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Do educational reforms increase or decrease health inequalities: A matter of methods?

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ABSTRACT

Evaluating whether social policies reduce health inequalities is complicated by the fact that these upstream determinants may also change the socioeconomic distribution. Failure to account for these compositional changes may severely bias the effect estimation procedure. In this article, we illustrate how a health inequality impact assessment of a policy that (also) changes the socioeconomic distribution may produce biased results. First, we show why analyses that do not account for compositional changes fail to estimate the correct counterfactual outcome. This problem most notably occurs when using repeated cross-sectional data, often the only available option to evaluate the health effect of large-scale policies. Second, we conducted a microsimulation study to estimate the magnitude of the bias under various conditions. The results showed that the actual impact of the policy on health inequalities is often underestimated and may even produce results that are in the opposite direction of the actual causal effect of the policy. Future studies should explore new strategies, such as simulation methods, to assess the impact of policies that (also) cause changes in the socioeconomic composition of the population, to enable researchers to accurately estimate their effect on health inequalities.

1. Introduction

Despite ongoing efforts to reduce health inequalities in Western societies, the scientific evidence-base for effective measures to tackle socioeconomic inequalities in health is still limited. This is usually attributed to the fact that little is known about the effects of macro-level determinants of health, such as social policies and institutions (Braveman et al., 2011; Lorenc et al., 2013; Petticrew et al., 2004). While these upstream determinants probably have the greatest potential to reduce health inequalities, changes in these determinants are also hard to evaluate. They require quasi-experimental methods and adequate control groups for assessing causal effects (Basu et al., 2017; Craig et al., 2017).

Assessing the impact of social policies on socioeconomic inequalities in health (i.e. the equity impact) is complicated by the fact that these upstream determinants may also define the nature of stratification in a society. For example, policies and programs that reduce the number of early school leavers, allocate subsidies to low income groups, or change the labor market, may substantially improve population health by lowering the proportion of people exposed to disadvantageous social positions. Obtaining an accurate assessment of the equity impact of these policies requires that evaluation studies factor in these socioeconomic shifts (Harper and Lynch, 2006). Failure to do so, may severely bias the results and even produce results that are in the opposite direction of the actual causal effect of the policy.

An increasing number of studies propose to tackle health inequalities not by using traditional health care reforms, but by relying on educational reforms (Cohen and Syme, 2013; Low et al., 2005; Walsemann et al., 2013). To evaluate whether these policies actually have an impact on educational inequalities in health, one needs to disentangle the direct health effects of these policies from the health effects that occur via increasing social mobility. As studies often fail to distinguish these effects, they might be subject to bias. For example, a recent study used a difference-in-differences design on repeated cross-sectional data to investigate the effects of comprehensive school reforms (increasing the age of early tracking) on educational inequalities in self-rated health (Delaruelle et al., 2019). They found that middle and high educated had better, and early school leavers worse health after the reform, and interpreted this as evidence that comprehensive school reforms were unable to reduce educational inequalities in health. However, these

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findings could also be the result of increased educational mobility: comprehensive schooling reforms reduce the probability that children drop out of school early (Van de Werfhorst and Mijs, 2010), which may positively affect their health. Since repeated cross-sectional data are not able to factor in compositional changes in the population, the true equity impact of the reform remains unknown. In the next section, we illustrate why an analysis that does not account for compositional changes fails to identify the causal effect of a policy on health inequalities. Subsequently, we use microsimulations to estimate the magnitude of this bias under various conditions.

2. Assessing the equity impact of policies that change the socioeconomic distribution

Imagine a hypothetical population consisting of 8 individuals, 4 of which are less-educated and 4 of which are more-educated (Table 1). In this population, the less-educated have a mean life expectancy of 73 years, whereas the more-educated have a mean life expectancy of 82 years. Let's assume that at one particular point in time, the members of this population decide that it would be beneficial if a higher proportion of them would be more-educated. In order to achieve this, they implement a particular policy that encourages more students to attain a higher educational degree. Although not the primary goal of the policy, someone suggests that the policy may also be an effective strategy to tackle health inequalities. How would one decide whether or not this policy was (also) an effective means to reduce educational inequalities in health (i.e. whether or not the policy had an equity-positive impact)?

To illustrate our argument, imagine that we know the pre-policy and post-policy life expectancy from all individuals in this hypothetical population. Also imagine that their life expectancy would not have changed if the policy had not been implemented. This is known as their potential outcome or counterfactual outcome: the outcome that would have been observed if, counter to the fact, the policy had not been implemented (Hernan, 2004). Table 1 shows that most individuals were not affected by the policy: the pre-policy life expectancy of Hector, Laodice, Paris, Andromache, Aeneas and Glaucus is equal to their post-policy life expectancy. However, Polydamas and Cassandra were affected by the policy: their life expectancy increased by 4 years because they became more-educated.

Comparing the pre-policy and post-policy life expectancies shows that the policy caused a 1-year increase in mean life expectancy (Table 1). Moreover, this gain in life expectancy occurred exclusively among (initially) less-educated individuals (Polydamas and Cassandra). Whereas the difference in life expectancy between the less and more-educated was 9 years before the policy was implemented, it was only 7 years after the policy had been implemented (Table 2). Hence, the equity impact of the policy was a 2-year reduction in the difference in life expectancy between those with a (pre-policy) low level of education and those with a (pre-policy) high level of education.

Table 1
Educational level and life expectancy of all individuals in the hypothetical population.

	Educational level (pre-policy – post-policy[a])	Pre-policy life expectancy	Post-policy life expectancy[a]	Policy effect on life expectancy
Hector	Low – Low	70	70	0
Laodice	Low – Low	72	72	0
Polydamas	Low – High	74	78	4
Cassandra	Low – High	76	80	4
Paris	High – High	80	80	0
Andromache	High – High	82	82	0
Glaucus	High – High	82	82	0
Aeneas	High – High	84	84	0
Mean life expectancy total population		77.5	78.5	1

[a] Counterfactual educational level and counterfactual life expectancy is equal to pre-policy level.

3. Compositional changes lead to ambiguous causal inference

Identification of the causal effect of the policy becomes ambiguous when the compositional changes in educational level caused by the policy are used to 're-classify' the less and more-educated groups: individuals that have obtained a higher educational level due to the policy are classified as less-educated in the pre-policy period and classified as more-educated in the post-policy period (Table 2). In our hypothetical population, this re-classification would result in an observed post-policy life-expectancy of 71 years for the less-educated (now only Hector and Laodice) and 81 years for the more-educated (which now includes Polydamas and Cassandra). Crucially, because the post-policy more-educated group includes individuals who would have remained less-educated in the absence of the policy, the comparison between observed outcomes is no longer made between the same groups of individuals. Calculating the policy effect in this way would lead us to falsely conclude that the policy caused a 1-year increase in the difference in life expectancy between the less and more-educated (Table 2).

Because we are unable to actually observe the counterfactual educational levels, we must rely on a comparison between a population that has been exposed to the policy and a population that has not been exposed to the policy. Doing so, however, also implies that in the exposed population, the observed educational distribution will always include any compositional changes brought on by the policy. Consequently, the actual equity effect of the policy cannot be identified without any correction for these compositional changes. Returning to our hypothetical example: the policy has, in fact, improved the health of the less-educated, which, in this population, is the health of Hector, Laodice, Polydamas and Cassandra. That this result is actually achieved by raising Polydamas' and Cassandra's educational level doesn't negate the fact that – from a counterfactual perspective – implementing the policy caused a decrease in educational inequalities in health.

Note, however, that from a health equity perspective, it may also be relevant to consider how the policy impacts the health gap that remains between those that are not upwardly mobile (i.e. Hector and Laodice) and the others, but this is a different research objective. Since the policy is not targeting any intermediate factor, but rather educational attainment itself, it is much less likely that it will also increase the health of the non-mobile.

4. Bias in difference-in-differences analysis

To further illustrate our argument, we consider the example of a difference-in-differences (DiD) analysis. This estimation procedure is a common approach to evaluate the health effects of social policies (Basu et al., 2017; Saeed et al., 2019; Wing et al., 2018). If a suitable control population is available to fulfil the counterfactual assumptions of the DiD approach, the design allows researchers to estimate the total causal effect of a policy. However, when the equity effect of the policy is of interest, any change in the distribution of socioeconomic position associated with a change in exposure (i.e. the policy) violates the common trends assumption of the DiD model (Stuart et al., 2014). (Note that estimating the equity effect of policies is complicated even further by the fact that it may also induce collider bias (Cole et al., 2010; Elwert and Winship, 2014; Hernan et al., 2004). This bias occurs because conditioning on SES (e.g. by stratifying the analysis) introduces a non-causal association between SES and health by opening up a backdoor path via any unmeasured confounder of this relationship (e.g. parental SES, ethnic background or genetic factors). Because our paper aims to specifically address the problems associated with compositional changes, we assume the absence of collider bias.)

Let y_t^j denote the average life expectancy of treatment group j ($j = 1$ if

Table 2
Causal and observed effect of the policy by level of education.

	Mean life expectancy			Policy effect	
	Pre-policy	Post-policy (based on counterfactual educational levels)	Post-policy (based on observed educational levels)	Causal effect	Observed effect
Low education	73[a]	75[a]	71[c]	2	−2
High education	82[b]	82[b]	81[d]	0	−1
Difference high-low education	9	7	10	−2	1

[a] Hector, Laodice, Polydamas, Cassandra.
[b] Paris, Andromache, Glaucus, Aeneas.
[c] Hector, Laodice.
[d] Polydamas, Cassandra, Paris, Andromache, Glaucus, Aeneas.

treated and $j = 0$ if untreated) at time t. Fig. 1A illustrates how the DiD approach estimates the effect of the policy for the total population. For the sake of simplicity, we only demonstrate the scenario for two time points, but the same argument is applicable when multiple time points are considered. In Fig. 1, the lowest (dashed) line depicts the change in outcome among the (untreated) control group and the highest (solid) line depicts the trajectory among the (treated) intervention group. To estimate the causal effect of the policy, the DiD approach uses the trend in the control group as the unobserved counterfactual trend that would have occurred in the intervention group in the absence of the policy (the dashed line in the middle). Consequently, the effect of the policy is calculated as the difference between the change in the outcome before and after the policy in the intervention group and the change in outcome in the corresponding time period in the control group.

When the same approach is used to estimate the effect of the policy on educational inequalities in health, however, the effect should be separately identifiable for all educational groups considered. For example, if two groups are considered (i.e. 'high SES' and 'low SES'), the policy effect is calculated as the difference in the DiD estimate for the two groups ($\Delta_{\text{inequality}} = \Delta_{\text{high SES}} - \Delta_{\text{low SES}}$). Fig. 2 illustrates how the DiD approach estimates these separate effects and why this approach is invalid if the policy also changes the educational composition of the population. In this scenario, half of the initially-less educated ('low SES') individuals among the intervention group become more-educated ('high SES') due to the intervention. In the control group, however, these compositional changes do not occur. Consequently, the counterfactual outcome for the intervention group (estimated from the trend in the control group) cannot be estimated.

5. Microsimulation model description

The previous paragraphs illustrate that the impact assessments of policies and interventions on socioeconomic inequalities in health may be biased by compositional changes brought on by these same policies and interventions. However, the magnitude of this bias depends on several factors, such as the degree to which the policy is able to cause upward mobility, whether or not this upward mobility is related to health (e.g. are those who are already more healthy also more likely to be upwardly mobile), and the amount of health gained by upward mobility. To estimate how these various conditions affect the results of an impact assessment, we conducted a microsimulation study.

Our microsimulation model followed a synthetic population of 10.000 individuals divided into two educational groups and consisted of two stages. In the first stage, the baseline characteristics of the population were set up. Each member of the population was categorized as either less-educated or more-educated. In the first set of simulations, 20% of the synthetic population was less-educated; in the second set of simulations, 50% of the synthetic population was less-educated, and in the third set of simulations 80% of the synthetic population was less-educated. The more-educated group's life expectancy (LE) followed a normal distribution with mean 84 and standard deviation 13, while the less-educated group's LE followed a normal distribution with mean 79

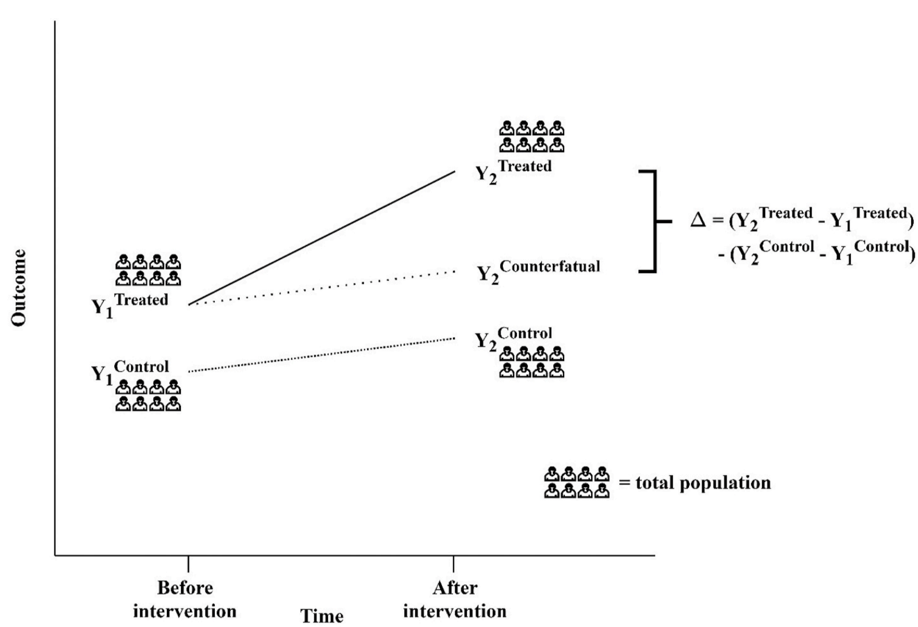

Fig. 1. Illustration of a difference-in-differences approach to estimate the effect of a policy for the total population.

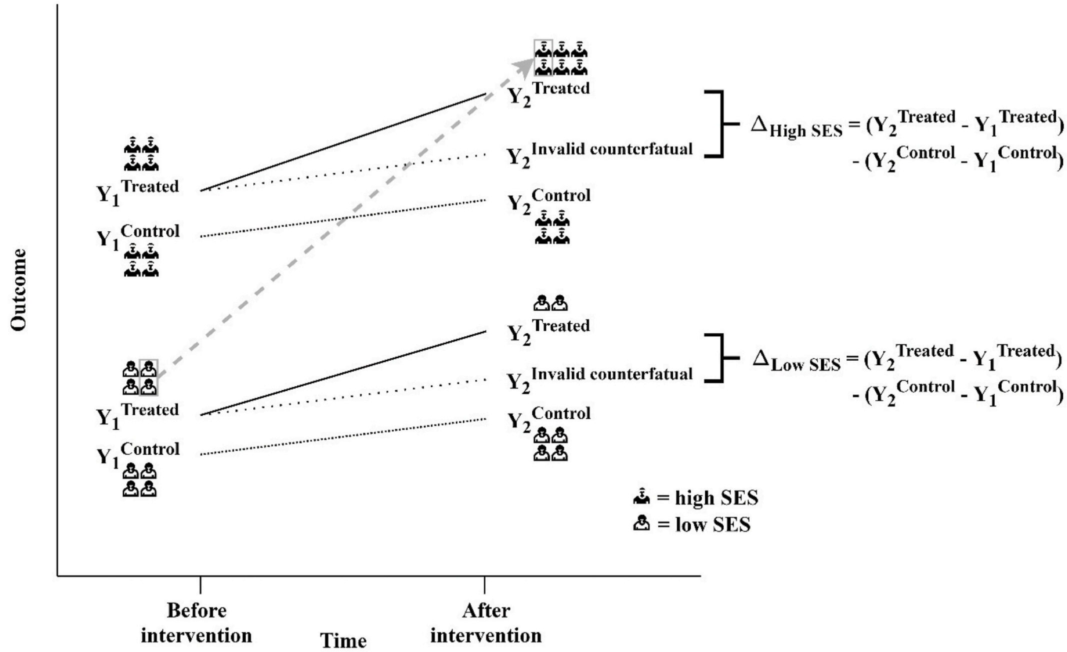

Fig. 2. Illustration of a biased difference-in-differences approach to estimate the effect of a policy for different socioeconomic groups (the grey arrow depicts a compositional change caused by the policy).

and standard deviation 13, reflecting the actual life expectancies of less and more-educated individuals in The Netherlands (Volksgezondheidenzorg.info, 2021). In the second stage, we simulated a policy intervention that caused some of the initially less-educated individuals, to become more-educated. We let the probability of upward mobility (which corresponds to the proportion of initially less-educated individuals that become more-educated due to the policy) range from 0 to 0.40 in steps of 0.02. We chose this wide range of probabilities and small incremental step for the purposes of illustration, rather than specifically targeting one real-life example. We simulated four scenarios:

1) Upward mobility was assumed to be completely random. In other words, those whose educational level is increased by the policy (i.e. the upwardly mobile) had a mean LE of 79 prior to policy implementation (calculated from the first stage of the simulation), similar to those who are not upwardly mobile.
2) Social mobility is often not random, but associated with health (i.e. direct selection) or determinants that are relevant for health (i.e. indirect selection). In other words, those who are already more healthy or have individual characteristics conducive to good health are more likely to benefit from a policy that increases their probability to become more-educated. To investigate the impact of this selection effect we simulated a scenario where the probability of upward mobility was twice as high among less-educated individuals with an above-average LE than among less-educated individuals with a below-average LE. In this scenario, the upwardly mobile had a mean LE of 80.5–81.0 (depending on the probability of upward mobility) prior to policy implementation.
3) We simulated an additional scenario where the probability of upward mobility was three times as high among less-educated individuals with an above-average LE than among less-educated individuals with a below-average LE. In this scenario, the upwardly mobile had a mean LE of 81.5–82.1 (depending on the probability of upward mobility) prior to policy implementation.
4) Although a much less realistic scenario, we also simulated a scenario where the probability of upward mobility was half as high among less-educated individuals with an above-average LE than among less-educated individuals with a below-average LE. In this scenario, the upwardly mobile had a mean LE of 76.3–76.8 (depending on the probability of upward mobility) prior to policy implementation.

Last, we also varied the gain in LE acquired by the upwardly mobile individuals by calculating their post-policy LE as a percentage of the LE of more-educated individuals (randomly estimated from its distribution) and the remainder based on the individual's pre-policy LE. We let the percentage of life expectancy gained via upward mobility range from 0% to 100% in steps of 10% (again using a wide range for illustrative purposes). These parameters correspond to a scenario where there is no causal effect of education on LE (i.e. the gain in LE for upwardly mobile individuals is 0% because becoming more-educated does not increase LE) to a scenario where the association between education and LE is completely causal (i.e. those who become more-educated obtain 100% of the LE of the high educated).

For the sake of simplicity, we assumed that the policy did not cause any downward mobility and only impacted life expectancy via its impact on educational attainment. Making these assumptions underestimates the magnitude of the bias, except for the unrealistic scenarios where downward mobility has a positive effect on health or where the positive (negative) health effect of the policy is stronger (weaker) among those who remain less-educated.

For each simulation, we calculated the actual causal effect of the policy on educational inequalities in LE (based on the pre-policy classification of education only) and the observed effect of the policy if compositional changes in education caused by the policy are used to reclassify low and high educated individuals (based on the post-policy classification of education). We used 1.000 iterations for each simulation.

6. Results

The microsimulations showed that, in most scenarios, the actual impact of the policy on educational inequalities in life expectancy was severely underestimated if compositional changes were not accounted for, and in many conditions even produced results that are in the opposite direction of the actual causal effect of the policy. To illustrate how the different parameters affect the magnitude and direction of the bias, we plotted the results of the microsimulations where 50% of the synthetic population was less-educated and 20% of those were upwardly mobile in Fig. 3. It shows that the difference between the actual causal effect of the policy accounting for compositional changes and the observed effect of the policy not accounting for compositional changes was strongly dependent on the health gain acquired via upward mobility: the greater the actual increase in LE (i.e. the more effective a policy is in reducing health inequalities), the lower the observed change in inequality in LE. In other words, the actual effect and the observed effect are inversely related to each other and depend on the extent to which an increase in education leads to an increase in LE. Moreover, the results from the microsimulation showed that the observed change in inequality in LE may even be in the opposite direction of the actual causal effect of the policy (i.e. show an increase in health inequalities) when upward mobility is positively related to health. Given the high likelihood that policies that increase upward mobility do so especially among those with (characteristics conducive to) better health, it is conceivable that studies may actually conclude that a particular policy increases health inequalities, while in fact it does the opposite. Results from the other microsimulations also showed that the probability of observing an effect in the opposite direction of the actual causal effect is larger when the prevalence of low education is smaller or when the probability of upward mobility is larger. A complete overview of the results of all microsimulations is provided in the Supplementary Material.

7. Discussion

Scholars increasingly recognize the need to address the most upstream determinants of health to effectively tackle health inequalities. Promising strategies to tackle these 'causes of the causes' include the implementation of social policies that directly address people's social and economic opportunities, such as educational policies, social security policies and labor market policies. Evaluating whether these social policies affect health inequalities, however, will be biased if no correction is made for the compositional changes brought on by these policies. This is an especially pressing issue in case of policies that were not specifically designed to decrease health inequalities, but rather to increase social mobility or reduce social inequalities in general. While we used the example of educational reforms and its impact on educational inequalities in health, the same argument applies to, for example, the impact of income redistribution or active labor market policies on income inequalities in health. Whenever the policy under evaluation also affects the socioeconomic indicator that is used in the study, the equity impact assessment may be biased. Results from our microsimulations suggest that this bias may be substantial.

One solution to the problem of compositional changes is to use a socioeconomic indicator that is not affected by the policy: in the case of an educational reform, studies could examine the impact of the policy on health inequalities by parental SES (Ravesteijn et al., 2017). Since parental SES will not be affected by the educational reform, the evaluation study will be not be biased by compositional changes. Following the same argument, studies can examine the impact of social security and labor market policies on educational inequalities in health. However, the downside is that this may also change the study's substantive research question. A different solution would be to use health inequality measures that are able to account for compositional changes in the socioeconomic distribution. The Slope Index of Inequality and the Relative Index of Inequality are often used for these purposes (Harper and Lynch, 2006; Mackenbach and Kunst, 1997), although a recent study suggests that these measures are also not able to sufficiently factor in socioeconomic shifts (Renard et al., 2019). In addition, using these measures does also not allow for pairwise comparisons between specific socioeconomic groups (e.g. early school leavers), because they aggregate information from the entire socioeconomic distribution. Future studies should explore new strategies to assess the equity impact of policies that (also) cause changes in the socioeconomic distribution. Microsimulations, such as the one we used in this study, may be a useful tool to estimate complex and long-term health equity impacts (Abraham, 2013; Epstein, 2008). These models can be recalibrated to different contexts and certain parameters (i.e. the probability of upward mobility) can be quantified to reflect real-life asymmetries and facilitate data provision for effective policy implementation. Finally, these models can also be used to conduct sensitivity analyses and estimate the magnitude of potential biases in real-life applications (Epstein, 2008).

In conclusion, empirical analyses attempting to estimate if a social policy decreases health inequalities may be severely biased if they do not account for compositional changes brought on by the policy. Generally, policies that reduce social inequality are also beneficial from a health equity perspective; we should be careful not to convince ourselves otherwise.

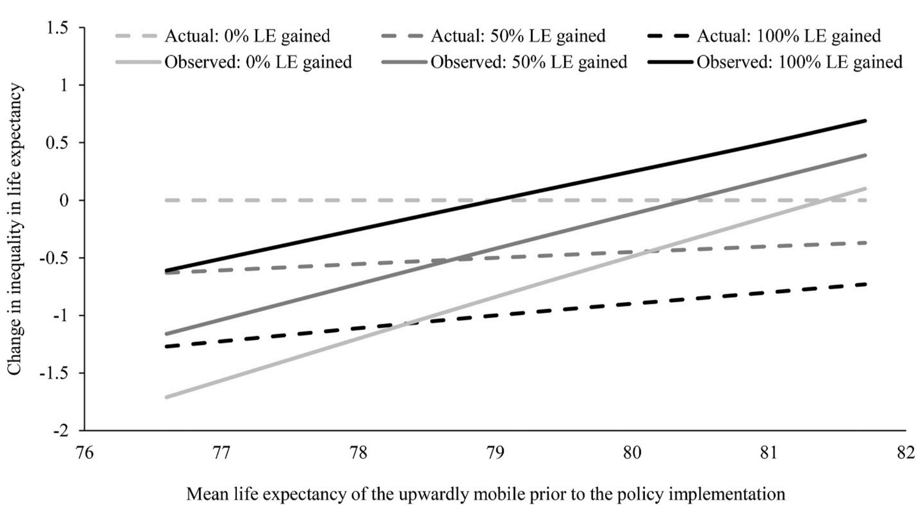

Fig. 3. Estimated changes in inequality in life expectancy in a synthetic population in which 50% was less-educated and 20% of those were upwardly mobile, by mean life expectancy of these upwardly mobile persons prior to the policy implementation. The dashed lines represent the causal effect of the policy accounting for compositional changes (depicted as 'actual'). The solid lines represent the effect of the policy not accounting for compositional changes (depicted as 'observed'). The darkest lines represent the scenarios where the upwardly mobile obtain 100% of the life expectancy of the more-educated, the lightest lines represent the scenarios where the upwardly mobile obtain 0% of the life expectancy of the more-educated. The y-axis displays the estimated change in inequalities in life expectancy (a negative score indicates a decrease in health inequalities). The x-axis displays different LE's of the upwardly mobile prior to the policy implementation (i.e., their mean LE in the first phase of the simulation). This indicates to what extent the probability of upward mobility is related to a person's LE: a score of 79 (similar to the mean LE of all less-educated individuals) indicates that the upwardly mobile had the same mean LE prior to the policy implementation as the other less-educated, a mean LE above 79 years indicates that the upwardly mobile already had a higher mean LE prior to the policy implementation than the other less-educated, and a mean LE below 79 indicates that the upwardly mobile had a lower mean LE prior to the policy implementation than the other less-educated.

Declaration of competing interest

The authors declare that they have no competing interests.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.socscimed.2021.114003.

Authors' contributions

JOG conceived the study. JOG and MKR ran the microsimulation study. All authors contributed to writing the manuscript. All authors read and approved the final manuscript.

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