The Importance of Causality and Pollution Dispersal in Quantifying Pollution Disparity Consequences: Reply to Pastor et al. (2022)

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Abstract

This brief note replies to Pastor et al. (2022). We first summarize two key methodological differences between Hernandez-Cortes and Meng (2022) (HMC) and the Cushing et al. (2018) (CBW) and Pastor et al. (2022) (PMC) papers, in establishing the pollution disparity effects of California's greenhouse gas cap-and-trade (C&T) program. First, HCM use a causal inference framework with regulated and unregulated facilities as treatment and control units, respectively, to estimate the program's effects on emissions. By contrast, PMC and CBW use only C&T regulated facilities, making it difficult to discern whether their results are due to C&T, as claimed, or to confounding factors. Second, causality aside, by not modeling where pollution disperses, PMC and CBW's results are generally uninformative about the program's actual pollution disparities consequences across California. HCM develop an approach that combines causal inference with pollution dispersal modeling to overcome this issue. We also respond to technical critiques made by PMC.

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1 Background

Across the U.S., pollution exposures have been shown to be systematically higher in places where disadvantaged communities reside.[1] A critical question moving forward is whether environmental policies widen or narrow such pollution disparities. In Hernandez-Cortes and Meng (2022) (HCM), we couple a causal inference framework with a pollution dispersal model to quantify the pollution disparity consequences of California's greenhouse gas (GHG) cap-and-trade (C&T) program. We find that air pollution disparities across California from facilities regulated only by C&T narrowed as a consequence of the program.

This methodology contrasts with that of Pastor et al. (2022) (PMC) and their earlier Cushing et al. (2018) (CBW) paper. Section 2 provides a high-level summary of two key methodological differences between HCM and the PMC and CBW papers, notably HCM's inclusion of a control group of facilities and explicit modeling of pollution dispersal. Section 3 responds to technical critiques of HCM made by PMC.

Before starting, we want to acknowledge our intellectual debt to this other team of authors. Indeed, our own research was inspired from reading CBW. From the very beginning, our aim was to get to the bottom of whether the market-driven emissions reallocation under California's C&T program caused pollution disparities to widen or narrow across the state. As empirical researchers, this meant prioritizing the development of a methodology best suited to answering this question, and importantly, one that does not prejudge the answer. We could very well have written a paper showing that California's C&T widened pollution disparities had such a conclusion been revealed by our methodology.

It is in that light that we welcome PMC's data-driven critiques of HCM. While there may be differences between our two efforts, we believe that an open, transparent, and intellectually-rigorous exchange on methodological approaches between researchers offers the most constructive path forward for this defining environmental problem of our time. Indeed, over the last few months we have spent a considerable amount of time interacting with PMC, giving their team our data and code, and in some cases even writing new code to assist in PMC's analysis. It is in that spirit that we strongly believe that the authors on both teams are "on the same side": we all have a shared interest in understanding the true pollution disparity consequences of this important climate policy.

We also emphasize, as we do in HCM, that our approach only tackles one of the many potential distributional concerns regarding the program. Questions remain over how the program may have altered the distribution of health outcomes as well as the distribution of the

[1] Disparities across various pollutants in the U.S. have been documented through case (Bullard, 2000; Bowen, 2002; Ringquist, 2005; Mohai, Pellow and Roberts, 2009; Banzhaf, Ma and Timmins, 2019) and population-level (Colmer et al., 2020; Currie, Voorheis and Walker, 2020) studies.

program's cost burden, including changes in energy prices and wages. Similarly, there are important outstanding procedural justice issues regarding the ability of disadvantaged communities to partake in decision-making around environmental policies.

Finally, despite our findings, we stress in HCM and here that market-based environmental policies should not be used explicitly to address environmental justice concerns. Market-based policies are intended for allocative efficiency and not distributional objectives, per se. In some settings, an environmental market could widen pollution disparities. As a safeguard, policies that specifically address environmental justice concerns should be considered in tandem with market-based policies. In short, environmental justice problems need environmental justice policies.

2 Methodological differences

This section provides a high-level summary of two key methodological differences between HCM and the PCM and CBW papers.

2.1 Importance of having a control group

PMC note that HCM examine "whether average local-pollutant emissions from a set of cap-and-trade facilities selected for comparability to non-cap-and-trade facilities declined **relative to** the non-cap-and-trade facilities" (Pastor et al., 2022, p.2). By contrast, PMC examine "whether there are distinct temporal patterns **among those sectors regulated** by the cap-and-trade program" (Pastor et al., 2022, p.6) (emphasis added). That is, a key difference between the two approaches is HCM's inclusion of polluting facilities not subject to C&T regulation as a control group.

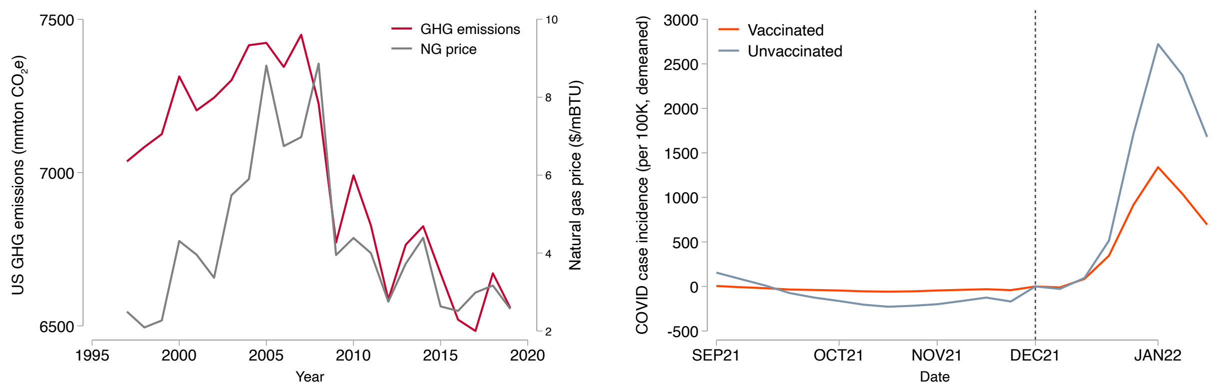
Why might a control group of facilities be important? GHG and criteria air pollution (i.e., $PM_{2.5}$, PM_{10}, NO_x, and SO_x) emissions respond to macroeconomic conditions such as global and national energy prices. This is evident from the left panel of Figure 1 showing the tight coupling between recent U.S.-wide GHG emissions and natural gas prices.[2] Failure to account for these potentially confounding influences makes it difficult to justify claims that results are due to C&T. For example, if one detects an emissions change for C&T regulated facilities before and after the introduction of the program, how does one know those changes are due to C&T and not, say, changes in energy prices during that period?

A common way to address this challenge is to have a control group, which is subject to

[2]Data obtained from: https://cfpub.epa.gov/ghgdata/inventoryexplorer/#allsectors/allsectors/allgas/econsect/all and https://www.eia.gov/dnav/ng/hist/rngwhhdM.htm

these confounding influences but not to the policy. HCM employ such a quasi-experimental approach by including polluting facilities in California that are not regulated by the C&T program. Estimating how emissions change for C&T regulated facilities relative to unregulated facilities allows one to remove the influence of macroeconomic conditions. By contrast PMC and CBW only use data from C&T regulated facilities.

Figure 1: Importance of having a control group



NOTES: Left panel shows annual U.S. GHG emissions (in million metric tons of CO_2e) and the Henry Hub natural gas spot price (in USD per million BTU) for 1997-2019. Right panel shows US-wide age-adjusted COVID-19 weekly case incidence rates between vaccinated and unvaccinated individuals during the omicron variant wave. Case rates normalized to values for the first week of December 2021 for both groups.

The right panel of Figure 1 illustrates the importance of having a control group for a different policy question: whether vaccinations during the recent U.S. omicron COVID-19 variant wave was effective.[3] A focus on just vaccinated individuals (red line) may erroneously lead one to conclude that vaccines were not effective against omicron. However, comparing the response for vaccinated individuals relative to unvaccinated individuals (blue line) clearly tells a different story: vaccines were indeed effective in dampening omicron transmission.

2.2 Modeling pollution dispersal

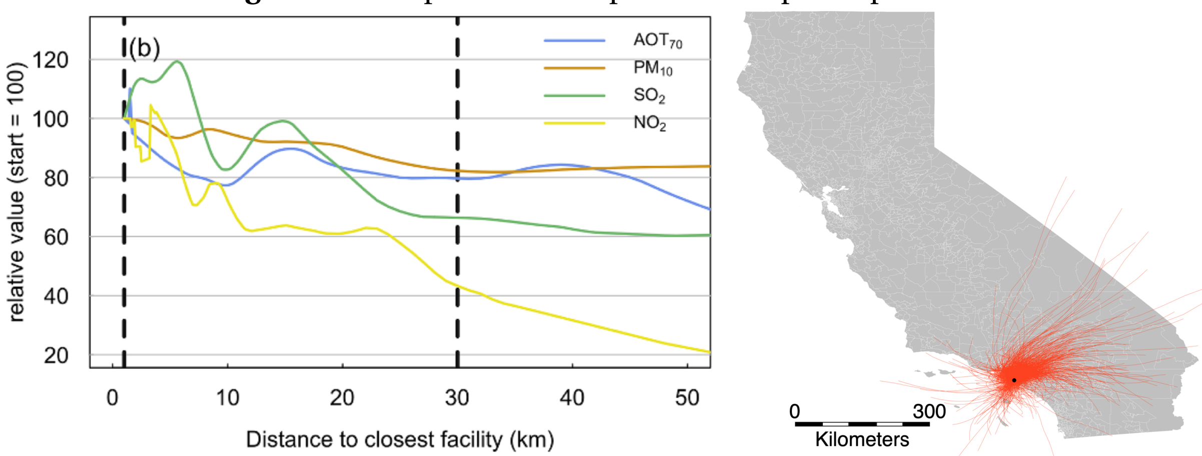
Pollution, once emitted from a facility, disperses across space, often in spatially complex ways. Characterizing these dispersal patterns is essential for accurate conclusions about pollution disparity consequences.

Figure 2 illustrates the complicated nature of pollution dispersal. The left panel of Figure 2, reproduced from Lobell and Burney (2021), shows how far criteria air pollutants can travel from an emitting facility on average (across dispersal directions). Pollution disperses over

[3] Data obtained from: https://data.cdc.gov/Public-Health-Surveillance/Rates-of-COVID-19-Cases-or-Deaths-by-Age-Group-and/3rge-nu2a

large distances (i.e., greater than 20 miles) and patterns vary by pollutant. Dispersal is also not the same in every direction. The right panel of Figure 2, reproduced from HCM, uses a pollution dispersal model to characterize the area affected by pollution from a C&T regulated emitting facility in Los Angeles. Pollution travels varying distances in different directions.

Figure 2: Examples of actual pollution dispersal patterns



NOTES: Left panel shows the spatial decay of criteria air pollution concentrations from U.S. coal-fired power plants, averaged across radial directions of the plants, reproduced from Lobell and Burney (2021). Right panel shows the spatial distribution of particle trajectories every 4-hours from a C&T regulated facility during 2016, reproduced from Hernandez-Cortes and Meng (2022).

By contrast, PMC and CBW impose that pollution from a facility only affects locations within a 2.5 mile circle centered at the facility's location. This assumes that pollution from the facility travels uniformly in every direction and then stops moving after 2.5 miles.[4]

This assumption about pollution dispersal is clearly invalid, but can ensuing results still be informative about the true pollution disparity consequences of an environmental policy? Unfortunately, the answer is no. Not only does failure to characterize pollution dispersal lead to biased results, but the direction of the bias can go in either direction, making it impossible to discern whether results from PMC's distance-circle approach is higher or lower than the true effect on pollution disparities.

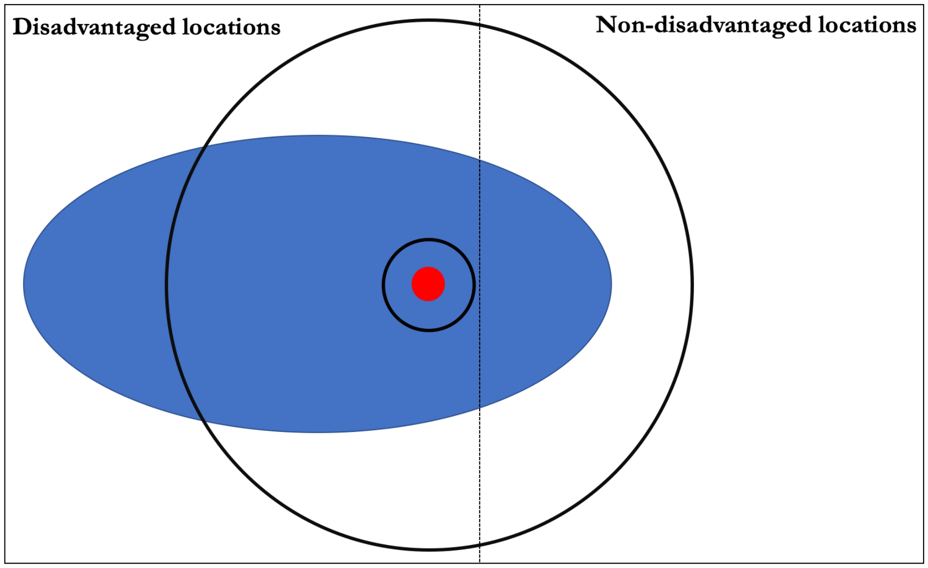
Figure 3 illustrates this issue through a simple spatial example.[5] The polluting facility is indicated by the red dot. The actual set of locations with air quality affected by this polluting facility is shown by the blue area. A disproportionate (but not complete) share of the pollution falls upon locations where disadvantaged communities reside, denote that as $s^o \in (0.5, 1)$.

What happens when one assumes distance-based circles of arbitrary radii for pollution dispersal instead of characterizing the actual pattern of pollution dispersal? If one assumes

[4]CBW also considers a 1 mile distance-based circle, which also has little empirical basis for criteria air pollutants.

[5]A more formal treatment can be found in Deschenes and Meng (2018).

Figure 3: Distance-based circles can lead to uninformative results



NOTES: Figure illustrates the bias in estimates of pollution disparity when assuming a distance-based circle for pollution concentrations. The red dot denotes an emitting facility. The blue area shows the actual spatial extent of pollution concentrations resulting from the facility as overlaid upon locations where disadvantaged and other communities reside. Black-lined circles indicate distance-based circles of varying radii for pollution concentrations assumed by a researcher.

the smaller circle in Figure 3, the conclusion would be that only disadvantaged communities are affected, or $\hat{s}_{small} = 1 > s^o$, a result that is biased upwards. If alternatively, one assumes the larger circle in Figure 3, in which the disadvantaged community share of affected locations is closer to one-half, the result could be biased downwards, or $\hat{s}_{large} < s^o$. The issue is that whenever one fails to characterize the actual area of pollution dispersal, the size of any distance-based circle is essentially arbitrary. Of course, the spatial arrangement presented in Figure 3 does not reflect the case of every polluting facility. Rather, it differs by facility. But this reinforces our argument. Without characterizing actual pollution dispersal, a distanced-based circle of any size is essentially arbitrarily chosen for any facility. And because the researcher cannot verify whether results are upward or downward biased, this approach is generally uninformative of the true pollution disparity consequence. This issue is further exacerbated in policy settings with multiple polluting facilities that can affect air quality in the same locations, as with California's C&T program.

HCM avoid this issue by explicitly modeling pollution dispersal. Specifically, for each C&T regulated facility in our sample, we model where annual C&T-driven emissions disperse via a Lagrangean particle dispersal model, building on recent advances in the incorporating of pollution transport modeling from both natural and social science communities.[6] Our computationally-

[6]For example, see applications of pollution transport models in Ash and Fetter (2004); Morello-Frosch and Jes-

intensive procedure involve over two million pollution trajectories, processed by a thousand paralleled high-performance compute notes over 24 hours. As a robustness check, we also employ a reduced-complexity atmospheric chemical dispersal model (see Table S13 of Hernandez-Cortes and Meng (2022)). We show that incorporating pollution dispersal modeling is essential for detecting our pollution disparity results. Figure 6 of HCM show that simpler, less realistic, approaches for pollution dispersal - including distance-based circles - produce highly unstable pollution disparity effects.

3 Technical critiques

This section replies to specific technical critiques of HCM by PMC.

Heterogeneity in facility pollution abatement PMC critique HCM's statistical approach to modeling heterogeneity in C&T-driven pollution abatement levels, writing "the baseline HCM method is bound to show estimated improvements in equity" (Pastor et al., 2022, p.2). We respectfully disagree. Throughout HCM, we are clear that whether pollution disparities widen or narrow under C&T is an empirical matter and that we take care not to prejudge the answer. This includes empirically testing critical statistical assumptions along the way such that nothing is "bound" to occur without empirical justification.

C&T generates heterogeneous emission abatement levels across regulated facilities. Because it is impossible to estimate facility-level treatment effects when there is only one realization of emission changes per facility, heterogeneous abatement levels across facilities requires the researcher to specify an observable facility characteristic that generates this heterogeneity,[7] and then to empirically validate that assumption.

Our starting point is a recent literature building on Melitz (2003)'s heterogeneous firm model. That framework emphasizes heterogeneity in abatement levels as a function of a facility's emissions size (Forslid, Okubo and Ulltveit-Moe, 2018; Shapiro and Walker, 2018), with bigger emitting facilities having flatter marginal abatement cost curves and therefore higher pollution abatement levels under C&T. To test whether this assumption is valid in our setting, we estimate flexible regression models on facility-level emissions that examine whether abatement levels under C&T indeed vary with average emissions size. These models consistently

dale (2006); Sullivan (2017); Grainger and Ruangmas (2018); Mansur and Sheriff (2019); Cummiskey et al. (2019); Henneman et al. (2019); Henneman, Choirat and Zigler (2019); Kim et al. (2020).

[7] For example, suppose the data generating process is $y_i = \beta_i x_i + \text{error}_i$, with β_i varying with each unit i. With only one observation per unit, β_i cannot be estimated. However, suppose there is a characteristic z_i which varies systematically with β_i. Then one can estimate $y_i = b_1 x_i + b_2 x_i z_i + \text{error}_i$ to recover $\beta_i = b1 + b_2 z_i$.

reveal that larger emitting facilities abate more in levels under C&T (see Table S3 in HCM).[8]

Furthermore, large emitting facilities abating more under C&T does not automatically imply that pollution disparities between disadvantaged and other communities across California will narrow. One still needs to account for the spatial distribution of polluting facilities, how pollution disperses across space, and where disadvantaged and non-disadvantaged communities reside. This reinforces the importance of modeling pollution dispersal, as articulated in Section 2.2.

Interpreting C&T emissions effects PMC explore HCM's regression model of C&T effects on facility emissions (i.e., equation 1 in HCM) in arguing that there are potential data concerns (discussed below). In these critiques, PMC focus on the wrong regression statistic.

For emissions Y_{jt}^p of pollutant $p \in \{GHG, PM_{2.5}, PM_{10}, NO_x, SO_x\}$ for facility j in year t, HCM's differential emissions trend-break model is

$$asinh(Y_{jt}^p) = \kappa_1^p [C_j \times t] + \kappa_2^p [C_j \times \mathbf{1}(t \geq 2013) \times t] + \phi_j^p + \gamma_t^p + \nu_{jt}^p$$

where C_j is a dummy variable for C&T regulation status; ϕ_j^p are facility-specific dummy variables that remove time-invariant determinants of pollution p for facility j; γ_t^p are year-specific dummy variables that remove common determinants of emissions affecting all sample facilities in year t, such as macroeconomic conditions. ν_{jt}^p is the error term.

κ_1^p captures the differential emission pre-trend for pollutant p between facilities that would and would not eventually be regulated by the C&T program during 2008-2012. κ_2^p is the parameter of interest. It captures the change, or break, in the differential emission trend after the program's introduction during 2013-2017. In particular, $\kappa_2^p < 0$ would indicate that C&T led emissions trends for regulated facilities to fall relative to unregulated facilities after the program's introduction. Rather than discussing κ_2^p, PMC's critiques center on $\kappa_1^p + \kappa_2^p$, arguing it is problematic when $\kappa_2^p < 0$ but $\kappa_1^p + \kappa_2^p > 0$. This interpretation is incorrect.

We draw an analog between our differential trend-break model and the standard difference-in-difference (DiD) model. A differential trend-break model tests whether there is a *change* in differential outcome *trends* between treated and control units following a policy. Analogously, in a DiD model, interest is in the *change* in differential outcome *levels* between treated and control units following a policy. In DiD models, one rarely focuses on the sum of the pre- and post-treatment difference between treated and control units. Certainly, one would never ar-

[8]To implement this dimension of heterogeneity, we multiply a common estimated percentage emissions abatement effect under C&T with each facility's estimated fixed effect. We find this approach to yield very similar pollution disparity consequences to that of more flexible emissions models that allow the percentage abatement effect to vary with average emission levels, as shown in col. 6 of Table S7 in HCM.

gue that detection of a DiD treatment effect must take into account pre-treatment differences. In the same manner, the treatment effect in a differential trend-break model is κ_2^p, not $\kappa_1^p + \kappa_2^p$.

Unbalanced panel data PMC note that in CARB's Pollution Mapping Tool, some facilities do not report emission values every year. This is known as an unbalanced panel dataset, a feature this is quite common with firm-level data when entry and exit into production can occur. PMC suggest sample restrictions by limiting the estimating sample of facilities according to some missing-data rule. This approach does not get at the underlying problem, if there is one.

With unbalanced panel data, the critical issue is whether data is missing in a random or non-random manner (Cameron and Trivedi, 2005, p.739). If data is missing at random, estimates using the full unbalanced panel are not biased, and any sample restrictions would only lead to noisier results. If however, data is not missing at random, the sample restriction approach suggested by PMC does not eliminate bias.

To see this, consider PMC's example of examining the "change in the performance of students after a change in teaching strategy in a particular school district but including students who dropped out before teaching practices changed and others who arrived later" (Pastor et al., 2022, p.31). Suppose the new teaching strategy lowered school attendance of some students, forcing them to drop out of school and thus the dataset. A sample restriction to only students that did not drop out would falsely lead the researcher to conclude that the new teaching strategy had no effect on attendance. Alternatively, a sample restriction to only students that dropped out would lead the researcher to falsely conclude that the new teaching strategy was universally detrimental to attendance. In both cases, the sample restriction would lead the researcher to incorrect conclusions.

Other critiques

1. PMC's Figure 14 compares raw GHG emissions data for four C&T regulated facilities (solid lines) against HCM's predicted C&T-driven emissions (dashed lines), writing "the estimated pattern for these four facilities does not bear a strong resemblance to the pattern of the actual data." This is an odd comparison as one should not expect these two sets of lines to match. The dashed lines from HCM represents estimated emissions (i.e. with residuals removed), and more importantly estimated emissions of regulated facilities relative to unregulated facilities. The solid lines indicate observed emissions.

   Another invalid comparison is PMC's mention that HCM's estimated relative GHG decline between C&T regulated and unregulated facilities is higher than the state-wide total GHG decline of 5.3% (Pastor et al., 2022, p.29). There is no reason why these two values should be comparable. Consider two values, X_1 and X_2. PMC's argument amounts to claiming that $X_1 + X_2$ should equal $X_1 - X_2$.

In both cases, PMC is conflating observed emissions trends with emissions trends of C&T regulated facilities **relative** to unregulated facilities. As discussed in Section 2.1, observed emissions trends are unlikely to reflect the causal effect of C&T.

2. PMC note in pg. 27 that overall emission reporting requirements changed between the 2008-2010 and 2011-2017 periods. We are aware of this. However, there is no evidence that these reporting changes were different for C&T regulated and unregulated facilities, which would be needed in order to concern HCM's approach. Instead, any common reporting changes is addressed in HCM's equation 1 through year-specific fixed effects, γ_t^p. Note, however, that a change in data reporting requirements would affect PMC's analysis as they only have data from C&T regulated facilities.

3. Our facility-by-year emissions data for 2008-2015 was downloaded from the CARB Pollution Monitoring Tool on April 27, 2018. We added 2016 data on June 15, 2018, followed by 2017 data on August 8, 2019.

   Using 2008-2017 data downloaded from CARB's Pollution Monitoring Tool in April 2020, PMC note that facility-by-year C&T regulation status has changed in a few cases between our and their versions of CARB's data. PMC further reassign C&T regulation status for a few facilities, arguing that CARB's regulation status for those facilities are wrong.

   Why C&T regulation, or treatment, status has changed across versions of CARB's dataset is worth looking into, in consultation with CARB data managers. However, we caution against researchers being too "hands on" in altering data on treatment status. While there may be errors in treatment status, those errors are more likely to be random when the data is assembled by someone else besides the researchers.

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