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Identifying and Addressing Confounding Bias in Violence Prevention Research

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Abstract

Purpose of Review Violence prevention research has enhanced our understanding of individual and community risk and protective factors for aggression and violence. However, our knowledge of risk and protective factors for violence is highly dependent on observational studies, since there are few randomized trials of risk and protective factors for violence. Observational studies are susceptible to systematic errors, specifically confounding, and may lack internal validity.

Recent Findings Many violence prevention studies utilize methods that do not correctly identify the set of covariates needed for statistical adjustment. This results in unwarranted matching and restriction leading to further confounding or selection bias. Covariate adjustment based on purely statistical criteria generates inconsistent results and uncertain conclusions.

Summary Conventional methods used to identify confounding in violence prevention research are often inadequate. Causal diagrams have the potential to improve the understanding and identification of potential confounding biases in observational violence prevention studies, and methods like sensitivity analysis using quantitative bias analysis can help to address unmeasured confounding. Violence research studies should make more use of these methods.

Keywords Confounding · Directed acyclic graphs · Violence prevention

Introduction

Violence prevention research has enhanced our understanding of individual and community risk and protective factors for aggression and violence. However, a weakness of the field is that our knowledge of risk and protective factors for violence is highly dependent on observational studies. There are a few randomized trials of examining the effect of risk and protective factors for violence, but conducting such studies in violence prevention introduces many ethical concerns that most often can only be navigated by using non-randomized observational designs. However, observational studies may lack internal validity. The main challenge to the validity of observational studies is that the observed associations between risk/protective factors and health outcomes may be biased due to the presence of other factors acting as confounders or selection factors [1]. Observational studies may also have information bias that leads to misclassification of exposures and outcomes [1]. These three types of biases (confounding bias, selection bias, and information bias) are collectively referred to as systematic errors [1]. Such biases are widely noted in observational studies examining the association between various risk/protective factors and violence-related outcomes like violence victimization [2, 3, 4••], sexual and intimate partner violence [5•, 6•, 7••], youth violence [8•, 9•], child maltreatment [10•, 11•], elder abuse [12•], firearm-related violence [13•, 14•], homicides [2–5, 6•, 7••], suicides [15•], and legal intervention deaths [16]. Yet, methods for addressing these biases are seldom discussed. This review illustrates the use of modern epidemiologic methods for addressing the most common of these sources of bias, confounding.

Of the three systematic biases noted above, selection and information biases tend to be readily identified and discussed in limitation sections of most peer-reviewed published research [17–19]. However, confounding bias is seldom examined or discussed in violence prevention research. In the majority of

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observational violence research, potential confounders remain unmeasured with little discussion on how this might affect the study results. Some authors discuss additional confounding as a limitation but do not address it directly in their research and sometimes conclude, with little or no empirical justification, that such confounding may not affect their study substantively [20]. Many authors have gone a step further and stated that causal interpretations cannot be made from their study [21–25]. However, as public health scientists, we must acknowledge that such blanket statements do not absolve us from how our study results are used in informing violence prevention interventions. Despite their limitations, the majority of our understanding today about how risk/protective factors affect violence-related outcomes depends on observational studies, since randomized studies examining risk/protective factors for violent outcomes are not likely to be ethically feasible.

A common analytical strategy in violence research is to control for as many covariates as possible, typically using one or more statistical techniques. Some studies have attempted to refine this approach by only controlling for covariates that have a statistically significant relationship with the study outcome [24, 26, 27]. Some authors also focus on controlling for covariates that produce an a priori determined magnitude of change in the relationship between the risk/protective factor of interest and the outcome [27, 28]. However, such criteria still do not provide any clarity in identifying potential confounders or deepening our understanding of the confounding processes at play. In fact, adjustment for covariates identified through such criteria may sometimes be unadvisable as they may cause further selection bias in the study [29–31]. In addition, most violence prevention researches discount the potential of time-varying confounding and almost never attempt to explore the possibility of unmeasured confounding.

In this review, I will define confounding, discuss the pros and cons of conventional and more definitive methods of identifying confounding using examples from published literature, and discuss methods to explore and address unmeasured confounding in observational violence prevention research.

What Is Confounding?

Confounding bias occurs when the association between a risk factor and a violent outcome can be completely or partially explained by a third factor (confounder) [1], which predicts both the risk factor and the violent outcome, and the confounder cannot be predicted by the risk factor (Fig. 1) [32]. As an example, let us consider greenery (trees and green areas in neighborhoods) in relation to firearm-related violence. In non-randomized studies examining the association of greenery with firearm-related assaults [17], the amount of economic activity in an area (represented by the number of shopping

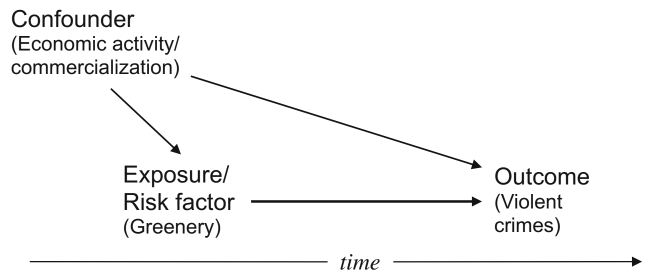

Fig. 1 Relationship of a confounder with exposure and outcome

centers, recreational centers, restaurants, movie theaters, and other commercial activity) may be associated with both the amount of greenery and violent crimes [18]. Economic activity in an area affects greenery if trees are cut down to make way for shopping centers and malls. Similarly, more economic activity in an area brings more people to the area and increases the likelihood of assaults related to robbery and gang-related violence [18]. Thus, greenery appears to be positively associated with firearm- and robbery-related assault; however, this relationship is confounded by economic activity. An inverse relationship may exist between greenery and intimate partner violence and still confounded by economic activity. Assault related to intimate partner violence is more likely to occur in the privacy of home [33], or residential neighborhoods where greenery is higher and economic activity is lower.

Note that in thinking about confounding and examining associations between risk and protective factors and violent outcomes, the concept of time and temporality are very important [34]. A confounder always occurs temporally prior to the exposure and outcome (Fig. 1). A factor that occurs after the exposure has already taken place cannot be a confounder because it cannot retroactively modify the exposure.

The ideal method for confounding control in an experimental study is randomization [35]. Randomization allows assignment of intervention randomly so that the intervention and control arms are balanced [34, 35]; that is, all potential confounders are equally distributed between the intervention and the control arms [34]. This balance ensures comparability between the two groups, such that, if we were to switch the groups so that the control group now gets the intervention and the intervention group ends up getting the control treatment, the observed effect of the intervention on the violent outcomes will be the same. In other words, randomization affords the creation of a surrogate for the true counterfactual group for purposes of making an experimental comparison [34]. Note, however, that randomization controls for confounding on average, meaning for large sample sizes or over many studies.

In observational studies, where the risk factors are not random and individuals choose their exposures or get exposed through various non-randomized and correlated mechanisms, we have to employ other means of confounding control.

Commonly used methods for confounding control in observational studies include restriction, matching, stratification, and statistical adjustment including direct adjustment of variables in regression analyses, direct standardization (e.g., inverse probability weighting), and indirect standardization.

Identifying Covariates to Control Confounding in Observational Studies

In an attempt to address the problem of measured and unmeasured confounders, a common—but flawed—analytical strategy in violence research is to control for as many covariates as possible, typically by adjusting for these covariates in a regression-based analysis [29]. These covariates might include not only potential confounders for which control would be advisable, but also others for which control would be unnecessary or could even resulting in selection bias (e.g., due to missing data on a confounder). Other conventionally used methods to identify confounders include (1) adjusting for all factors that have a p value lower than 0.05 in the statistical model, i.e., adjusting for all statistically significant predictors of the outcome, regardless of the predictors' association with the risk/ protective factor under study [24, 26, 27]; (2) adjusting for covariates that produce an a priori determined amount of change (e.g., 10% or 15%) in the effect estimate representing the relationship between the outcome and risk/protective factor under study [27, 28]; and (3) adjusting for all covariates that produce a greater change in the effect estimate as compared with the potential inflation of the standard error of the effect estimate representing the relationship between the risk/protective factor and the outcome (seldom used in violence prevention studies) [36]. These methods are frequently inadequate to address confounding (further discussed in the section on traditional methods for confounding control below).

Another well-known method of identifying the covariates needed to control confounding is drawing directed acyclic graphs (DAGs). However, the use of this method in violence research to date has been limited [4••, 7••, 32, 37, 38••, 39••, 40••]. A DAG allows the investigator to identify causal and non-causal paths of association between a risk/protective factor and an outcome of interest. Considering Fig. 1, the arrow starting from the exposure (E) and ending into the outcome (O) is considered a causal path, which can be denoted by E→O. However, the path where the confounder (C) affects both the exposure and outcome is the non-causal path, denoted as E←C→O. The goal is to block the non-causal path (by adjusting for the confounder) to assess the causal association between the exposure and the outcome. In essence, Fig. 1 is a simple form of a DAG. A DAG usually only includes the variables that an investigator observes and includes in his/her analysis. This means that there is always some level of unmeasured or unknown confounding that may not have been addressed. Hence, there is typically a preference for the use of the word "association" rather than "effect" in reporting results from observational studies.

In addition to the causal and non-causal paths observed in Fig. 1, there are other types of causal and non-causal paths. A path that starts from the exposure affecting another variable (also known as the intermediate variable or I), which in turn affects the outcome, is termed as an indirect causal path. Such a path can be denoted as E→I→O, note that all the arrows point toward the outcome. An example of an indirect causal path can be seen in Fig. 2a, where the exposure, police reporting, affects an intermediate variable, change in behavior, which further predicts the outcome, future victimization. Naturally, the kind of causal path we see in Fig. 1 (E→O) is called a direct causal path. Similarly, non-causal paths can also go through many other covariates. It is important to note that, to block a non-causal path, we only need to control for one well-measured covariate on that path. Ultimately, the purpose of a DAG is to identify a minimally sufficient set of well-measured covariates that control for all known confounding in the relationship between a risk factor and an outcome of interest [32, 37].

Identifying Covariates to Control Confounding Using a DAG

To demonstrate the utility of DAGs to identify a minimally sufficient set of control variables, I will use data from a previously published violence study where the authors utilized a DAG to identify the minimal set of control covariates to include in a regression model (the DAG was not published) [4••]. The research question was, "does police reporting of crime victimizations affect the incidence of future victimizations?" Figure 2a is the final DAG used in that study. Note that DAGs can be subjective; that is, different researchers may write different DAGs to address the same research questions. DAGs that can be supported by previously published literature and developed with consensus among research team members are likely to be more reliable [32]. The DAG presented here (Fig. 2a) was similarly developed using prior literature and with consensus from all co-authors listed on the original study [4••].

The minimally sufficient set of covariates to control confounding in Fig. 2a includes some variables that meet the definition of a confounder (affects both exposure and outcome and is not affected by the exposure). These included type of baseline victimization (interpersonal violence/burglaries/thefts), victim demographics (age, sex, race, income, education), victim–offender relationship (stranger/non-stranger), and offender sex (male/female). However, the authors also adjusted for some of the covariates that do not meet the definition of a confounder—place of victimization (inside home/outside home/friend's home/commercial place/parking

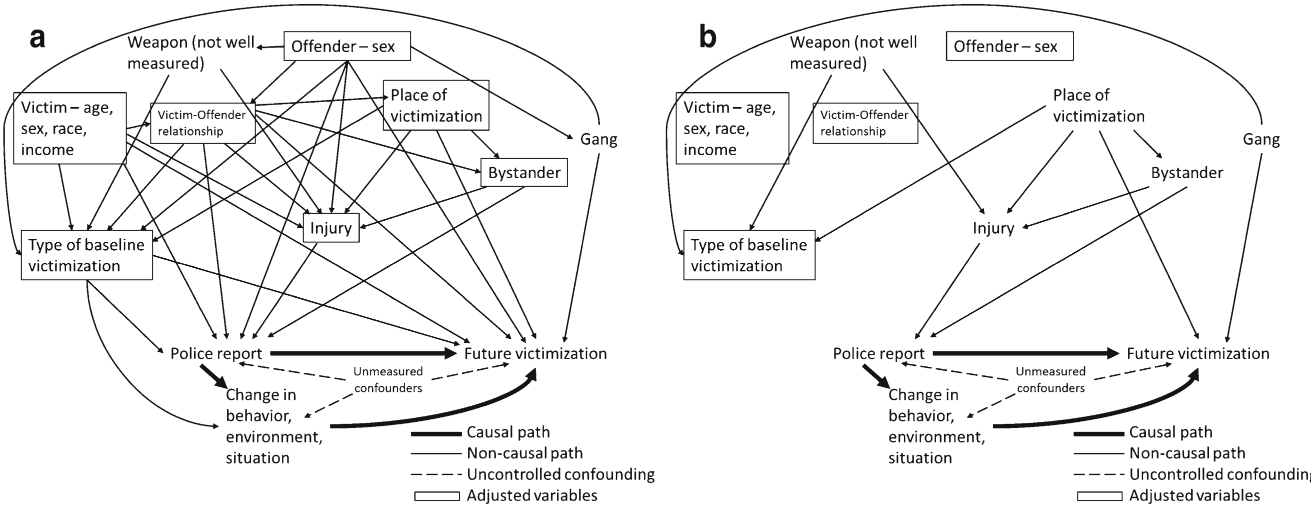

Fig. 2 Association of police reporting with the incidence of future victimization. **a** Minimal sufficient set of well-measured covariates (boxed variables) needed to control for all observed confounding. **b** Adjustment for only the traditional confounders leads to incomplete confounding control

places/school/ public places/other), victim injury during the baseline victimization (yes/no), and bystander presence (yes/no/do not know).

It may seem odd to adjust for covariates that do not meet the definition of a true confounder. To understand, let us consider the scenario where these factors were not adjusted. Essentially, when we adjust for a covariate, we nullify its effect on other factors in the DAG using our statistical model. So if we only adjust for the covariates that are true confounders, the regression model may do something like in Fig. 2b to our data. Note, the arrows do not disappear in reality, but their effect is nullified by controlling for them. By removing the arrows associated with the true confounders, we can explicitly appreciate the remaining non-causal paths.

Upon removing the arrows associated with the true confounders, we see that there are still eight non-causal paths that remain:

1) Police report ← Injury ← Place of victimization → Future victimization
2) Police report ← Injury ← Weapon → [baseline victimization] ← Gang → Future victimization
3) Police report ← Injury ← Weapon → [baseline victimization] ← Place of victimization → Future victimization
4) Police report ← Bystander → Injury ← Weapon → [baseline victimization] ← Place of victimization → Future victimization
5) Police report ← Injury ← Bystander ← Place of victimization → [baseline victimization] ← Gang → Future victimization
6) Police report ← Bystander → Injury ← Place of victimization → [baseline victimization] ← Gang → Future victimization
7) Police report ← Bystander → Injury ← Weapon → [baseline victimization] ← Gang → Future victimization
8) Police report ← Bystander ← Place of victimization → [baseline victimization] ← Gang →Future victimization

Path 1 above can be controlled by controlling for place of victimization. Paths 2 to 8 may appear to be controlled since baseline victimization on those paths was adjusted for (indicated by square brackets around it). However, on paths 2–8, the variable baseline victimization is something we call a "collider" (where two arrows collide) [29–32]. A collider is a covariate that is affected by two other variables that are otherwise independent of each other [30, 32]. In such instances, since the two variables affecting the collider are independent, the path is naturally blocked or closed. However, adjusting (or restricting or stratifying) for the collider will open up the pathway and induce a relationship between two naturally independent predictors of the collider. This resulting bias leads to a form of selection bias, also known as collider stratification/conditioning bias [29–32].

In our example, one way to not induce the collider conditioning bias would be to not control for baseline victimization; however, baseline victimization is a true confounder (Police report ← type of baseline victimization → Future victimization), hence this path must be controlled. Therefore, a better way to control for these paths is by controlling for other covariates on that path. Note that paths 3–5 will be also get closed once we control for place of victimization for path 1. Also, note that weapon and gang variables were not well measured in this dataset, so controlling for those will not solve the problem. Hence, the only remaining option to close path 2 was to control for the injury variable. However, the injury variable is also a collider on paths 6 and 7. So if we had not controlled for injury, paths 6 and 7 would have been closed naturally, being blocked at the injury variable. However, since we did control for injury to close path 2, we effectively opened paths 6 and 7. Hence, the simplest way to close paths 6 and 7 is by adjusting for bystander presence. Lastly, controlling for place of victimization (for paths 1, 3–5) and/or bystander presence (for paths 6 and 7) would automatically close path 8. Thus, we were able to completely close all the pathways. Hence, in addition to adjusting for the traditional confounders, we also adjusted for the place of victimization, bystander presence, and injury, which addressed all measured confounding.

Once a minimally sufficient set of well-measured control variables is identified, we can use standard statistical methods (e.g., regression analyses) to control for these covariates and estimate the association between a risk/protective factor and a violent outcome [32]. In studies with repeated measurements of the exposure/risk factor (e.g., prison entry) and violent outcome (e.g., homicide death), the confounder (e.g., mental health condition) may vary with time, thereby causing time-varying confounding [41]. Essentially, time-varying confounding occurs when a subsequent measure of a confounder is affected by prior exposure [41]. Such relationships can be mapped out using a DAG; however, addressing such relationships may require the use of advanced statistical techniques like inverse probability weighted marginal structural models [29] or g-formula [42].

Why Are Traditional Methods of Identifying Confounding Control Covariates Inadequate?

As stated earlier, some conventional techniques used to identify potential confounders may depend on p values, a priori change in estimate criteria, and bias-variance tradeoff. Although their use is widespread, these methods have several limitations, as described below.

A p value of > 0.05 in the statistical model, for a particular covariate, indicates that the covariate is a predictor of the outcome, but it does not tell us anything about the covariate's relationship with the exposure (risk/protective factor for violence). Such a factor may either have no relationship with the exposure or even be on the causal pathway from the exposure to outcome—for example, the variable for change in behavior in Fig. 2. Adjusting for such an intermediate variable may in fact block the causal pathway and induce selection bias due to adjustment on colliders. Note, for example, that change in behavior (e.g., change in work commute route) is a collider on this path: Police report → [Change in behavior] ← unmeasured confounders (e.g., job change) → Future victimization. This method of controlling for only strong predictors of the outcome is similar to model fitting approaches generally used in predictive modeling. But, using model fitting approaches to examine the association of a specific risk factor with an outcome is fraught with similar limitations [43]. A model will be more parsimonious (or better fit) if it includes more and the strongest of the predictors of the outcome, which may or may not be related to the exposure and may even be on the causal path. Hence, measures of associations (e.g., risk ratios, rate ratios, odds ratios, hazard ratios) obtained from a predictive model may not represent the entire relationship between the risk factor and the outcome, and may even be affected by collider conditioning bias. Similar criticism has been appropriated toward interpreting model coefficients for confounders obtained from statistical models [31].

Likewise, an a priori determined change (e.g., 10% or 15%) in the effect estimate does not satisfy all the requirements for a confounder. Specifically, if adjustment for a covariate leads to a substantive change in the effect estimate, it indicates that the covariate is on some pathway between the exposure and the outcome. But such change in estimate will also be observed when controlling for an intermediate variable. Thus, change in estimate criteria do not distinguish between intermediate and confounder variables and can lead to the blocking of the causal effect. Additionally, they may induce selection bias due to collider conditioning. Similar limitations are also observed when the selection of adjustment covariates is based on the comparison between the magnitude of bias removed (examined by the percentage change in estimate) and the variance introduced (change in variance of the effect estimate) in the model.

In contrast, utilizing a DAG explicitly examines all potential pathways through which confounding may arise [4••, 7••, 38••, 39••, 40••]. This helps address bias not only in the data analyses phase, but also in the study design phase. As an example, one study examining the association of firearm possession on gun assaults presented "fully adjusted" results controlling for the known predictors of the outcome and "limited adjusted" results controlling for factors that produce 15% or more change in estimate [28]. The fully adjusted model included factors such as bystander presence and surrounding area at the time of assault, which predict the outcome but do not temporally precede the risk factor of interest (firearm possession). Hence, they are not traditional confounders of the firearm possession and assault relationship. We may be able to argue that they are part of some noncausal pathway, but we can also equally argue that they may in fact be a part of a causal pathway. Similarly, if a DAG is drawn for such a study (association of firearm possession on gun assaults) before the data is collected, we may be able to see that factors like gang affiliation and gun ownership would be strong confounders, which were not controlled in this study [28]. Because of such limitations of the conventional confounding control methods, we cannot be sure which effect estimate to be certain about, the fully adjusted or the reduced one. Regardless, in this example, it should be noted that, given the large effect estimates in this study and other literature supporting similar results [13, 26], the direction of associations noted in this study seems robust [28].

Limitations of DAGs

DAGs offer a pictorial view of a research question at hand, but the picture is only as good as the substantive knowledge of those developing it [44]. The relationships of the known covariates with the exposure and outcome should be determined based on published literature, expert knowledge, and research team consensus. Similarly, a minimally sufficient set of adjustment covariates obtained from a DAG is only as good as the covariate measurement methods [44]. Errors in covariate measurement (misclassification) or a large amount of missingness in covariates may lead to further bias.

In reality, it is often difficult to accurately identify all confounders or confounding mechanisms in an observational study. Hence, there is often assumed to be some degree of unmeasured confounding in observational studies. In such cases, the best that a DAG could do is to simply acknowledge that fact. The best tool we have to address all potential cofounding (known or unknown) is conducting a randomized controlled trial (RCT). There is no DAG needed for an RCT because randomization ensures that the intervention and control arms are balanced with respect to all potential confounders, thereby removing all arrows that go into the assigned intervention. But an RCT may not be ethically feasible for all violence-related research questions. In the absence of a randomized design, it is incumbent on the investigator to use statistical/ epidemiological tools to increase the robustness of our inferences in the face of unmeasured or unknown confounding. Such tools include sensitivity analyses and quantitative bias analyses.

Addressing Unmeasured Confounding

Unmeasured confounding can be thought of as (1) confounding pathways that we know exist, but do not have data on, e.g., confounding due to gang affiliation and firearm ownership in studies examining the association of firearm possession on firearm assaults [28], or (2) confounding pathways that we do not know about, potentially because of lack of research in that area, but may exist and bias the relationship between the risk/protective factor and the outcome under study [45]. Acknowledging the presence of these types of confounding is essentially acknowledging the limits of our understanding of the phenomenon that takes place in nature. Once acknowledged, one of the best ways to address unmeasured and unknown confounding is to conduct sensitivity analyses.

The goal of such sensitivity analyses may vary depending on how much information is available to the investigators on the number and strength of plausible unmeasured confounders. For example, consider a study of the association between firearm possession and assaults in which the research team lacks data on gang affiliation and is concerned that it may confound the results. The research team may be able to establish (perhaps from prior research) how gang affiliation affects firearm possession and how it may affect assaults. They could then simulate these associations in their data and examine how the observed effect estimate (of the relationship between the risk/protective factor and the outcome) would change if they hypothetically were able to adjust for gang affiliation. Such analyses can be readily conducted using quantitative bias analyses methods [46, 47]. In one study, the authors used simple sensitivity analyses to develop and adjust for a history of crime variable by combining known information from the data while examining the association of hospitalization due to a firearm injury and subsequent violent outcomes [48••].

In places where no information may be available about a confounder, an alternative way could be to examine the magnitude of confounding it would take to shift the observed effect estimate completely to null [45]. This is, in essence, one form of "worst-case scenario", in which the observed association is entirely due to unmeasured confounding. Such a measure has been termed the "E-value", defined as "the minimum strength of association that an unmeasured confounder would need to have with both treatment and the outcome to fully explain away a specific treatment-outcome association" [49]. If, for example, a very large E-value is needed, then it seems plausible that such a strong phenomenon would have been already studied and characterized. If there is no research that documents such a strong phenomenon regarding potential unmeasured confounders, we can safely assume that the observed effect estimate for the relationship between the risk/protective factor and the outcome of interest is less likely to be subject to confounding. Methods to calculate the E-value are similar to conducting quantitative bias analyses and should be a standard practice for researchers using observational data [49].

Conclusions

Epidemiologic studies of violence prevention have been helpful in informing interventions and policies that have the potential to shape our society for the better. Utilization of modern epidemiologic methods like DAGs and analytic techniques like quantitative bias analyses will strengthen those efforts by producing a robust evidence-base of risk and protective factors of violence.

Compliance with Ethical Standards

Conflict of Interest The author declares that there is no conflict of interest.

Human and Animal Rights and Informed Consent This article does not contain any studies with human or animal subjects performed by any of the authors.

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