable and restricted job positions. The distribution of limited valuable goods distinguishes research on (educational) inequality from most other research fields applying RCTs (e.g., health studies), where the primary outcome is typically situated at the individual level and not directly limited (e.g., psychological or physical health).

The main virtue of RCTs lies in the randomization of participants to treatment conditions before treatment start, a characteristic that distinguishes RCTs from observational studies and that leads to the classification of RCTs as experiments or field experiments. Since the allocation of subjects is purely random, resembling the tossing of a coin, a selection bias into conditions is minimized. This allows for the assumption that no meaningful differences between groups exist before treatment. When a difference between treatment groups is found after treatment, it is assumed that given the groups' pre-treatment similarity, this difference was caused by the treatment. This comparison of groups on the primary outcome generates the main result when conducting RCTs. Such a comparison delivers the so-called ATE. This parameter is typically specified as an effect size. For continuous outcomes, Cohen's d can be used and reflects the difference in the mean value between groups in relation to within-group variance. For dichotomous outcomes, the difference in percentage points is typically reported. These core features of RCTs, the randomization and post-treatment comparison of groups, are most important in making internally valid claims about the effect of an intervention.

Several factors threaten the internal validity of conclusions derived from RCTs, inter alia systematic differences in the attrition of participants across treatment conditions (e.g., Shadish et al., 2002: 62; Jüni, Altman, & Egger, 2001). Selective panel attrition endangers the comparability of experimental groups and, therefore, the main virtue of randomization. Furthermore, the internal validity rests upon the stable unit-treatment value assumption (SUTVA) that assumes that there is "no interference between units [...], leading to different outcomes depending on the treatment other units received [...]" (Rubin 1980: 591). Although these factors challenge the internal validity of the results, RCTs are typically superior to other methods in terms of internal validity (e.g., Shadish et al. 2002).

Accordingly, we believe that RCTs should be a central pillar when investigating the effect of interventions on educational inequality in correspondence with previous research (for an overview see: Herbaut & Geven, 2019). However, some trials conducted in educational research have investigated the effect of interventions on the attainment of exclusively or mainly low-SES students (for the access to higher education: e.g., Ford, Grekou, Kwakye, & Nicholson, 2014), which does not provide sufficient evidence about the effect on disparities if the program is also delivered to socially privileged students under real world conditions. In contrast, we stress the importance of establishing group-specific ATEs for all distinct social groups that take part in the program, as the relations between groups define disparities.² When an RCT is developed, distinct social groups should be considered cautiously. This requires that each group be represented in sample size calculations and accordingly in sampling and recruitment procedures. In conducting RCTs with such a focus on group-specific ATEs, it can be determined to what degree the educational success of low- and high-SES students *participating in an RCT* is influenced by the treatment.

² If the possibility of a significant participation of high-SES students under real world conditions is cautiously excluded, a study focus on low-SES students is plausible. However, it is difficult to establish the cut-off for a significant PR of the high-SES group without prior knowledge of group-specific ATEs, because even with high-SES students being underrepresented inequality might be increased, as will be shown in the following. Hence, only an almost complete exclusion of high-SES students from interventions under real world conditions allows for ignoring the high-SES group in establishing the ATEs when the evaluation aims at drawing conclusions on disparities.

3.2 External validity: generalizability and conclusions at the macro level

In the following section, we present current methodological discussions on external validity in the context of RCTs. Although we principally agree with the importance of cautiously considering different threats to external validity that are discussed comprehensively in the methodological literature, we argue that it is necessary to also consider the PRs of distinct social groups to draw externally valid conclusions about educational disparities.

3.2.1 General threats to external validity

In the context of RCTs, external validity refers to the extent to which a causal relationship is generalizable beyond a study's context, e.g., across variations in persons and settings (e.g., Shadish et al. 2002: 38). Researchers are typically interested in whether the results obtained through RCTs are generalizable to the conditions under which a program is delivered in the real world (e.g., Rothwell, 2005; Stuart, Bradshaw, & Leaf, 2015).

Several general threats to external validity are discussed in the literature and should be considered carefully in research on educational inequality (e.g., Shadish et al. 2002: 86 ff; see also: Rothwell, 2005): (i) *variations in persons*: studied subjects might differ from participants in the real world in observed characteristics such as gender or in unobserved characteristics such as the motivation to learn. (ii) *variations in treatment*: studied subjects typically receive treatment under best-practice conditions whereas persons in the real world might receive treatment under suboptimal conditions. (iii) *variations in settings*: a program might work especially well in socially privileged areas while failing to have an impact in socially disadvantaged areas due to a limited access to resources in socially disadvantaged schools.

These examples illustrate that the external validity of ATEs will be attenuated if the studied sample or, respectively, the studied treatment differs from participants or, respectively, treatment under real world conditions. First, differences in the potential outcome given no treatment can exist (also referred to as: \bar{Y}^0). Second, differences in responsiveness to treatment can exist (also referred to as the difference between potential outcomes given treatment and given no treatment: $\bar{Y}^1 - \bar{Y}^0$). In line with the present debate, we acknowledge the importance of considering these threats in research on educational disparities. Externally valid ATEs can demonstrate how strongly the educational attainment of low- and high-SES students, *who receive treatment under real world conditions*, is influenced by a given educational program.

Even though establishing internally and externally valid group-specific ATEs is crucial to drawing conclusions about disparities, merely considering ATEs is frequently not sufficient.

3.2.2 Specific threats to external validity in research on inequalities

We suggest not to narrow the definition of external validity to the question of whether causal inferences are transferable to variations that can be found in the real world, but to broaden the definition to conclusions about an intervention's effect in the real world in general.

When dealing with the effect of interventions on disparities in the real world, it is important not only to estimate how *strongly* an intervention affects participants from different social groups under real world conditions (i.e., to obtain group-specific internally and externally valid estimates of ATEs) but also to determine how *many* persons of different social origins participate in the intervention under real world conditions. We capture this quantity with the term participation rate (PR) and define it as the share of a subpopulation of a specific social origin that actually participates in an intervention under real world conditions. Since PRs link the real world and the macro level to the intervention level, their consideration adds external validity.

The following example illustrates that an intervention's effect on disparities most often cannot be determined based exclusively on externally valid group-specific ATEs, but that PRs must also be taken into account. Let us assume that it has been established that a given educational program enhances under real world conditions the university enrollment of participating low-SES students to the same considerable extent as it fosters the university enrollment of participating high-SES students (i.e., equal externally valid ATEs across groups). Does this program enhance or reduce educational inequality in the transition to higher education under real world conditions? The answer depends on PRs. If we assume that after program implementation *all* low-SES students of a population participate in the program whereas only *half* of high-SES students take part, the program will reduce educational disparities in university enrollment. This is the case, as socially disadvantaged students are overrepresented and ATEs are equal across groups. Conversely (*half* of the low-SES students participate; *all* high-SES students participate), the program will increase disparities. Therefore, PRs not only slightly influence a program's impact on disparities, but rather an intervention's effect can even be reversed based on PRs.

Some work on the participation of social groups in educational programs exists and finds low-SES students to have less access to beneficial guidance counselling programs (Avery et al., 2014; for social inequalities in access to (preventive) health programs: e.g., Ahmed, Creanga, Gillespie, & Tsui, 2010; Hoeck, Van der Heyden, Geerts, & Van Hal, 2014; Palencia, Espelt, Rodriguez-Sanz, Puigpinos, Pons-Vigues, Pasarin, Spadea, Kunst, & Borrell, 2010; Veugelers & Yip, 2003; Zackrisson, Andersson, Manjer, & Janzon, 2003). However, the fact that educational programs might exacerbate educational inequality based on certain combinations of group-specific ATEs and PRs is not stressed in the broad literature on educational programs, even though the joint impact of group-specific ATEs and PRs on inequality has occasionally been discussed in research on educational inequality (e.g., Argentin 2017).³ In health studies, a common understanding exists about the joint influence of group-specific ATEs and PRs on disparities: if interventions are population-wide or targeted to low-SES persons comparing group-specific ATEs or, respectively, focusing on the ATE for low-SES persons is appropriate

³ For example, Avery et al. (2014), who stress the importance of social differences in access to beneficial programs, do not frame this as an issue of disparities: "The flipside of the positive effects found in the academic literature from additional college counseling is that lack of counseling is a strong explanatory factor of the failure of many qualified students to enroll in (appropriate) four-year colleges" (p. 10). The authors acknowledge that low-SES students do not benefit from educational programs to the same extent as high-SES students due to lower access, but they do not take the next step in making this a subject of educational disparities between social classes.

for drawing conclusions about the program's impact on inequality (e.g., Hillier-Brown, Bamba, Cairns, Kasim, Moore, & Summerbell, 2014; Moore, Littlecott, Turley, Waters, & Murrey, 2015; Vilhelmsson & Östergren, 2018).

However, many educational interventions neither include the whole population nor exclude high-SES students completely. If a certain share of distinct groups participates in a program under real world conditions, the intervention's effect on disparities depends on the unique pattern of group-specific ATEs and PRs. Thus, in case of distinct groups being represented in a program, ad hoc conclusions about the program's effect on disparities based exclusively on a comparison of group-specific ATEs (or exclusively on group-specific PRs) are susceptible to error. Since to the best of our knowledge no model formalizing the joint contributions of group-specific ATEs and PRs has been derived so far, estimations of how these programs shape disparities are hampered. Therefore, we help to make methodological refinements in this research field by deriving how these parameters jointly affect the treatment effect on disparities.

4. A model and simulation of the treatment effect on disparities

In the following section, we simulate how group-specific ATEs and PRs contribute jointly to the treatment effect on educational inequalities.

First, we derive a mathematical equation that describes how group-specific ATEs and PRs contribute to the treatment effect on inequality. Second, we show how these group-specific parameters affect the treatment effect on disparities for the transition to higher education. To keep the illustration as realistic as possible, we refer to previous empirical findings on the group-specific parameters. Using realistic values for the parameters in the previously derived formula, we simulate the joint contributions of group-specific ATEs and PRs to the treatment effect on disparities within realistic limits and we construct a fictitious yet realistic example that we discuss in more detail. Therefore, we use the simulation as a tool for mitigating the complexities of multifaceted phenomena by manipulating variables separately (e.g., Axelrod, 2006; Bratley, Fox, & Schrage, 1987). Third, we discuss simplifications of the derived formula by pointing to uncertainties in point estimators and underlying assumptions.

4.1 Modeling the treatment effect on disparities

We aim to formulate a model that describes an intervention's effect on disparities. Since disparities are by definition located on the macro level, we pay particular attention to this level. The derived formula can be used to draw externally valid conclusions in cases where only a certain share of the population takes part in a program under real world conditions; however,

the formula is also applicable to cases where all units of a population participate in an intervention. The final equation describes the treatment effect on educational inequalities (TE_{EI}) based on group-specific ATEs and PRs.

We begin with a formal definition of the ATE.⁴ According to the counterfactual framework, the definition of a causal effect is formulated as $\delta = Y_i^1 - Y_i^0$ (e.g., Rubin 2005). Thus, the effect of an event (e.g., treatment) is defined as the difference in potential outcomes observed under two conditions: first, in the case the event occurs (Y_i^1) and second, in the case it does not occur (Y_i^0). Given the impossibility of obtaining both outcomes for the same person at the same time (counterfactual), the causal effect is estimated as an ATE for different observations. Therefore, the ATE is estimated as:

$$ATE = \bar{Y}^1 - \bar{Y}^0 \quad (4.1).$$

We now formally introduce a distinction between (i) the potential outcome under the condition of an intervention (\bar{Y}^1) and (ii) the potential outcome under the condition of an intervention at the macro level (\bar{Y}^1_{macro}). In doing so, we make use of the PR, which indicates the share of a population taking part in a program under real world conditions. We define the potential outcome under the condition of an intervention at the macro level as:

$$\bar{Y}^1_{macro} = \bar{Y}^1 * PR + \bar{Y}^0 * (1 - PR) \quad (4.2).$$

Hence, we formally define \bar{Y}^1_{macro} as the sum of the potential outcome given an intervention (\bar{Y}^1) weighted by the PR and the potential outcome given no intervention (\bar{Y}^0) weighted by $(1 - PR)$. For cases in which all units of the population are treated ($PR = 1$), \bar{Y}^1_{macro} equals \bar{Y}^1 . In all other cases, \bar{Y}^1_{macro} will be attenuated in comparison to \bar{Y}^1 based on the fact that the population does not fully participate in the program.

We now derive the treatment effect on educational inequality (TE_{EI}). Since inequalities are defined as differences between social groups at the macro level, we define TE_{EI} as the difference between group-specific treatment effects on the macro level:

$$TE_{EI} = TE_{macro, low-SES} - TE_{macro, high-SES} \quad (4.3).$$

Because TE_{macro} is group-specific, all parameters contributing to TE_{macro} for distinct social groups might differ between groups. Therefore, we assume group-specificity of the parameters in the following.

⁴ For more detailed descriptions of the estimation of causal effects, see: Rubin 2005, Deaton & Cartwright 2018.

We now use the general formula for the ATE (4.1) to define group-specific TE_{macro} contained in eq. 4.3 above. However, we modify the common definition of the ATE, since we aim to derive effects at the macro level. Therefore, we insert the potential outcome given an intervention at the macro level (\bar{Y}^1_{macro}) instead of the potential outcome given an intervention (\bar{Y}^1):

$$TE_{EI} = (\bar{Y}^1_{macro, low-SES} - \bar{Y}^0_{low-SES}) - (\bar{Y}^1_{macro, high-SES} - \bar{Y}^0_{high-SES}) \quad (4.4).$$

By inserting the equation for \bar{Y}^1_{macro} (4.2) into the corresponding group-specific parameters in eq. 4.4 above, we derive the following formula:

$$TE_{EI} = \left((\bar{Y}^1_{low-SES} * PR_{low-SES} + \bar{Y}^0_{low-SES} * (1 - PR_{low-SES})) - \bar{Y}^0_{low-SES} \right) - \left((\bar{Y}^1_{high-SES} * PR_{high-SES} + \bar{Y}^0_{high-SES} * (1 - PR_{high-SES})) - \bar{Y}^0_{high-SES} \right) \quad (4.5).$$

After shortening the equation, only the main parameters are left:

$$TE_{EI} = (\bar{Y}^1_{low-SES} - \bar{Y}^0_{low-SES}) * PR_{low-SES} - (\bar{Y}^1_{high-SES} - \bar{Y}^0_{high-SES}) * PR_{high-SES} \quad (4.6).$$

To highlight the function of group-specific ATEs in the last equation, we insert the ATEs instead of potential outcomes:

$$TE_{EI} = ATE_{low-SES} * PR_{low-SES} - ATE_{high-SES} * PR_{high-SES} \quad (4.7).$$

The derived equation illustrates clearly that group-specific ATEs and PRs contribute jointly to TE_{EI} . Furthermore, the equation helps to derive conditions under which certain parameters become irrelevant for TE_{EI} :

- (i) When a program targets the population ($PR_{low-SES} = 1$ and $PR_{high-SES} = 1$), TE_{EI} corresponds to comparing group-specific ATEs. Thus, with full participation of the population under real world conditions, it is appropriate to draw conclusions on TE_{EI} based exclusively on the comparison of group-specific ATEs.
- (ii) When a program targets the subpopulation of low-SES students and excludes high-SES students ($PR_{low-SES} = 1$ and $PR_{high-SES} = 0$), TE_{EI} corresponds to $ATE_{low-SES}$. Thus, conclusions about TE_{EI} can be drawn solely on $ATE_{low-SES}$ in this case, and consideration of $ATE_{high-SES}$ is not necessary.

(iii) When a program targets low-SES students and excludes high-SES students ($0 < PR_{\text{low-SES}} < 1$ and $PR_{\text{high-SES}} = 0$), TE_{EI} will be, in comparison to $ATE_{\text{low-SES}}$, attenuated by the fact that the subpopulation of low-SES students does not participate fully in the program. In this case, variations of TE_{EI} that depend on $PR_{\text{low-SES}}$ mirror the scaling-up and -down of interventions, and $ATE_{\text{high-SES}}$ does not influence TE_{EI} .

However, many educational programs neither include the full population nor exclude certain SES-groups completely. In these cases (i.e., ($0 < PR_{\text{low-SES}} < 1$ and $0 < PR_{\text{high-SES}} < 1$) or ($PR_{\text{low-SES}} = 1$ and $0 < PR_{\text{high-SES}} < 1$) or ($0 < PR_{\text{low-SES}} < 1$ and $PR_{\text{high-SES}} = 1$) group-specific ATEs and PRs contribute jointly to TE_{EI} in a unique pattern depending on parameter values.

Therefore, it is reasonable to clarify the interdependencies of these parameters in influencing TE_{EI} . To illustrate their joint impact on TE_{EI} , we run a simulation using the derived formula with reasonable values for the parameters. Even though the formula is simplified, the simulation highlights the importance of considering group-specific ATEs and PRs, and shows inter alia that conclusions on TE_{EI} based exclusively on a comparison of group-specific ATEs (or exclusively on a comparison of group-specific PRs) are susceptible to error. After the simulation, we discuss simplifications and assumptions of the formula.

4.2 Simulation of the treatment effect on disparities

To demonstrate the joint contribution of group-specific ATEs and PRs to the treatment effect on disparities in university enrollment, we simulate interdependencies by inserting reasonable values for these factors into the formula for TE_{EI} .

Our simulation contains two approaches. First, we derive reasonable limits for the parameters and simulate the joint contributions within these reasonable limits to visualize general patterns by covering a wide range of possible combinations. Second, we construct a fictitious yet realistic example of one specific combination of parameters to discuss one case in more detail.

Literature overview

To derive reasonable values for the parameters, we refer to previous research.⁵ Due to the scarceness of studies investigating the impact of programs on university enrolment, we used an extended criteria catalog to select studies. Consequently, our selection includes studies applying an RCT design and focusing on interventions fostering the transition to higher education through information, mentoring or mixed programs but with no further limits. As a result, our sample of studies varies, inter alia, in terms of particular interventions and operationalizations of the primary outcomes (i.e., application for higher education, intention to enroll at a higher education institution, actual enrollment at different types of higher education institutions). Furthermore, the studies were conducted in different countries, meaning that they refer to different

⁵ A comprehensive systematic review of interventions that aim at reducing inequalities in access to higher education is provided by Herbaut and Geven (2019), who also include results derived from quasi-experimental methods.

educational systems. In some studies, the intervention was carried out for the investigation exclusively and did not continue. Despite this heterogeneity, we are convinced that the gathered information provides sufficient reference points for simulations conducted within a realistic framework (for an overview of the included studies, see tab. 1).⁶

Table 1: Overview of intervention studies

Author(s)/ state	Intervention	Outcome(s)	Sample size/ selection	Findings on enrollment or intention to enroll (only significant effects with $p < 0.05$ reported)
Abbiati, G., Argentin, G., Barone, C., & Schizzerotto, A. (2018: 1262) * + <i>Italy</i>	information on costs, benefits and success expectations	college enrollment; expectations about the profitability of college education	n = 9,159; high school students from different social classes	(i) no significant effect on enrollment
Avery, C. (2013: 18, 22) * <i>USA</i>	extensive tutoring; support in financial aid application and college admission	college enrollment; ACT score	n = 238; low-income high school students	(i) no significant effect on enrollment at any institution; (ii) + 30 p.p. application for a 4-year college; (iii) + 44 p.p. application for a competitive institution; (iv) + 15 p.p. enrollment at a 4-year college.
Barr, A., & Castleman, B. (2017: 28) * <i>USA</i>	college counseling	college enrollment; persistence in college	low-income, first generation junior or senior high school students (GPA ≥ 2.5)	(i) + 7.0 p.p. for enrollment at any institution; (ii) - 3.4 p.p. for enrollment at a 2-year institution; (iii) + 10.3 p.p. for enrollment at a 4-year institution
Bettinger, E. P., Long, B. T., Oreopoulos, P., & Sanbonmatsu, L. (2012: 1226) * <i>USA</i>	assistance in completing the Free Application for Federal Student Aid (FAFSA); information on student aid	FAFSA filing; college enrollment	n = 868; low-income families	(i) + 15.7 p.p. for filling out FAFSA; (ii) + 8.1 p.p. for enrollment at any college; (iii) information-only treatment no significant effects
Bonilla, L., Bottan, N. L., & Ham, A. (2017: 25) * + <i>Colombia</i>	information on returns to higher education, financial aid and admission criteria	beliefs; test scores; college enrollment	n = 6,289; low-income and middle-income high school seniors,	(i) no significant effect on enrollment at any institution for any group; (ii) 0.5-0.6 p.p. for enrollment at top-10 institution for all students
Bos, J.M., Berman, J., Kane, T.J., & Tseng, F.M. (2012: 19) * + <i>USA</i>	outreach from advisors to support the college and financial aid identification, application, and admissions processes	grades; taking the SAT; college enrollment; persistence in college	junior high school students a) whose parents did not attend college n = 1,188, b) whose parents did attend college n = 260	a) + 6.1 p.p. enrollment at 4-year institution; b) no significant effect for enrollment at 4-year institution
Carrell, S. E. & Sacerdote, B. (2013: 53)	mentoring, paying application and College Board/ ACT fees	college enrollment	n = 2,623; high school students from different social classes	(i) + 6 p.p. for enrollment at any institution

⁶ We believe that the sample of intervention studies is too heterogeneous for a meta-analysis, which is commonly used to obtain more precise and robust information about the average ATE across studies. Furthermore, we did not evaluate the results of the studies in terms of internal and external validity. In some studies, the reliability of the estimates of group-specific ATEs may be limited due to low sample size. As we use previous research only to obtain realistic limits for illustrative purposes and to construct a fictitious yet realistic example, we assess these uncertainties as tolerable.

<i>USA</i>				
Castleman, B. L., Page, L. C., & Schooley, K. (2014: 334) ** <i>USA</i>	summer counseling interventions including college financial aid advising, short messages and scholarships	on-time college enrollment persistence in college	n = 784 & 1,446; college-intending high school graduates (already applied and accepted) from different social classes	(i) + 3 p.p. for enrollment on time; (ii) low-income students: + 12.3 p.p. for enrollment on time (only uAspire Program); (iii) + 5 p.p. for continuous enrollment through three semesters; (iv) higher-income students: no significant effect (both programs)
Ehlert, M., Finger, C., Rusconi, A., & Solga, H. (2017: 203) ** <i>Germany</i>	information on costs and return to higher education	intention to enroll at college (application)	n = 428; high school students from different social classes	(i) students without academic background: + 12.4 p.p. for application for college (ii) students with one parent with academic degree: + 17.4 p.p. for application for college (ii) both parents with academic degree: no significant effect
Ford, R., Grekou, D., Kwakye, I., & Nicholson, C. (2014: 15) * <i>Canada</i>	extracurricular project with focus on information on post-secondary studies	college/university enrollment	n = 1,033; low-income and first generation high school students (10 th grade)	(i) + 10.1 p.p. for enrollment at any institution (see Herbaut, E., & Geven, K. 2019: 55) (ii) + 7.7 p.p. enrollment at university
Hahn, A., Leavitt, T., & Aaron, P. (1994: 8) * <i>USA</i>	very intensive program with around 750 hours of education, activities and service from 9 th grade to graduation and extra monetary incentives	college enrollment	n = 158; low-income high school students	(i) + 26 p.p. enrollment at any institution
Hastings, J., Neilson, C. A., & Zimmerman, S. D. (2015: 24) * <i>Chile</i>	information on earnings and costs for past students	college enrollment earnings net of chosen degree	n = 16,594; low-SES high school students; applicants for student loan	(i) no significant effect on enrollment
Kerr, S. P., Pekkarinen, T., Sarvimäki, M., & Uusitalo, R. (2015: 39) <i>Finland</i>	information on labor market prospects in school	college application college enrollment	n = 3,500 ; high school students	(i) no significant effect on application or enrollment at any institution
Loyalka, P., Song, Y., Wei, J., Zhong, W., & Rozelle, S. (2013: 34) * <i>China</i>	information on college costs and financial aid	college enrollment	n = 2,256; high school seniors in the poorest counties	(i) +8 p.p. enrollment at any institution
McGuigan, M., McNally, S., & Wyness, G. (2016: 509) <i>UK</i>	information on costs and benefits of postsecondary education	stay in full-time education beyond age of 16 intention to apply for college	n = 6,614; high school students from different social classes (10 th grade)	(i) effect on intention to stay in full-time education beyond age of 16 (ii) no significant effect on intention to apply
Myers, D., Olsen, R., Seftor, N. S., Young, J., & Tuttle, C. (2004: 36) * <i>USA</i>	academic tutoring with cultural program, information and preparation for college	college enrollment	n = 2,292; low-income or first generation high school students (9 th /10 th grade)	(i) + 6 p.p. enrollment at 4 – year institution
Rosinger, K. (2016: 39) * <i>USA</i>	personalized information on financial aid	college enrollment	n = 2,471; pell-eligible students admitted to university from different classes	(i) no significant effect on enrollment
Seftor, N. S., Mamun, A., & Schirm, A. (2009: 41) * <i>USA</i>	academic tutoring with cultural program, information and preparation for college	college enrollment	n = 2,102; low-income or first generation high school	(i) no significant effect on enrollment

USA			students (9 th /10 th grade)	
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p.p.: percentage point;

*: used for calculating the mean ATE of low-SES students (for constructing the reasonable example in subsection 4.2.2);

†: used for calculating the mean ATE of high-SES students (for constructing the reasonable example in subsection 4.2.2)

4.2.1 Simulation within reasonable limits

In the following, we simulate the joint contribution of group-specific ATEs and PRs on TE_{EI} within realistic limits. The literature provides concrete information on group-specific ATEs but little information about PRs.

According to the reported intervention studies, the ATE for high school students with a low socioeconomic background or without an academic background ranges from 0 to 26 percentage points, while the ATE for high school students with a high socioeconomic background or with an academic background ranges from 0 to 17.4 percentage points. Consequently, we vary group-specific ATEs for low-SES and high-SES students within a range from 0 to 20 percentage points for illustration.

Remarkably, no existing study discusses or presents group-specific PRs under real world conditions. Thus, our simulation considers the full range of theoretically possible PRs (0% - 100%).

Selection of scenarios

Although the formula for the TE_{EI} is straightforward, additional specifications are necessary to illustrate the joint influence of group-specific ATEs and PRs. The formula includes four different variables to be considered (ATEs and PRs for each group), which can vary widely; this increases model complexity and yields many potential variations. Therefore, we applied restrictions.

To illustrate the contribution of group-specific PRs against the background of fixed ATEs and, therefore, to address the fallacy of drawing conclusions based solely on group-specific ATEs without considering group-specific PRs, we provide a first set of scenarios where ATEs are constant while PRs vary. To visualize the contribution of group-specific ATEs against the background of fixed PRs, we also provide a second set of scenarios in which ATEs vary with fixed group-specific PRs. This set shows that drawing conclusions based exclusively on group-specific PRs without considering group-specific ATEs is also susceptible to error. Each set of scenarios contains three simulations. Table 2 provides an overview of the specifications used for all six scenarios.

Table 2: Overview of specifications of the simulation

	ATE	PR (%)
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Scenario 1	low SES	+ 17 p.p.	*
	high SES	+ 12 p.p.	*
Scenario 2	low SES	+ 7 p.p.	*
	high SES	+ 7 p.p.	*
Scenario 3	low SES	+ 12 p.p.	*
	high SES	+ 17 p.p.	*
Scenario 4	low SES	*	70
	high SES	*	30
Scenario 5	low SES	*	50
	high SES	*	50
Scenario 6	low SES	*	30
	high SES	*	70

* Values vary within the defined range.
p.p. = percentage points

For the scenarios with fixed ATEs (first column of tab. 2), we define the first scenario as being advantageous for low-SES students with higher ATEs applied to this group compared to high-SES students. In the second scenario, ATEs are equal across groups. In the third scenario, the situation is more beneficial to high-SES students.

The second set of scenarios with fixed PRs (second column of tab. 2) follows the same logic as specifications of the first set. Thus, scenario 4 describes a situation in which low-SES students participate to a higher extent than high-SES students; scenario 5 assumes equal participation across groups; and in the last scenario, socially privileged students are overrepresented.

Simulation results

The results are displayed graphically (Fig. 1). Each of the graphs presents the outcomes calculated by the equation for TE_{EI} (4.7). We dichotomized TE_{EI} . To illustrate which combinations lead to either reductions or increases in educational inequality, positive outcomes (reduction in inequality due to intervention) are denoted with unshaded areas, and negative outcomes are denoted with shaded areas (increase in inequality due to intervention).

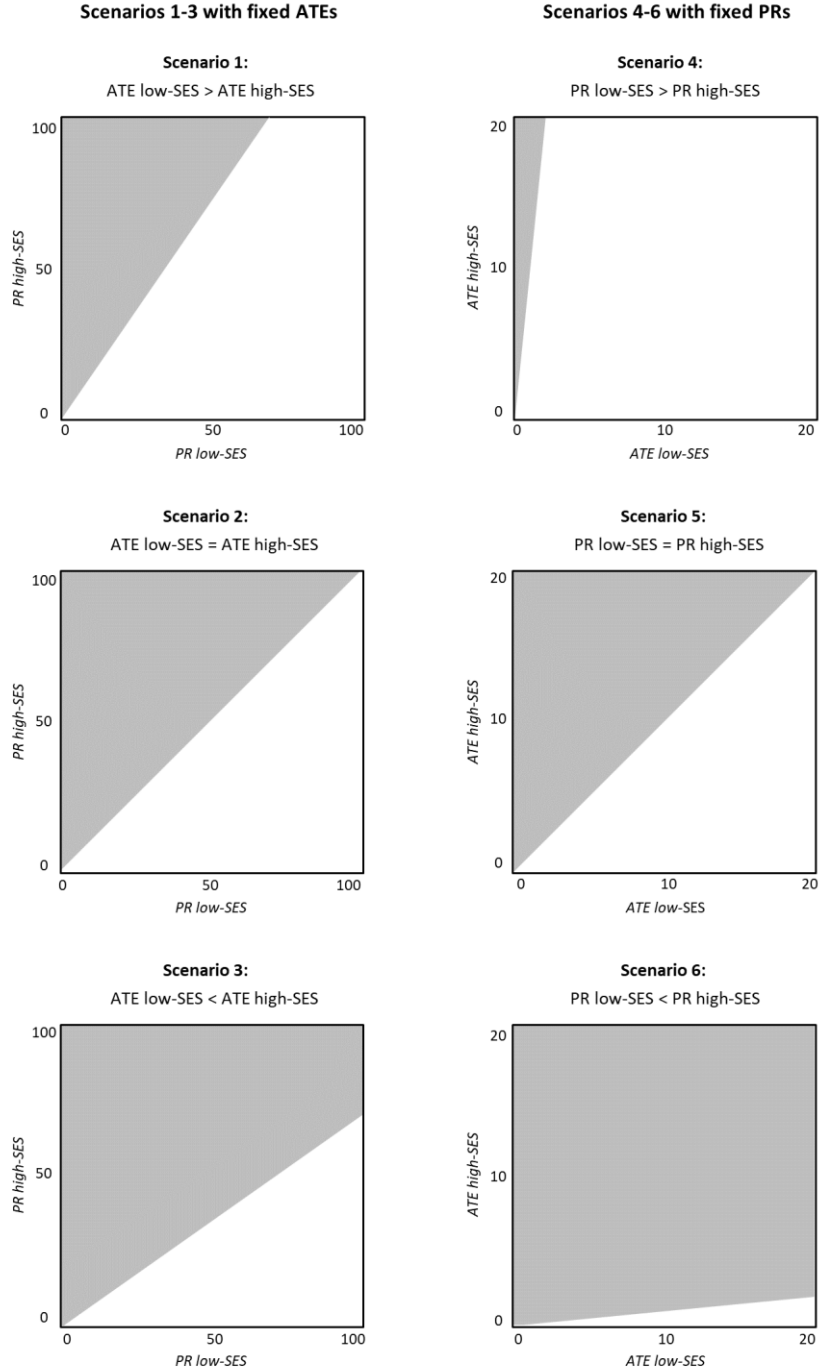


Figure 1: Simulation within reasonable limits: TE_{EI} dependent on group-specific ATEs and PRs

ATE = average treatment effects; PR = participation rate; shaded areas = educational inequality increases, unshaded areas = educational inequality decreases

The results illustrate the joint contributions of group-specific ATEs and PRs to TE_{EI} . Conclusions about inequalities based solely on the comparison of group-specific ATEs (or solely on comparisons between group-specific PRs) are thus shown to be prone to error. However, the significance of the parameters varies across conditions.

For the first set of scenarios, the contribution of varying PRs against different combinations of fixed group-specific ATEs are illustrated. Under the first scenario, the intervention is more beneficial for low-SES students than for high-SES students in terms of group-specific ATEs

($ATE_{low-SES} = 17$; $ATE_{high-SES} = 12$). Ad hoc conclusions on TE_{EI} based on group-specific ATEs suggest that this program reduces inequality. However, the ratio of the shaded area (increase in inequality) to the unshaded area (decrease in inequality) indicates ambiguity that depends on group-specific PRs. Even though a program with this combination of group-specific ATEs would lead to a decrease of inequality in most cases of the considered group-specific PR-range (i.e., in approximately 65% of cases⁷), it frequently would have the opposite effect of enhancing disparities (i.e., in approximately 35% of cases). Therefore, under this condition of group-specific differences in ATEs, group-specific PRs should be considered cautiously in evaluation and/or program implementation if the treatment aims to decrease disparities.

Under the second scenario, an ad hoc comparison of group-specific ATEs suggests that this intervention does not influence disparities due to the equality of group-specific ATEs. However, the opposite is true: only with equal PRs across groups would this program not affect disparities. In all other cases, the program either enhances or reduces inequality depending on group-specific PRs. Since the shaded and unshaded areas of the figure are equal in size, PRs are most important in predicting disparities in cases involving equal ATEs across groups.

For the third scenario, an ad hoc comparison of group-specific ATEs suggests that this program increases inequality ($ATE_{low-SES} = 12$; $ATE_{high-SES} = 17$). However, this is not always true, as this program would reduce disparities when low-SES students are overrepresented. For example, when 25 % of high-SES students take part in the intervention, the share of low-SES students participating should exceed 35 % to decrease disparities.⁸ Therefore, when evaluating the program, researchers should not immediately assume that the program enhances disparities, regardless of participation. It might even be sensible to advise implementation of this program if group-specific PRs can be regulated sufficiently so that PRs compensate for group-specific differences in ATEs and if the program is, for example, cost-effective.

For the second set of scenarios, the impact of varying group-specific ATEs against the background of fixed PRs is illustrated. Therefore, the fallacy of drawing conclusions based exclusively on group-specific PRs is visualized. Even though such ad hoc conclusions might not be as widespread as conclusions based on comparing group-specific ATEs, sometimes it is assumed, that a program reduces disparities based solely on its delivery to areas that are on average socially disadvantaged. This involves implicit assumptions about the impact of group-specific PRs on TE_{EI} . However, in the following scenarios, it becomes apparent that conclusions made based on PRs alone are also susceptible to error.

The fourth scenario is highly advantageous to low-SES students in terms of PRs ($PR_{low-SES} = 0.70$; $PR_{high-SES} = 0.30$). However, even such an enormous overrepresentation does not always lead to a reduction of inequality; group-specific ATEs must still be considered. When, for example, the ATE for high-SES students is 20 percentage points, the ATE for low-SES students should exceed approximately 9 percentage points to prevent an increase in inequality.⁹ Otherwise, this program will increase disparities.

⁷ Percentages can be derived by applying the following equation: $1 - \left(\frac{ATE_{high-SES}}{ATE_{low-SES}} * 0.5 \right)$

⁸ This result is obtained after transforming the equation for TE_{EI} (4.7) and inserting the discussed parameters. In subsection 4.2.2, we show how the parameter can be derived in more detail.

⁹ Again, this result is obtained after transforming the equation for TE_{EI} (4.7) and inserting the discussed parameters.

The fifth scenario duplicates the second scenario. With group-specific PRs being equal, group-specific ATEs prove to be most important in influencing inequality. In other words, equal representation of groups does not guarantee that the program has no impact on disparities. The opposite is true: with ATEs for high-SES students being slightly higher than for low-SES students, this program will in fact enhance disparities. Conversely, with slightly higher ATEs for low-SES students than for high-SES students, the program will reduce disparities.

The sixth scenario is highly advantageous to high-SES students in terms of PRs ($PR_{\text{low-SES}} = 0.30$; $PR_{\text{high-SES}} = 0.70$). Even with this enormous overrepresentation of high-SES students, the program will reduce disparities with large group-specific differences in ATEs that favor low-SES students. When, for example, the ATE for high-SES students is 5 percentage points, the ATE for low-SES students should be higher than approximately 12 percentage points so that this intervention mitigates disparities. Therefore, conclusions based solely on PRs are also susceptible to error in this case. From the perspective of policy advisers, the program might be worthwhile consideration, if the difference between group-specific ATEs is large enough to compensate for differences in group-specific PRs and if, for example, the program is cost-effective involving low costs for treating privileged students.

Considered together, these scenarios show the joint contributions of group-specific ATEs and PRs, and provide a clear pattern of the varying impact of group-specific PRs that depends on differences in group-specific ATEs. The effect of group-specific PRs on TE_{EI} is most severe when ATEs are equal across groups and decreases when differences between group-specific ATEs increase. Thus, similarity in group-specific ATEs can be compensated for by differences in group-specific PRs, and the PRs' ability to compensate declines as differences in group-specific ATEs increase. Accordingly, when very pronounced differences between group-specific ATEs exist, ad hoc conclusions about a program's effect on inequality based exclusively on a comparison of group-specific ATEs are close to the true TE_{EI} . However, in cases of group-specific ATEs being not extremely different, which is presumably true for many educational programs, PRs must be considered. Conversely, the same applies to the varying impact of group-specific ATEs that depends on the difference between group-specific PRs.

In constructing these scenarios, we used arbitrary values within a reasonable range to illustrate general patterns. To discuss a specific example in more detail, we provide a fictitious yet realistic example in the next section.

4.2.2 A fictitious example

In the following, we construct a fictitious yet realistic example of an educational intervention, and illustrate in detail how the impact of this educational program on educational inequality can be evaluated by applying the formula for TE_{EI} .

For illustrative purposes we assume that a guidance counseling program, which aims to foster students' transitions to higher education, is evaluated. This fictitious program is delivered to high schools with academic tracks. It does not target schools in areas that are socially disadvan-

tagged on average but is spread across different regions. However, low-SES students are especially encouraged to participate, since the program aims at reducing inequality. Program participation is voluntary, time-consuming and open to every interested student. Rough estimations of the program's distribution indicate that around one fifth of the population of high school students (regardless of social origin) participate in the program; the intervention is thus widespread. The main research question is whether this intervention reduces inequality in the transition to higher education. An RCT has been conducted, which has provided estimates for group-specific ATEs on university enrolment. For a well-informed guess on what estimates might have been obtained, we refer to previous research.

Derivation of reasonable point parameters: mean values

As reasonable point parameters, we calculated the mean value for each group-specific ATE based on reported group-specific ATEs in the literature. To reduce the heterogeneity of considered studies, we included results on the ATE for applying for higher education or enrolment at any higher education institution or at a 4-year institution. Studies that did not report group-specific effect sizes were not included.¹⁰

For calculating the mean ATE for low-SES students, 15 studies with 17 group-specific ATEs were considered.¹¹ Group-specific ATEs ranged from 0 to 26 percentage points, and the mean was calculated to be 6.6 percentage points. For calculating the mean ATE for high-SES students, 5 studies were included that provided 7 group-specific ATEs.¹² The ATE for high-SES students ranged from 0 to 17.4 percentage points, and the mean was 2.5 percentage points.

Therefore, we assume that the group-specific ATE for low-SES students (6.6 p.p.) is considerably higher than the group-specific point estimate for high-SES students is (2.5 p.p.).

Estimating the critical point of group-specific PRs

As group-specific ATEs differ and favor low-SES students, the fictitious program appears to be promising for reducing educational inequality. A simple ad hoc comparison of group-specific ATEs clearly suggests that the program mitigates disparities.

However, as educational disparities are situated on the macro level, group-specific PRs must be considered carefully. The program would reduce disparities markedly if low-SES students were overrepresented in the program under real world conditions. Furthermore, a reduction of disparities in university enrolment could be expected if group-specific PRs were equal. However, whether low-SES students are at least equally represented in the fictitious program can be questioned on several grounds.

¹⁰ Even after application of further exclusion criteria on the previously reported literature (tab. 1), the included studies remain too heterogeneous for meta-analysis in terms of the type of investigated interventions and primary outcomes. Therefore, we did not apply the common standard of meta-analyses in calculating the mean values. We did not weight reported effect sizes by sample size and we included non-significant effect sizes into mean calculation as being 0. By this rather rough calculation of mean values, we aim at avoiding the impression of precision, which cannot be supported against the background of the heterogeneity of included studies. Accordingly, the provided example is fictitious yet realistic.

¹¹ Studies are marked with * in table 1.

¹² Studies are marked with + in table 1.

First, on average, low-SES groups take part less frequently in beneficial programs than high-SES groups do (Avery et al., 2014; for inequalities in access to (preventive) health programs: e.g., Ahmed et al., 2010; Hoeck et al., 2014; Palencia et al., 2010; Veugelers & Yip, 2003; Zackrisson et al., 2003). Second, no institutional strategies are implemented in program delivery that strongly foster the participation of low-SES students and thereby could help to overcome possible disadvantageous self-selection processes of low-SES students. The program neither targets areas that are on average highly disadvantaged, nor is individualized program access strictly regulated by social origin. Third, further indications that PRs might not be at least equal across groups might exist. For example, some counsellors responsible for program delivery might have reported high-SES students being especially interested in program participation, since the program not only supports transition to higher education but also fosters success within higher education, which appears to attract high-SES students in particular.

These considerations and reports might lead to the reasonable concern that self-selection processes yield unequal representation across distinct social groups, which could undermine the program's promising appearance based on group-specific ATEs. However, at first glance, it is not possible to establish how strong a possible overrepresentation of high-SES students must be to reverse the promising intervention to instead enhance inequality in university enrolment. Thus, the critical point of group-specific PRs at which the effect on disparities is reversed cannot be determined intuitively.

This critical point can be established by applying the final formula of our derivation (4.7). The above defined values for group-specific ATEs ($ATE_{low-SES} = 6.6$; $ATE_{high-SES} = 2.5$) can be inserted into the formula. TE_{EI} should be set to zero, as this indicates the point at which a program's impact on inequality is reversed:

$$0 = 6.6 * PR_{low-SES} - 2.5 * PR_{high-SES} \quad (4.8).$$

By converting the formula to $PR_{high-SES}$, the critical point can be determined:

$$PR_{high-SES} = \frac{6.6}{2.5} * PR_{low-SES} = 2.64 * PR_{low-SES} \quad (4.9).$$

The effect of the fictitious program is thus reversed to enhancing inequality if the PR of high-SES students is more than 2.64 times as high as that of low-SES students. When, for example, 10% of low-SES students participate in the fictitious program, the PR of high-SES students must be lower than 26.4% for the program to still reduce inequality (for an illustration of this critical point, see cross in fig. 2).

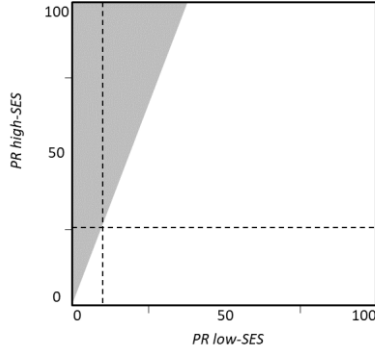


Fig. 2: Results of a fictitious example: TE_{EI} dependent on group-specific PRs for fixed ATEs

ATE = average treatment effects, PR = participation rate; shaded areas = educational inequality increases, unshaded areas = educational inequality decreases

Having identified this critical point, it should be carefully considered whether such a high overrepresentation of high-SES students is possible in the fictitious intervention. On the one hand, some aspects suggest high-SES students to be overrepresented (see above). On the other hand, the degree of overrepresentation of high-SES students needed to reverse the program's effect is high. However, there is insufficient research on group-specific PRs in similar educational programs to determine realistic patterns of group-specific representation. Thus, the possibility of a high overrepresentation cannot be ruled out and, therefore, an empirical investigation of real world group-specific PRs in the fictitious program should be performed.

Further empirical conclusions on TE_{EI} and thus policy implications depend highly on the empirical findings of group-specific representations. If group-specific PRs approximate the critical point, then the program likely will not mitigate disparities under real world conditions. In this case, those responsible for program implementation should introduce procedures that regulate program access to favor low-SES students. Conversely, if participation under real world conditions proves to be at least approximately equal across distinct social groups, the program likely will reduce disparities in transition to higher education against the background of all collected empirical evidence. However, even with equal participation, policy advises might still suggest regulating program access based on social origin, since the program's potential to reduce disparities is not fully realized with equal representation.

4.3 Challenges and assumptions

The final formula for TE_{EI} (4.7) and thus the simulation are a simplified model that illustrates the joint contributions of group-specific ATEs and PRs on the treatment effect on disparities. In the formula, (i) challenges in point estimations of ATEs and PRs are not considered, and (ii) the equation is based on several assumptions that are specific to the estimation of a program's effect on educational disparities as proposed by us.

Challenges in point estimation

The application of the equation assumes that highly reliable point estimations for group-specific ATEs and PRs are available. However, such an expectation simplifies parameter estimation.

Point estimators are subject to uncertainty. Therefore, research on ATEs provides point estimators for this parameter and confidence intervals (CI) indicating possible deviations of the true population's value from its point estimator. Similarly, the determination of PRs is uncertain when, inter alia, PR estimation is sample-based or when data collection on SES-status on the population level is not highly reliable. Even though these uncertainties are ubiquitous in social research, multiple uncertain parameters might lead to inconclusive results regarding the treatment effect on disparities, which challenges the empirical usefulness of the proposed formula.

The following illustration exemplifies challenges that may occur when multiple uncertain point estimators are present. Let us assume that it is of interest under which conditions of varying PRs a program enhances or reduces educational inequality against the background of estimated group-specific ATEs. Due to the uncertainty of point estimators for the ATEs, the consideration of their CIs is sensible. Depending on the width of CIs and the difference between group-specific ATEs, inconclusive results about the importance of PRs for TE_{EI} might occur. Combinations most beneficial to low-SES students and least beneficial to high-SES students (i.e., higher CI-bound for low-SES students, lower CI-bound for high-SES students) might indicate that the program reduces inequality in nearly all cases of varying group-specific PRs, whereas opposite combinations (i.e., lower CI-bound for low-SES students; higher CI-bound for high-SES students) might suggest that PRs are crucial for the effect on disparities. Similarly, results might be inconclusive when TE_{EI} is predicted with uncertain point estimators for all relevant parameters (i.e., group-specific ATEs and group-specific PRs).

The usefulness and manageability of the final formula in terms of making clear statements thus depends on the pattern of research findings on group-specific ATEs and PRs. Future research will show how frequently researchers find the application of the formula with consideration of the CIs to be useful in estimating TE_{EI} .

Assumptions

The derivation of the equation and, therefore, the simulation rest upon several assumptions that are specific to the estimation of a program's effect on educational disparities as proposed by us.

In general, the external validity of ATEs rests on the assumption that the potential outcome under the condition of no treatment (\bar{Y}^0) and the difference between potential outcomes with and without treatment ($\bar{Y}^1 - \bar{Y}^0$) are equal between RCT subjects studied in the trial and participants under real world conditions. In the derived equation, we assume externally valid estimates for group-specific ATEs and, therefore, presume the corresponding assumptions to be correct. However, the formula rests upon the following three additional assumptions:

- 1) Within SES-groups, the potential outcome given no treatment (\bar{Y}^0) is assumed to be equal not only between RCT subjects studied in the trial and real world participants, but also for persons not participating in the intervention under real world conditions. We did not introduce variations of \bar{Y}^0 for these different groups, which would have altered the derivation. This assumption is violated when, for example, low-SES students who participate in the program under real world conditions are positively selected in learning motivation in comparison to low-SES

students not participating in the program, and learning motivation is a covariate of transition to higher education.

2) Connected to the presumed equality of \bar{Y}^0 across groups, is the assumption of \bar{Y}^0 being stable across varying levels of PRs. Therefore, the derivation of the formula does not include variations of \bar{Y}^0 as a function of PRs. Thus, the equation assumes equality of \bar{Y}^0 between participating and non-participating persons on average, regardless of how many persons participate in the program. This assumption is violated when scaling up an intervention leads to the participation of students who are the least motivated to participate in the program, and motivation is a covariate of the transition to higher education.

3) The derivation of the equation rests upon a further assumption that concerns the difference between potential outcomes with and without treatment ($\bar{Y}^1 - \bar{Y}^0$). Within SES-groups, $\bar{Y}^1 - \bar{Y}^0$ is assumed to be invariant across varying levels of PRs. In other words, externally valid estimates of group-specific ATEs are assumed to be independent of PRs. Accordingly, the final formula does not model group-specific ATEs as a function of varying group-specific PRs.

The third assumption of PR-invariant externally valid ATEs can be violated by different factors: (i) *variations in persons*: When, for example, an intervention becomes part of the school curriculum and accordingly targets the whole subpopulation of low-SES students, the students who are the least motivated to take part are involved in the treatment. This subgroup might not only differ on the potential outcome given no intervention from students that have been involved thus far (which concerns PR-invariant \bar{Y}^0 across groups, see second assumption), but these students might also react quite differently to the program than students who have participated thus far, which would involve changes of externally valid ATEs dependent on PRs. (ii) *variations in setting*: In a similar way, variations in settings might occur based on varying PRs and may affect the external validity of ATEs. Scaling up a school-based intervention involves implementation at schools that are least motivated or capable to deliver it. In these schools, a program's effect on educational attainment might differ from those observed in other settings based on, for example, limited resources, which may again affect the external validity of the ATE. (iii) *variations in treatment*: Furthermore, variations in treatment might occur after a program has been scaled up, leading to changes in externally valid ATEs. For example, when the PR rises intensively, the overall quality of program delivery might decline. Such a change in treatment might attenuate the program's impact on educational attainment. All of the mentioned PR-driven changes in externally valid ATEs might influence ATEs of various social groups equally or heterogeneously.

The extent to which assumptions are violated is an empirical rather than a theoretical question. Due to limited research, it is currently not possible to determine the magnitude of the bias for specific cases. The severity of the bias might vary across different types of interventions and educational outcomes, and might be larger in extreme cases involving very high or very low PRs and correspondingly lower on moderate PR levels. The simulation showed theoretically that group-specific PRs and ATEs jointly influence the TE_{EI} to a not negligible extent. Therefore, to enhance precision in estimating the treatment effect on disparities, we stress the im-

portance of further research on (i) empirical variations of \bar{Y}^0 for participating and nonparticipating groups, (ii) empirical variations of \bar{Y}^0 across different PR-levels under real-world conditions, (iii) and variations of $\bar{Y}^1 - \bar{Y}^0$ for different PR-levels.

5. Discussion

Against the backdrop of defining educational disparities as unequal chances for distinct social groups to achieve educational success at the macro level, we suggest methodological refinements for scrutinizing the impact of interventions on inequality. We mathematically derived and showed through a simulation that group-specific ATEs and PRs contribute significantly and jointly to the treatment effect on inequality.

Thus, we highlight that ad hoc comparisons of group-specific ATEs are frequently insufficient when conclusions about the impact of an intervention on disparities are drawn. Group-specific PRs do not only influence the extent to which educational inequality is shaped by an educational program, but a program's impact on disparities can be even reversed based on the pattern of group-specific participation. Accordingly, ad hoc conclusions based on a comparison of group-specific ATEs might not only under- or overestimate a program's effect on disparities, but they might even suggest an inaccurate direction of the impact. Therefore, we hope that group-specific PRs will henceforth be considered by researchers, practitioners, and policy makers as a major predictor of educational disparities.

We offer a simple mathematical equation that describes how group-specific ATEs and PRs contribute jointly to the treatment effect on inequality. However, the proposed equation is a simplification in two regards. (i) The equation simplifies the problem of parameter estimation by ignoring the fact that point estimators are uncertain estimations of true values, which is typically reflected by confidence intervals. Future research will show whether the application of the equation proves itself manageable and useful for evaluating specific interventions when confidence intervals are additionally considered. (ii) Furthermore, the equation rests on certain assumptions regarding the stability of group-specific ATEs across varying levels of group-specific PRs *inter alia*. Due to a lack of research, we do not know how strong results coming from the formula are biased due to violated assumptions. To gain more precision in estimating the treatment effect on disparities, we encourage further research *inter alia* on how group-specific ATEs vary depending on group-specific PRs.

However, even against these uncertainties, we still recommend applying the equation as an approximation of the impact of interventions on inequality based on group-specific ATEs and PRs because PRs are shown to influence educational disparities considerably under certain conditions. As discussed in detail, this is true for programs (i) where a certain share of distinct social groups participates in the intervention under real world conditions and (ii) that foster the

educational attainment of each social group to some extent with group-specific differences between ATEs not being extremely large. Researchers can use the equation to estimate the effect of both on-going and future interventions.

The impact of on-going programs on disparities can be estimated by applying the empirical values for group-specific ATEs and PRs. In this case, PRs should be determined from data that reliably indicate social origins of persons who participate in a program under real world conditions and the share of distinct social groups in the population. Researchers can also obtain rough predictions of the potential impact with varying PRs after having established group-specific ATEs by means of an RCT. We are convinced that both scientific evaluations of the actual effect and recommendations for policy-making should frequently be based on estimations that consider PRs. Presumably the consideration of group-specific PRs will in many cases lead to recommending that program access should be (heavily) restricted to low-SES students when the program aims at reducing inequality.

The results of this paper do not provide information about the present impact of educational programs on disparities. We applied only reasonable limits on group-specific ATEs based on heterogeneous studies and did not derive reference values for group-specific PRs because we did not find any research on actual patterns of representation based on social origin. Similarly, the provided example was realistic yet fictitious. Only if establishing PRs becomes a new standard in the conduct of evaluations in the field of educational disparities will future research be able to expand our theoretical reflections and thereby indicate the empirical impact of programs targeting educational disparities.

We stress the importance of a cautious investigation of the actual contributions of group-specific PRs to disparities, as PRs can function as a mechanism that exacerbates disparities against the backdrop of introducing educational programs that appear promising on the surface. At least two issues call for thorough empirical investigation.

First, there is reason to believe that PRs are unequal across groups. Discussions on guidance counselling in the USA indicate that low-SES students have in fact the least access to beneficial programs due to financial barriers at the school level, which are most severe in schools attended by socially disadvantaged students (Avery et al., 2014). Since the social composition of participants under real world conditions likely does not only depend on the average SES in schools or districts but might also be influenced by monetary and nonmonetary costs of participation or by a program being voluntary or mandatory, social differences in access to educational interventions might exist for various programs in various countries.

Second, researchers have described a variety of mechanisms that have stabilized educational inequalities against a backdrop of promising institutional modifications in the past. For example, in the wake of educational expansion, which could have reduced inequalities, new horizontal differentiations were established that helped to maintain inequalities, a phenomenon known as effectively maintained inequality (Lucas, 2001). Another example concerns the introduction of standardized eligibility tests aimed at reducing disparities in the USA. However, this standardization led to a rise of cost-intensive preparation courses that mainly affluent students can

afford (Alon, 2009). These examples describe unexpected adaptive mechanisms and consequential unintended effects and, therefore, are not directly comparable to a potential PR-driven rise of disparities. However, the examples underline the persistence of disparities within a background of promising institutional changes and, therefore, stress the need to investigate whether the implementation of promising programs actually comes with a PR-driven exacerbation of inequalities.

Conclusions based exclusively on group-specific PRs and ATEs are, of course, limited. A program is implemented within a wider educational system that might be modified through program implementation and that might itself respond to changes stemming from large-scale interventions. Researchers cannot predict these modifying processes by combining group-specific ATEs and PRs alone, and yet these processes might influence disparities considerably. For example, when a program that enhances university enrollment is scaled up, the actual effect may be less impactful than expected based on group-specific ATEs and PRs due to institutional modifications in reaction to the program's effect of rising transition to higher education (for example, universities might ration seats in reaction to rising transition rates). Socially privileged classes might react to an intervention-driven rise of educational success among lower social classes in a competitive way, aiming at maintaining their relative advantage. Such reactions to changing realities are hardly empirically predictable.

Finally, we can imagine that the above-discussed PR-driven mechanism of enhancing inequality works in many areas. From a methodological viewpoint, there is no reason to believe that the joint contributions of group-specific ATEs and PRs to inequality are limited to educational programs. For example, inequalities in the labor market might be driven by specific combinations of group-specific ATEs and PRs of job programs that support unemployed persons in finding jobs or of training programs for employees. Therefore, we hope that the suggested methodological refinements will not only be considered in research on educational disparities but also in other research fields evaluating the impact of various programs on inequality.

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