Revisiting Simpson’s Paradox: 
a statistical misspecification perspective

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Abstract

The primary objective of this paper is to revisit Simpson’s paradox using a statistical misspecification perspective. It is argued that the reversal of statistical associations is sometimes spurious, stemming from invalid probabilistic assumptions imposed on the data. The concept of statistical misspecification is used to formalize the vague term ‘spurious results’ as ‘statistically untrustworthy’ inference results. This perspective sheds new light on the paradox by distinguishing between statistically trustworthy vs. untrustworthy association reversals. It turns out that in both cases there is nothing counterintuitive to explain or account for. This perspective is also used to revisit the causal ‘resolution’ of the paradox in an attempt to delineate the modeling and inference issues raised by the statistical misspecification perspective. The main arguments are illustrated using both actual and hypothetical data from the literature, including Yule’s ”nonsense-correlations” and the Berkeley admissions study.

KEYWORDS: Association reversal; Spurious correlation; Statistical misspecification; Statistical vs. substantive adequacy; Misspecification testing; Untrustworthy evidence; Causality; Confounding.
1 Introduction

True to "Stigler's Law of Eponymy" (Stigler, 1980), Simpson’s paradox has a long history in statistics going back to Yule's (1903) 'spurious' association, but it is currently credited to Simpson (1951) for reframing it as a 'paradox'; see Blyth (1972).

The paradox seems to have a number of alternative conceptions, and thus, it is often described interchangeably as a counter-intuitive statistical result pertaining to:

(a) Statistical associations that reverse themselves, such as "a marginal association can have a different direction from each conditional association" (Agresti, 2013).

(b) Either the magnitude or the direction of an association between two variables is influenced by a third variable, such as "the association between a pair of variables \(X, Y\) reverses sign upon conditioning on a third variable, \(Z\)." (Pearl, 2014).

(c) Apparent statistical associations that after closer scrutiny of the data are rendered 'spurious' (Yule, 1903).

The recent discussions in statistics have focused on adopting one of the perspectives (a)-(c), and using actual or hypothetical data to either explain away the paradox or criticize other proposed 'solutions'. The current dominating view revolves around perspective (b) that differs from (a) in so far as it emphasizes the causal dimension of conditioning on a confounder; see Pearl (2009), Spirtes et al. (2000). Armistead (2014) put forward a dissenting view by arguing that perspective (b) is rather narrow to explain the different facets of the paradox:

"Simpson’s Paradox, like all paradoxes, can be defined as an apparent contradiction that may contain more than one truth." (p. 6)

A strong case can be made that Simpson’s paradox has different dimensions that are often conflated or ignored in the literature. As argued by Wasserman (2004):

"Simpson’s paradox is a puzzling phenomenon that is discussed in most statistics texts. Unfortunately, most explanations are confusing (and in some cases incorrect)." (p. 259)

The primary aim of this paper is to shed light on the different conceptions of the paradox by bringing out the similarities and differences between perspectives (a)-(c). The key is provided by Yule’s idea of ‘spuriousness’ in (c). Beginning with Yule (1903), the problem of ‘fictitious’ associations and ‘spurious’ correlations was a recurring theme in Yule’s papers that culminated in Yule (1926) on “nonsense-correlations”. Although he shed some light on the issues involved, he did not succeed in establishing a direct link between spurious associations and invalid probabilistic assumptions for reasons to be discussed in the sequel. The notion of statistical misspecification can be used to formalize the term ‘spurious’ as ‘statistically untrustworthy’ results, stemming from unreliable inference procedures. This enables one to delineate between two distinct cases of association reversal:

Case 1. The reversal is statistically trustworthy due to statistical adequacy.
Case 2. The reversal is statistically untrustworthy due to statistical misspecification.

It turns out that the statistical misspecification perspective suggests that in both cases there is nothing counterintuitive to explain.

In section 2, we discuss the case where the reversal is statistically trustworthy due to the fact that the statistical models involved are statistically adequate: the invoked probabilistic assumptions are valid for the particular data. When this is not the case, the inference results are likely to be statistically untrustworthy (spurious). This is discussed in section 3 using two empirical examples that bring out the distinction between statistical and substantive misspecification. The statistical misspecification argument is illustrated further in section 4 using several widely discussed examples of the paradox. In section 5, we revisit the causal ‘resolution’ of the paradox in an attempt to delineate the modeling and inference issues raised by the statistical misspecification perspective.

2 Marginal vs. conditional associations

Consider the case of a Linear Regression (LR) model:

\[ y_t = \beta_0 + \beta_1 x_{1t} + \beta_2 x_{2t} + u_t, \quad (u_t | X_{1t} = x_{1t}, X_{2t} = x_{2t}) \sim \text{NIID}(0, \sigma_u^2), \quad t \in \mathbb{N}, \]

where 'NIID' stands for ‘Normal, Independent and Identically Distributed’. It is often insufficiently appreciated that the error assumptions imply a particular statistical parameterization for the unknown parameters \( \theta := (\beta_0, \beta_1, \beta_2, \sigma^2) \) in terms of the moments of the observable process \( \{Z_t := (y_t, X_{1t}, X_{2t}), \quad t \in \mathbb{N}\} \) underlying data \( Z_0 \) (see Appendix). Alternatively, one can derive the parameterization directly using the joint distribution of the observable random variables involved:

\[
\begin{pmatrix}
\begin{pmatrix} x_{1t} \\
\end{pmatrix} \\
\begin{pmatrix} x_{2t} \\
\end{pmatrix}
\end{pmatrix} \sim \text{NIID} \left( \begin{pmatrix}
\begin{pmatrix} \mu_1 \\
\mu_2 \\
\mu_3
\end{pmatrix} \\
\begin{pmatrix} \sigma_{11} & \sigma_{12} & \sigma_{13} \\
\sigma_{12} & \sigma_{22} & \sigma_{23} \\
\sigma_{13} & \sigma_{23} & \sigma_{33}
\end{pmatrix}
\end{pmatrix} \right) \quad (2)
\]

In this case, the regression and skedastic functions take the form:

\[ E(y_t | X_{1t} = x_{1t}, X_{2t} = x_{2t}) = \beta_0 + \beta_1 x_{1t} + \beta_2 x_{2t}, \quad \text{Var}(y_t | X_{1t} = x_{1t}, X_{2t} = x_{2t}) = \sigma^2, \]

where the parameterizations of \( \theta := (\beta_0, \beta_1, \beta_2, \sigma^2) \) are (table 1):

\[
\begin{align*}
\beta_0 &= \mu_1 - \beta_1 \mu_2 - \beta_2 \mu_3, \\
\beta_1 &= \frac{(\sigma_{12} \sigma_{33} - \sigma_{13} \sigma_{23})}{(\sigma_{22} \sigma_{33} - \sigma_{23}^2)}, \\
\beta_2 &= \frac{(\sigma_{11} \sigma_{22} - \sigma_{12} \sigma_{23})}{(\sigma_{22} \sigma_{33} - \sigma_{23}^2)} \\
\sigma_u^2 &= \sigma_{11} - \sigma_{12} \left( \frac{(\sigma_{12} \sigma_{33} - \sigma_{13} \sigma_{23})^2}{(\sigma_{22} \sigma_{33} - \sigma_{23}^2)} \right) - \sigma_{13} \left( \frac{(\sigma_{11} \sigma_{22} - \sigma_{12} \sigma_{23})^2}{(\sigma_{22} \sigma_{33} - \sigma_{23}^2)} \right) = \sigma_{11} - \sigma_{12} \beta_1 - \sigma_{13} \beta_2
\end{align*}
\]

These results offer the key to elucidating perspectives (a)-(b) on Simpson’s paradox.

**Perspective (a) on Simpson’s paradox.** The correlation between \( y_t \) and \( X_{1t} \) \((\rho_{12} = \frac{\sigma_{12}}{\sqrt{\sigma_{11} \sigma_{22}}} \rangle\), is positive \((\rho_{12} > 0)\), but the coefficient \( \beta_1 \) in (1) is negative \((\beta_1 < 0)\).
Is this reversal of association possible, and under what circumstances?

In light of the parameterization of $\beta_1$ in (3), its numerator is negative when:

$$[\sigma_{12}\sigma_{33} - \sigma_{13}\sigma_{23}] < 0 \rightarrow \frac{\sigma_{13}\sigma_{21}}{\sigma_{33}} > \sigma_{12}$$

Multiplying both terms in the last expression by $1/\sqrt{\sigma_{11}\sigma_{22}}$, yields:

$$\frac{\sigma_{13}\sigma_{21}}{\sigma_{33}\sqrt{\sigma_{11}\sigma_{22}}} = \rho_{13}\rho_{23} > \rho_{12} = \frac{\sigma_{12}}{\sqrt{\sigma_{11}\sigma_{22}}},$$

where $\text{Corr}(Y_t, X_{2t}) := \rho_{13} = \frac{\sigma_{13}}{\sqrt{\sigma_{33}\sigma_{11}}}$ and $\text{Corr}(X_{1t}, X_{2t}) := \rho_{23} = \frac{\sigma_{23}}{\sqrt{\sigma_{22}\sigma_{33}}}$. Hence, $\rho_{12} > 0$ and $\beta_1 < 0$ occur when the following conditions hold:

(i) the correlation coefficients $\rho_{13}$ and $\rho_{23}$ have the same sign,

(ii) the product of $\rho_{13}$ and $\rho_{23}$ is greater than $\rho_{12}$, i.e., $\rho_{13}\rho_{23} > \rho_{12}$, and

(iii) the determinant of the correlation matrix of $Z_t$ is positive:

$$\text{Corr}(Z_t) = 1 - \rho_{12}^2 - \rho_{13}^2 - \rho_{23}^2 + 2\rho_{12}\rho_{13}\rho_{23} > 0.$$  

Condition (iii) ensures that $f(y_t|x_{1t}, x_{2t}; \varphi)$ in (2) is proper, giving rise to a well-defined conditional distribution $f(y_t|x_{1t}, x_{2t}; \varphi)$; see Spanos and McGuirk (2002).

2.1 Example 1. Correlations vs. partial correlations

Assuming $\sigma_{11} = \sigma_{22} = \sigma_{33} = 1$, without any loss of generality, let the relevant correlations be: $(\rho_{12}, \rho_{13}, \rho_{23}) = (0.5, \pm 7, \pm 8)$, which satisfy (i)-(iii) above.

(a) For values $(\rho_{13}, \rho_{23}) = (\pm 7, \pm 8)$: $\beta_1 = -0.167$, $\beta_2 = 0.833$, $\sigma^2 = 0.5$

(b) For values $(\rho_{13}, \rho_{23}) = (\pm 7, \pm 8)$: $\beta_1 = -0.167$, $\beta_2 = -0.833$, $\sigma^2 = 0.5$

Note that the sign of $\beta_2$ reflects the common sign of $(\rho_{13}, \rho_{23})$. In light of these results, it is clear that there is nothing paradoxical, or surprising, about the reversal of sign between the simple correlation $\rho_{12} > 0$ [stemming from the joint distribution $f(y_t|x_{1t}; \varphi_1)$], and the regression coefficient $\beta_1 < 0$ [stemming from the conditional distribution $f(y_t|x_{1t}, x_{2t}; \varphi_2)$]. This reversal is due to the conditions (i)-(iii) above, which are easily testable in practice; see Spanos (2006b).

It is well-known that there is a direct connection between $\rho_{12}$ and the regression coefficient of $x_{1t}$ in the context of the simple linear regression:

$$y_t = \alpha_0 + \alpha_1 x_{1t} + \varepsilon_t,$$

where $\varepsilon_t|X_{1t} = x_{1t}) \sim \text{NIID}(0, \sigma^2_t)$, $t \in \mathbb{N}$, whose implicit statistical parameterization of $\varphi := (\alpha_0, \alpha_1, \sigma^2_t)$ is:

$$\alpha_0 = \mu_0 - \alpha_1 \mu_2, \quad \alpha_1 = \frac{\sigma_{12}}{\sigma_{22}}, \quad \sigma^2_t = \sigma_{11} - \frac{\sigma_{12}^2}{\sigma_{22}}.$$  

This is because $\rho_{12}$ is a scaled $(\sqrt{\sigma_{22}/\sigma_{11}} > 0)$ reparameterization of $\alpha_1$:

$$\rho_{12} = \sqrt{\sigma_{22}/\sigma_{11}} \alpha_1.$$  

In the above numerical example, $\alpha_1 = 0.5$ and $\beta_1 = -0.167$, confirming the sign reversal. This implies that one can consider the question of association reversal by comparing the inference results in (1) and (5).

In conclusion, it is very important to emphasize that in the above example, both LR models, (1) and (5), are assumed to be statistically adequate: their probabilistic assumptions are valid. In the case of real data on $(y_t, x_{1t}, x_{2t})$, $t=1,2,\ldots,n$, one needs to establish the statistical adequacy of both models using comprehensive misspecification testing. What are the probabilistic assumptions that need to hold for data $Z_0$?
3 Spurious (statistically untrustworthy) results

In this section we bring out more explicitly the probabilistic assumptions comprising the Linear Regression (LR) model with a view to illustrate the role of statistical misspecification in shedding light on the various aspects of Simpson’s paradox.

3.1 The statistical misspecification perspective

Traditionally, the probabilistic assumptions underlying the Linear Regression (LR) model are specified in terms of the error term; see Appendix. It turns out, however, that such specifications are often incomplete and sometimes include non-testable assumptions. Table 1 specifies the LR, generally defined by: in terms of the Statistical Generating Mechanism (GM) and assumptions [1]-[5] that constitute a complete, internally consistent and testable set of assumptions in terms of the observable process \( \{(y_t|X_t=x_t), t \in \mathbb{N} \} \) underlying the data \( Z_0 := \{(y_t, x_t), t=1, 2, ..., n \} \). This provides a purely probabilistic construal for the notion of a statistical model, viewed as a particular parameterization of the process \( \{(y_t|X_t=x_t), t \in \mathbb{N} \} \). Intuitively, the statistical model comprises the totality of probabilistic assumptions one imposes on the process \( \{(y_t|X_t=x_t), t \in \mathbb{N} \} \) with a view to render data \( Z_0 \) a ‘typical’ realization thereof.

The ‘typicality’ is testable using thorough misspecification testing; see Spanos (2006a).

### Table 1: Linear Regression Model

| Statistical GM: \( y_t = \beta_0 + \beta_1^T x_t + \epsilon_t, \ t \in \mathbb{N} \). |
|---|
| [1] Normality: \( (y_t|X_t=x_t) \sim N(., .) \), |
| [2] Linearity: \( E(y_t|X_t=x_t) = \beta_0 + \beta_1^T x_t \), |
| [3] Homoskedasticity: \( Var(y_t|X_t=x_t) = \sigma^2 \), |
| [4] Independence: \( \{(y_t|X_t=x_t), t \in \mathbb{N} \} \) indep. process, |
| [5] t-invariance: \( (\beta_0, \beta_1, \sigma^2) \) are not changing with \( t \), |
| \( \beta_0 = E(y_t) - \beta_1^T E(X_t), \ |
| \( \beta_1 = [Cov(X_t)]^{-1} Cov(X_t, y_t), \ |
| \sigma^2 = Var(y_t) - Cov(X_t, y_t)^T [Cov(X_t)]^{-1} Cov(X_t, y_t) \) |

**Statistical adequacy.** An estimated LR model is said to be *statistically adequate* when all assumptions [1]-[5] are valid for data \( Z_0 \). In practice, statistical adequacy can be appraised using comprehensive misspecification testing; see Spanos (1999, 2015). The importance of establishing statistical adequacy stems from the fact that it secures the statistical reliability of inference based on such a model. That is, the inference propositions associated with the LR model, including the optimal properties of the MLE estimators and the relevant error probabilities of the t and F tests, are reliable in the sense that their actual sampling distributions approximate closely the theoretical ones derived by invoking the validity of assumptions [1]-[5].

**Unreliability of inference.** When any subset of the assumptions [1]-[5] are invalid, the reliability of inference of such procedures is called into question. Statistical misspecifications are likely to give rise to inconsistent estimators as well
as induce sizeable discrepancies between the nominal (assumed) error probabilities and the actual ones in testing. For instance, when any of the assumptions [2], [4]-[5] are invalid, the OLS estimators of \((\beta_0, \beta_1)\) are likely to be inconsistent, and the nominal error probabilities associated with the significance t-tests for the coefficients \((\beta_0, \beta_1)\) are likely to have significant discrepancies from the actual error probabilities; see Spanos and McGuirk (2001), Spanos (2010). Applying a .05 significance level t-test when the actual type I error is closer to .8 is likely to give rise to unreliable inferences.

It is important to emphasize that for assumptions [4] and [5] to be testable, one needs to select an ordering of interest for data \(Z_0\). In the case of time-series data, the ordering of interest is invariably ‘time’, which is an interval scale variable. For cross-section data, however, there are often several orderings of interest, depending on the individual unit being observed, and the modeler needs to think about such potential orderings as they relate to [4]-[5]. Potential orderings for cross-section can vary from gender (nominal scale), to age (ratio scale), etc.

3.2 Statistical vs. substantive misspecification

Let us return to example 1, where the problem of association reversal can be viewed in the context of comparing the regression coefficients of \(x_{1t}\), \(\alpha_1\) and \(\beta_1\), in the context of two Linear Regression models:

Model 1: \[ y_t = \beta_0 + \beta_1 x_{1t} + \beta_2 x_{2t} + u_t, \quad (u_t | X_t = x_t) \sim \text{NIID}(0, \sigma_u^2), \quad t \in \mathbb{N}, \]

Model 2: \[ y_t = \alpha_0 + \alpha_1 x_{1t} + \varepsilon_t, \quad (\varepsilon_t | X_{1t} = x_{1t}) \sim \text{NIID}(0, \sigma_\varepsilon^2), \quad t \in \mathbb{N}, \]

where \(x_t = (x_{1t}, x_{2t})^T\). In the previous section, it was argued that when both models are statistically adequate, it could happen that the estimated coefficients \(\alpha_1\) and \(\beta_1\) differ in both sign and magnitude. There is, however, a sizeable literature on ‘omitted variables’ which would call model 2 misspecified when \(\beta_2\) turns out to be statistically significant; see Greene (2011). In what sense is model 2 misspecified if its assumptions [1]-[5] (table 1) are valid? Similarly, the literature on causal modeling would test the significance of the covariances \(\sigma_{13}\) and \(\sigma_{23}\) as they relate to the regression coefficients, to decide whether \(x_{2t}\) is a confounder; see Pearl (2011). How does this relate to the statistical misspecification perspective?

A closer look at the literature suggests that statistical misspecification is often conflated with substantive misspecification, using confusing and confused claims, such as the OLS estimator of \(\alpha_1\) in model 2 is an inconsistent estimator of \(\beta_1\) in model 1 (Greene, 2011), ignoring the fact that the two coefficients represent very different parameterizations: \(\alpha_1 = \frac{\sigma_{12}}{\sigma_{22}}, \quad \beta_1 = \frac{\sigma_{12}(\sigma_{33} - \sigma_{13}\sigma_{23})}{(\sigma_{22}\sigma_{33} - \sigma_{23}^2)}.\)

To make any sense of such comparisons, one needs to distinguish between statistical and substantive adequacy because the former requires only that assumptions [1]-[5] are valid for \(Z_0\). Assumptions [1]-[5] have nothing to do with: the LR model includes all ‘substantively’ relevant variables. The latter is a substantive assumption that pertains to the explanatory potential of the estimated model as it relates to the phenomenon of interest. Substantive inadequacy can arise from missing but relevant variables, false causal claims, etc. The crucial
importance of this distinction stems from the fact that when models 1-2 are
statistically misspecified, both the test for an omitted variable, as well as the
tests for deciding whether $x_{2t}$ is a confounder, or a mediator, are likely to give
rise to untrustworthy results; see Spanos (2006b).

This distinction is also important when the term ‘spurious’ is employed without
being qualified to differentiate between statistically and substantively spu-
rious inference results. Indeed, the term ‘spurious correlation’ is often used to
describe the case where the statistical significance of a correlation coefficient is
taken at face value, and an attempt is made to explain it away using substantive
arguments; see Sober (2001). More often than not, however, one can show that
the statistical significance is more apparent than real, because it is just an
untrustworthy result stemming from a statistically misspecified model.

3.3 Example 2. Yule’s ‘nonsense-correlations’
The problem of ‘spurious’ associations, first noted by Pearson (1896), was high
up in Yule’s agenda during the first quarter of the 20th century, returning to it on
several occasions; see Yule (1909, 1910, 1921). Yule (1926) is the culmination of
his efforts to unravel the puzzle of ‘spurious’ results using the high correlations
between time series data as an example. He used data measuring the ratio
of Church of England marriages to all marriages ($x_t$) and the mortality rate
($y_t$) over the period 1866-1911, to demonstrate that their estimated correlation
$\hat{\rho}_{xy}=0.9512$ was both very high and statistically significant. He described this
result as ‘nonsense-correlation’ because ‘common sense judges to be incorrect.’
(p.4) He went on to reject any attempt, however ingenious, to rationalize such
a statistical result on substantive grounds:

“Now I suppose it is possible, given a little ingenuity and goodwill, to rationalize
very nearly anything. And I can imagine some enthusiast arguing that the fall in the
proportion of Church of England marriages is simply due to the Spread of Scientific
Thinking since 1866, and the fall in mortality is also clearly to be ascribed to the
Progress of Science; hence both variables are largely or mainly influenced by a
common factor and consequently ought to be highly correlated. But most people
would, I think, agree with me that the correlation is simply sheer nonsense; that
it has no meaning whatever; that it is absurd to suppose that the two variables in
question are in any sort of way, however indirect, causally related to one another.”
(p. 2)

Yule (1926) attempted to articulate the premise that ‘nonsense-correlations’
have something to do with the fact that his time series data are not ‘random
series’. He could not establish a clear and direct link between ‘spurious’ associ-
ations and statistical misspecification, however, because he was missing two key
components that were yet to be integrated into statistics. The first is the notion
of a ‘parametric statistical model’, innovated by Fisher (1922), and the second
is the theory of ‘stochastic processes’ founded by Kolmogorov (1933). The for-
erm comprises all the probabilistic assumptions imposed on the data, and the
latter formalizes the notions of a ‘random series’ into a realization of an IID
stochastic processes, as well as departures from it in the form of probabilistic
concepts for dependence and heterogeneity.

7
Yule’s reverse engineering. Given that there was no notion of a pre-specified parametric statistical model, comprising the probabilistic assumptions imposed on the data, Yule resorted to ‘reverse engineering’:

“When we find that a theoretical formula applied to a particular case gives results which common sense judges to be incorrect, it is generally as well to examine the particular assumptions from which it was deduced, and see which of them are inapplicable to the case in point.” (p. 4-5)

He went on to consider the formula for estimating the sample standard error and elicit the implicit probabilistic assumptions that render it a ‘good’ estimator of the distribution standard error. Let us emulate Yule’s reverse engineering using the sample correlation coefficient, which is the focus of his paper:

\[
Corr(X_t, Y_t) = \frac{\frac{1}{n} \sum_{t=1}^{n} (Y_t - \bar{Y})(X_t - \bar{X})}{\sqrt{\left[\frac{1}{n} \sum_{t=1}^{n} (X_t - \bar{X})^2\right] \left[\frac{1}{n} \sum_{t=1}^{n} (Y_t - \bar{Y})^2\right]}}
\]

\[\bar{X} = \frac{1}{n} \sum_{t=1}^{n} X_t, \quad \bar{Y} = \frac{1}{n} \sum_{t=1}^{n} Y_t, \quad Var(X_t) = \frac{1}{n} \sum_{t=1}^{n} (X_t - \bar{X})^2, \quad \]

\[Var(Y_t) = \frac{1}{n} \sum_{t=1}^{n} (Y_t - \bar{Y})^2, \quad Cov(X_t, Y_t) = \frac{1}{n} \sum_{t=1}^{n} (Y_t - \bar{Y})(X_t - \bar{X}), \]

as a ‘good’ estimator of the distribution correlation coefficient:

\[
Corr(X_t, Y_t) = \frac{Cov(X_t, Y_t)}{\sqrt{Var(X_t)Var(Y_t)}}
\]

The first assumption implicit in these formulae is the constancy of the moments:

\[E(Y_t) = \mu_1, \quad E(X_t) = \mu_2, \quad Var(Y_t) = \sigma_{11}, \quad Var(X_t) = \sigma_{22}, \quad Cov(X_t, Y_t) = \sigma_{12}, \quad t \in \mathbb{N}, \]

which corresponds to a form of the ID assumption. The formulae for \(Var(X_t)\) and \(Var(Y_t)\), implicitly assume non-correlation over \(t \in \mathbb{N}\), otherwise they should have included covariances over \(t \in \mathbb{N}\) terms. Yule also sought to unveil the implicit distributional assumption “in order to reduce the formula to the very simple form given.” (p. 5) The sample moments are not always ‘optimal’ estimators of the distribution moments. For instance, the estimators in (7) will be ‘optimal’ under Normality, but they will be non-optimal if the distribution is Uniform; see Carlton (1946).

In light of the fact that under Normality the assumption of ID reduces to the constancy of the first two moments, and non-correlation coincides with Independence, one could make a case that the implicit parametric statistical model underlying the above formulae is the simple bivariate Normal in table 2.

| Table 2 - The simple (bivariate) Normal model |
|------------------------------------------------|
| Statistical GM: | \(Z_t = \mu + \epsilon_t,\) |
| 1 Normal: | \(Z_t \sim \mathcal{N}(\mu, \Sigma),\) |
| 2 Constant mean: | \(E(Z_t) = \mu,\) |
| 3 Constant covariance: | \(Var(Z_t) = \Sigma,\) |
| 4 Independence: | \(\{Z_t, t \in \mathbb{N}\}\) is independent. |

\[Z_t := \begin{pmatrix} y_t \\ X_t \end{pmatrix}, \quad \mu := \begin{pmatrix} \mu_1 \\ \mu_2 \end{pmatrix}, \quad \Sigma := \begin{pmatrix} \sigma_{11} & \sigma_{12} \\ \sigma_{12} & \sigma_{22} \end{pmatrix}\]
When any of the assumptions [1]-[4] are invalid for the particular data $Z_0$, the estimated correlation coefficient is likely to be ‘spurious’ (statistically untrustworthy). Granted, certain departures from particular assumptions, such as [2]-[4], are more serious than other departures, say from [1]. A glance at the t-plots of Yule’s (1926) data suggests, to borrow his phrase on p. 5, that:

“Neither series, obviously, in the least resembles a random series” (aka IID).

Fig. 1: t-plot of $x_t$, ratio of Church of England marriages to all marriages
Fig. 2: t-plot of $y_t$, the mortality rate for the period 1866-1911

Both data series exhibit clear departures from IID (fig. 6) in the form of mean $t$-heterogeneity (trending mean) and dependence (irregular cycles). To bring out the cycles in the original data more clearly one needs to subtract the trending means using, say, a generic 3rd degree trend polynomial.

Fig. 3: t-plot of detrended $x_t$  
Fig. 4: t-plot of detrended $y_t$

In light of the direct relationship between the correlation ($\rho_{12}$) and the regression coefficient ($\beta_1$) in (9), one can pose the question of statistical adequacy in the context of the Linear Regression model, which will yield:

$$y_t = -10.847 + .419 x_t + \hat{u}_t, \quad R^2 = .905, \quad s = .664, \quad n = 46, \quad (9)$$
where the standard errors are reported in brackets below the coefficient estimates. Both coefficients \((\beta_0, \beta_1)\) seem statistically significant since the t-ratios are:

\[
\tau_0(z_0) = \frac{10.847}{1.241} = 8.660 [.000], \quad \tau_1(z_0) = \frac{4.19}{.027} = 20.95 [.000],
\]

and the p-values are given in square brackets. Note that the implied correlation (see (7)) yields the value in Yule (1926): \(\hat{\rho}_{xy} = .419(1.834) = .952 [.000] \).

A glance at the t-plot of the residuals (fig. 5), however, indicates that (9) is statistically misspecified; assumptions [4]-[5] are likely to be invalid. The residual t-plot differs from that of a NIID realization (fig. 6) in so far as it exhibits distinct trends and cycles. These misspecifications are confirmed formally by the statistical significance of the trends and lags in the auxiliary regression based on the residuals \((\hat{u}_t)\) from (9):

\[
\hat{u}_t = 11.987 - .413 x_t - 1.670 t - .406 t^2 + .885 y_{t-1} + .006 x_{t-1}
\]

These results suggest that the estimator of \(\beta_1\) is inconsistent, and the t-test for its significance is statistically untrustworthy. Taking mean deviations from \((x, y)\) when the actual means are trending, will render all the above estimators in (7) inconsistent.

In light of these departures from the IID assumptions, (7) is an inconsistent estimator of the correlation coefficient, and thus statistically spurious. Indeed, one can easily show that when the data are de-trended and de-memorized (subtract the temporal dependence using 2 lags) to render (7) an appropriate estimator, the estimated correlation is: \(\hat{\rho}_{xy} = .003 [.985]\), which is totally statistically insignificant.

In summary, the notion of statistical adequacy provides a direct and testable link between statistical misspecification and statistically untrustworthy (spurious) associations, or inference results more generally. A likely criticism of this link is that the probability assumptions of the assumed model in (8) are too strong, in contrast to the current statistical practice favoring as weak a set of assumptions as possible. The short reply to such a charge is that weaker but non-testable assumptions (i) do not render the assumed model less vulnerable to statistical misspecifications, and (ii) they underestimate the importance
of securing statistical adequacy. In addition, weak assumptions often rely on asymptotic sampling distributions without testing the validity of the assumptions invoked by limit theorems; see Spanos (2015). The truth of the matter is that the trustworthiness of all inference results will rely exclusively on the approximate validity of the probabilistic assumptions imposed on $\mathbf{Z}_0$, and nothing else. As argued by Le Cam (1986, p. xiv): “... limit theorems ”as n tends to infinity” are logically devoid of content about what happens at any particular $n$.

### 3.4 Example 3. The third variable reversion

In this sub-section we consider an empirical example based on cross-section data because statistical adequacy is less well appreciated in such a context.

Consider the case where a practitioner wants to evaluate the effect of education on a person’s income. The data refer to education, $x_t$-years of schooling, and income, $y_t$-thousands of dollars, for $n=100$ working people within the age group of 30-40 years old selected from a city’s population. The estimated LR model yields:

$$ y_t = 53.694 - 0.474 x_t + \hat{u}_t, \quad R^2 = 0.996, \quad s = 3.307, \quad n = 100. \quad (11) $$

Both coefficients ($\beta_0, \beta_1$) appear to be statistically significant since the t-ratios are:

$$ \tau_0(z_0) = \frac{53.694}{1.957} = 27.437[,0000], \quad \tau_1(z_0) = \frac{474}{1.147} = 3.224[.001]. $$

The practitioner is surprised by the negative sign of the coefficient of $x_t$, since that implies that additional years of education contribute negatively to one’s income. He takes a closer look at the data and decides to run separate linear regressions for men ($n_1=50$) and women ($n_2=50$).

The estimated LR model for men yields:

$$ y_{1t} = 45.229 + 0.409 x_{1t} + \hat{u}_{1t}, \quad R^2 = 0.973, \quad s = 2.371, \quad n_1 = 50. \quad (12) $$

The estimated LR model for women yields:

$$ y_{2t} = 35.106 + 0.675 x_{2t} + \hat{u}_{2t}, \quad R^2 = 0.193, \quad s = 2.124, \quad n_2 = 50. \quad (13) $$

The estimation results in (12)-(13) indicate that for both estimated regressions:

- the coefficients ($\beta_0, \beta_1$) are statistically significant, and
- the sign of the coefficient $\beta_1$, of education variable ($x_t$), is positive.

The positive sign of the estimated $\beta_1$ clearly contradicts the negative sign in (11), which is usually interpreted as a case where a statistical association is reversed. This is considered as an example of Simpson’s paradox when viewed from perspective (b), where gender ($D_t$) is viewed as a confounding variable that correlates with both $y_t$-income and $x_t$-education. In econometrics, this is usually viewed as a case of ‘omitted-variable bias’; see Greene (2011). According to Pearl (2014), p. 10, the only way to decide whether to rely on the aggregated data regression in (11) or the disaggregated data regressions (12)-(13) is to use causal calculus.
Upon reflection, however, the statistical misspecification perspective provides an alternative way to resolve the paradox on statistical adequacy grounds. The above estimation and testing results in (11)-(13) are trustworthy only when the model assumptions [1]-[5] are valid for the particular data for each of the three estimated equations. Estimating the aggregated data equation (11) using ‘gender’ as the ordering of interest, and plotting the residuals (fig. 7) suggests that (11) is statistically misspecified because the t-plot is far from being Normal white-noise. Assumption [5] is clearly invalid since its sample mean is not constant around zero, but shifts from positive for the first half to negative for the second, and the variance appears smaller for the second half; see Spanos (1999), ch. 5. This form of t-heterogeneity differs from that in Yule’s data discussed above.

This is confirmed by the auxiliary regression using the residuals ($\hat{u}_t$):

$$\hat{u}_t = -15.986 + .967 x_t + 6.556 D_t, \quad R^2=.54, \quad s=2.263, \quad n=100,$$

where $D_t := (1, 1, ..., 1, 0, 0..., 0)$, 1-male, 0-female, since its coefficient is statistically significant: $\tau_3(z_0) = ^{6.556}_{.616} = 10.643[.0000]$.

This suggests that the statistical misspecification perspective provides a very different interpretation of the reversion results and offers an alternative way to resolve the apparent paradox.

First, the key to resolving any seemingly conflicting inference results is not the notion of ‘confounding’ (Pearl, 2014), but that of statistical adequacy. Before one can talk about any form of reversal of a statistical association, one needs to establish that all the associations involved are statistically trustworthy. Any claim that there is a ‘reversal of association’ between equation (11) and (12)-(13) is misleading since the aggregated data equation (11) is statistically misspecified. Therefore, the inference that the coefficient of $x_t$ is negative and statistically significant is untrustworthy; an artifact of imposing invalid probabilistic assumptions on data $z_0$. Hence, the aggregate data misrepresent the relationship between $y_t$ and $x_t$. 

Fig. 7: Residuals from equation (11)
Second, the diagnosis that the variable ‘gender’, represented by $D_t$ is a missing ‘confounder’ seems rather misleading for two reasons. The information pertaining to the ordering(s) of potential interest is already in the original data $z_t$ (see figures 8-9). In addition, defining the confounder as an omitted variable $Z$ which is related to the included variables $X$ and $Y$ in the right way, requires that $Z$ is stochastic variable, not a deterministic ordering. For statistical inference purposes, the inclusion of generic terms such as shifts in the mean, trends and lags in the estimated equation could, in certain cases, secure statistical adequacy, without having to resort to finding additional explanatory variables.

In the case of (11), a more pertinent explanation is that the modeler neglected, or chose to ignore, the heterogeneity in the data by assuming constant mean and variance (ID) for both data series with respect to the ordering, gender. Such forms of misspecification pertain to statistical information contained in the data, which could be generically modeled using shift functions or/and trend polynomials in $t$ or/and lags, respectively; see Spanos (1999).

In the case of example 3, one could attempt to respecify the original equation in (11) by including the dummy variable ($D_t$):

$$y_t = 37.639 + 6.556D_t + .501 x_t + \hat{u}_t, \quad R^2=.58, \quad s=2.272, \quad n=100.$$  

(14)

As it stands, the coefficient of $x_t$ represents an misleading weighted average of the two coefficients from the disaggregated data in (12)-(13). The residuals from this equation (fig. 10) do not indicate any major departures from assumptions [1]-[5], but in practice one needs to apply thorough misspecification testing to confirm or deny such a claim. For instance, one needs to test that the variances of the residuals in the two sub-samples are equal; see Spanos (1986), p. 481-3.

Finally and most importantly, using the statistical misspecification perspective, one can distinguish clearly between example 1 (section 2), and examples 2 and 3 above. The key difference is that in example 1 both LR models (1) and (5) are statistically adequate. In contrast, in example 3 the estimated LR model (11) based on aggregated data, is statistically misspecified which renders the estimated coefficients and t-tests statistically untrustworthy. Hence, there was never a statistically trustworthy result at the aggregate level that gave rise...
to a reversal of associations. In example 3, only the disaggregated data give rise
to statistically reliable inferences. This calls seriously into question the conven-
tional wisdom that these two cases as identical, as stated by Samuels (1993), p.
87:

“Simpson’s paradox is actually no more paradoxical than the reversal or distortion
of association in other settings, no more, for instance, than the familiar fact that
a partial regression coefficient can have a different sign from a simple regression
coefficient.”

In concluding this section, it is important to emphasize that the statistical
misspecification perspective requires one to know the complete set of proba-
bilistic assumptions imposed on the data, i.e. the statistical model. More often
than not, practitioners have an incomplete picture of the statistical model, they
rarely test its assumptions, and thus the ensuing inference results are often un-
trustworthy. Hence, in evaluating published empirical papers, it is sometimes
useful to employ Yule’s reverse engineering to uncover the statistical model.

4 Misspecified Bernoulli models
In this section we will revisit two cross-section data sets that have been widely
discussed in the statistics literature, using the statistical misspecification per-
spective.

4.1 Example 4. The UC Berkeley admissions data
Bickel et al. (1975) published an influential paper in Science, where they il-
lustrated Simpson’s paradox using cross-section data based on UC Berkeley
admissions, for the Fall of 1973. Their perspective relates to perspective (a)
and pertains to the reversal of a statistical relationship between the aggregated
data, at the university level, and the disaggregated data, at the department
level. The aggregate data are shown in table 3 and the data for the largest 5
departments, denoted by A-F, are given in table 4:

see https://en.wikipedia.org/wiki/Simpson%27s_paradox.
The estimated parameter $\theta = \Pr(X=1)$ based on the aggregate data (table 3) indicates that the rate of admissions for female candidates ($\hat{\theta}_F = .35$) is smaller than for male candidates ($\hat{\theta}_M = .44$), and a test for the difference indicated a statistically significant difference; see Bickel et al. (1975). At the department level, however, the admissions rate for females is greater than that of males in five out of the six departments shown in table 4. This is interpreted as an apparent reversal of the inference based on the aggregate data. The statistical misspecification perspective, however, suggests that the estimated admissions rate using the aggregated data is statistically untrustworthy. Let us unpack this claim.

### Table 3: Admissions Aggregate Data

|       | Males | Females | total |
|-------|-------|---------|-------|
| Admit | 3738  | 1494    | 5232  |
| Deny  | 4704  | 2827    | 7531  |
| Total | 8442  | 4321    | 12763 |

$\hat{\theta}_M = \frac{3738}{8442} = .44$, $\hat{\theta}_F = \frac{1494}{4321} = .35$

### Table 4: Admissions disaggregated data for departments A-F

|   | Males | Females | total |
|---|-------|---------|-------|
| A | Admit | 512     | 89    | 601   |
|   | Deny  | 313     | 19    | 332   |
|   | Total | 825     | 108   | 933   |

$\hat{\theta}_{AM} = \frac{512}{825} = .62$, $\hat{\theta}_{AF} = \frac{89}{108} = .82$

|   | Males | Females | total |
|---|-------|---------|-------|
| B | Admit | 353     | 17    | 370   |
|   | Deny  | 207     | 8     | 215   |
|   | Total | 560     | 25    | 585   |

$\hat{\theta}_{BM} = \frac{353}{560} = .63$, $\hat{\theta}_{BF} = \frac{17}{25} = .68$

|   | Males | Females | total |
|---|-------|---------|-------|
| C | Admit | 120     | 202   | 322   |
|   | Deny  | 205     | 391   | 596   |
|   | Total | 325     | 593   | 918   |

$\hat{\theta}_{CM} = \frac{120}{325} = .37$, $\hat{\theta}_{CF} = \frac{202}{593} = .34$

|   | Males | Females | total |
|---|-------|---------|-------|
| D | Admit | 139     | 131   | 270   |
|   | Deny  | 278     | 244   | 522   |
|   | Total | 417     | 375   | 792   |

$\hat{\theta}_{DM} = \frac{139}{417} = .33$, $\hat{\theta}_{DF} = \frac{131}{375} = .35$

|   | Males | Females | total |
|---|-------|---------|-------|
| E | Admit | 53      | 94    | 147   |
|   | Deny  | 138     | 199   | 337   |
|   | Total | 191     | 293   | 484   |

$\hat{\theta}_{EM} = \frac{53}{191} = .28$, $\hat{\theta}_{EF} = \frac{94}{293} = .32$

|   | Males | Females | total |
|---|-------|---------|-------|
| F | Admit | 22      | 23    | 45    |
|   | Deny  | 351     | 318   | 669   |
|   | Total | 373     | 341   | 714   |

$\hat{\theta}_{FM} = \frac{22}{373} = .06$, $\hat{\theta}_{FF} = \frac{23}{341} = .07$

What is missing from the discussion of the traditional association reversal interpretation is any evidence that the above inferences based on the estimated $\theta$ is trustworthy. Such evidence can be secured by testing the validity of the
assumptions invoked by the above inferences, which comprise the underlying statistical model: a bivariate version of the simple Bernoulli model (table 5), with $\theta$ replaced with a vector of unknown parameters $\theta := (\theta_{00}, \theta_{01}, \theta_{10}, \theta_{11})$; see Bishop et al. (1975).

In relation to the Bernoulli model, it is important to point out that $\theta$ is also the mean of process underlying the data, as well as determining the variance, i.e.

$$E(X_t) = \theta, \ Var(X_t) = \theta(1 - \theta), \ 0 \leq \theta \leq 1, \ \forall t \in \mathbb{N}.$$ 

| Table 5 - The simple Bernoulli model |
|--------------------------------------|
| Statistical GM: $X_t = \theta + u_t$, |
| [1] Bernoulli: $X_t \sim Ber(\theta)$, |
| [2] Constant mean: $E(X_t) = \theta$ |
| [3] Constant variance: $Var(X_t) = \theta(1 - \theta)$ |
| [4] Independence: $\{X_t, \ t \in \mathbb{N}\}$ is independent. |

A glance at the estimated $\hat{\theta}$ for males and females at the department level in table 4, indicate clearly that the estimated means and variances differ, not only from those based on aggregate data, but also between departments. This renders assumptions [2] and [3] invalid at the aggregate level. That is, when the data are aggregated the process $\{X_t, \ t \in \mathbb{N}\}$ is no longer Identically Distributed (ID) with respect to the ordering ‘gender’.

In light of this, the association reversal is spurious because the estimated values:

$$\hat{\theta}_M = \frac{3738}{8442} = .44, \ \hat{\theta}_F = \frac{1494}{4321} = .35$$

from the aggregated data. This is because the estimators of $\theta$ using the aggregated data will be inconsistent estimators of the true $\theta$.

Fig. 11: t-plot of $X_t \sim BerIID(\theta, .16)$  
Fig. 12: t-plot of $Z_t \sim BerIID(\theta, .24)$

To see how this arises in practice, consider figures 11-12 that represent the t-plots of two Bernoulli IID $[BerIID(E(X_t), \ Var(X_t))]$ processes with $\theta = .2$ and $\theta = .6$, respectively.
As can be seen from these figures, the concentration of longer ‘runs’ [group of successive values of 0’s or 1’s] switches from the value 0 to the value 1 as \( \theta \) increases above .5. Hence, any attempt to ignore the differences in the two moments of such processes will give rise to a misspecified Bernoulli model. That invalidates any inferences based on the aggregate data, and the only potentially reliable inference can be drawn from the disaggregated data.

4.2 Example 5. The Lindley and Novick (1981) data
The above statistical misspecification perspective can be used to explain the seemingly contradictory results in Lindley and Novick’s (1981) hypothetical data shown below. This is a particularly interesting example because, as argued by Armistead (2014), the ordering of interest might become apparent after the data are collected. For instance, in a clinical trial the ‘gender’ or/and ‘age’ ordering(s) might turn out to be relevant after the data are collected.

The estimated \( \theta \)'s for the aggregated data in table 6:

\[
\hat{\theta}_W = \frac{20}{36} = 0.5, \quad \hat{\theta}_B = \frac{16}{40} = 0.4,
\]

are very different from those based on the disaggregated data (table 7), rendering the former statistically untrustworthy because it imposes an invalid assumption: the means of the Bernoulli process underlying the disaggregated data are constant.

| Table 6: Lindley-Novick Aggregated Data |
|-----------------|-----------------|-----------------|-----------------|
| White | Black | total |
| High | 20 | 16 | 36 |
| Low | 20 | 24 | 44 |
| Total | 40 | 40 | 80 |
| \( \hat{\theta}_W = \frac{20}{36} = 0.5, \hat{\theta}_B = \frac{16}{40} = 0.4 \) |

| Table 7: Lindley-Novick disaggregated data |
|-----------------|-----------------|-----------------|-----------------|
| Short | White | Black | total |
| High | 2 | 9 | 11 |
| Low | 8 | 21 | 29 |
| Total | 10 | 30 | 40 |
| \( \hat{\theta}_{SW} = \frac{2}{10} = 0.2, \hat{\theta}_{SB} = \frac{9}{30} = 0.3 \) |

| Tall | White | Black | total |
| High | 18 | 7 | 25 |
| Low | 12 | 3 | 15 |
| Total | 30 | 10 | 40 |
| \( \hat{\theta}_{TW} = \frac{18}{30} = 0.6, \hat{\theta}_{TB} = \frac{7}{10} = 0.7 \) |

5 Revisiting the causal modeling ‘solution’
Pearl’s (2014) claims that the only way to resolve the paradox is to use causal calculus:

“I am not aware of another condition that rules out effect reversal with comparable assertiveness and generality, requiring only that \( Z \) not be affected by our action, a requirement satisfied by all treatment-independent covariates \( Z \). Thus, it is hard, if not impossible, to explain the surprise part of Simpson’s reversal without postulating that human intuition is governed by causal calculus together with
a persistent tendency to attribute causal interpretation to statistical associations.” (p. 10)

Viewing examples 2-5 from the misspecification perspective, however, lends support to the Armistead’s (2014) key argument:

“Whether causal or not, third variables can convey critical information about a first-order relationship, study design, and previously unobserved variables. Any conditioning on a nontrivial third variable that produces Simpson’s Paradox should be carefully examined before either the aggregated or the disaggregated findings are accepted, regardless of whether the third variable is thought to be causal. In some cases, neither set of data is trustworthy; in others, both convey information of value.” (p. 1)

Indeed, in cases where the ‘third variable’ represents an ordering of potential interest for the particular data, the only relevant criterion to decide which orderings are relevant for the statistical analysis of the particular data is the statistical adequacy of the estimated equations. That is, when two or more alternative orderings are potentially relevant for a particular data set, one needs to test the statistical adequacy of all three equations relative to each of these orderings before one could draw reliable conclusions concerning how to resolve any apparently paradoxical results. Where does this leave the Pearl (2014) claim quoted above?

5.1 Statistical vs. substantive adequacy

Cartwright (1979) rightly points out that reliance on regularities and frequencies for statistical inference purposes is not sufficient for representing substantively meaningful causal relations. On the other hand, imposing causal relations that belie the chance regularities in the data would only give rise to untrustworthy inference results. While the causal dimension remains an important component in delineating the issues raised by Simpson’s paradox, it is not the only relevant, or even the most important, dimension in unraveling the puzzle. Indeed, the suggestion that in cases where the third variable (ordering of interest) is non-causal one should accept the results based on the aggregated data (Pearl, 2009), is called into question by examples 2-5. This is because when the model estimated using the aggregated data is statistically misspecified, the causal inference results pertaining to conditional independence are likely to be untrustworthy. One way or another, the modeler needs to account for the statistical information not accounted for by the original statistical model, with a view to ensure the trustworthiness of the ensuring statistical results.

It is interesting to note that Yule (1926) considered the third variable causal explanation, but questioned its value as a general ‘solution’ to the problem:

“Now it has been said that to interpret such correlations as implying causation is to ignore the common influence of the time-factor. While there is a sense – a special and definite sense – in which this may perhaps be said to cover the explanation, as will appear in the sequel, to my own mind the phrase has never been intellectually satisfying.” (p. 4)

A crucial issue that needs to be addressed by the causal explanation is that conditioning on a third variable is not as straightforward as adherents to this
‘explanation’ of Simpson’s paradox would have us believe. In practice, the question whether a particular variable $Z_t$ constitutes a confounder is not just a matter of testing whether $Z_t$ relates to $Y_t$ and $X_t$ the right way; see Pearl (2009), Spirtes et al. (2000). Before such testing can even begin, one needs to test for the statistical adequacy of the estimated model with respect to a relevant ordering. Although ‘time’ is the obvious ordering for time series, it is no different than other deterministic orderings for cross-section data such as ‘gender’, marital status, age, geographical position, etc.; only the scale of measurement differs. When the original model is statistically misspecified, it needs to be respecified with a view to secure statistical adequacy. Often one can restore statistical adequacy using generic terms relating to that ordering. To secure substantive adequacy, however, one needs to replace such generic terms with proper explanatory random variables without foregoing the statistical adequacy. The latter ensures the reliability of testing whether $Z_t$ is a confounder or not; see Spanos (2006b).

Yule (1926) considered ‘time’ as a third variable and expressed his misgivings:

“I cannot regard time per se as a causal factor; and the words only suggest that there is some third quantity varying with the time to which the changes in both the observed variables are due.” (p. 4)

Viewing his comment from the vantage point of today’s probabilistic perspective, the proposal to ‘condition’ on a third variable raises technical issues, since the conditional distribution, defined by:

$$f(y_t|x_t,d_t;\varphi)=\frac{f(y_t,x_t,d_t;\psi)}{f(d_t;\phi)}, \forall y_t\in\mathbb{R}Y,$$

makes no probabilistic sense when $d_t$ is a deterministic ordering (variable) such as time; see Williams (1991). This issue arises more clearly in cases where the ordering was deemed potentially important after the data have been collected, such as having plants grow short or tall, blood pressure being high or low, black or white plants, etc.; see Armistead (2014). How does one bridge the gap between a deterministic ordering of interest and conditioning on a third random variable related to that ordering?

Separating modeling from inference. The statistical misspecification perspective suggests that to ensure the reliability of inference one needs to separate the initial stages of specification (initial model selection) misspecification testing and respecification, from inference proper. The latter includes testing for substantive adequacy, such as attributing causality to statistical associations. In practice, this requires focusing first on the ordering(s) of interest that could potentially reveal statistical misspecifications that pertain to dependence and heterogeneity uncovered by misspecification testing. The next step is to respecify the initial model with a view to account for the statistical information revealed by the misspecification testing. This is usually achieved by employing generic terms, such as shifts, trends and lags, to ‘capture’ such forms of systematic statistical information. Once statistical adequacy is secured one can then proceed to ‘model’ such information by replacing the generic terms with appropriate explanatory variables with a view to improve the substantive adequacy.
without forgoing the statistical adequacy. This is because a third degree trend polynomial might capture the mean heterogeneity in the data to ensure the statistical reliability of inference, but from the substantive perspective it represents ignorance. Replacing the trend polynomial with explanatory variables without forsaking statistical adequacy will add to our understanding of the phenomenon of interest; see Spanos (2010).

Viewing the problem from a broader perspective, the primary reason for the untrustworthiness is that the question of probing for the nature of any causal connections pertains to substantive, and not statistical adequacy, even though the distinction between the two might not always be clear cut or obvious; see Spanos (2010). This distinction is crucial because any attempt to probe for substantive adequacy, including causal connections, before securing statistical adequacy of the assumed statistical model is likely to give rise to unreliable results. To avoid this problem of unreliable inferences, one needs to establish the statistical adequacy of the original model first before probing for any form of substantive adequacy, such as attributing a causal interpretation to statistical associations. These include probing for the appropriateness of a particular confounder or choosing between different potential confounders; see Spanos (2006b) for an extensive discussion.

This distinction is crucial in differentiating between statistically and substantively ‘spurious’ inferential results. Unfortunately, in the statistics and philosophy of science literatures the term ‘spurious’ is often used to describe the latter; see Blyth (1972). What is often insufficiently appreciated is that one needs to establish first that there is a statistically trustworthy statistical association, before attempting to explain it away as substantively spurious.

The statistical misspecification perspective also calls into question certain philosophical discussions of Simpson’s paradox that focus primarily on the ‘numbers’ associated with the relevant probabilities/associations as in the case of example 1. A typical representation of Simpson’s paradox in terms of events $A, B, C$ is:

$$P(A|B) < P(A|\neg B), \text{ but } P(A|B, C) > P(A|\neg B, C) \text{ and } P(A|B, \neg C) > P(A|\neg B, \neg C),$$

where ‘$\neg$’ denotes the ‘negation’ operator. Malinas and Bigelow (2016) illustrate (15) using made up numbers that satisfy the above inequalities, and describe the source of the paradox as follows:

“The applications arise from the close connections between proportions, percentages, probabilities, and their representations as fractions.” (p. 1)

They proceed to claim that their artificial illustration provides a way to explain an empirical example from Cohen and Nagel (1934) concerning death rates in 1910 from tuberculosis in Richmond, Virginia and New York city. As argued above, however, in the case of observed data some of the ‘numbers’ used in such arguments might be statistically untrustworthy, undermining the soundness of the logical argument in (15). Indeed, oversimplifications of the form (15), contribute to the perpetuation of the misconceptions beleaguering the paradox.
6 Summary and conclusions

What is often insufficiently appreciated in statistical modeling and inference is that the inference propositions (optimal estimators, tests, and predictors and their sampling distributions) depend crucially on the validity of the probabilistic assumptions one imposes on the data. The totality of these assumptions comprise the underlying statistical model, which is used to define the distribution of the sample and the likelihood function. If the statistical model is misspecified, in the sense that any of its assumptions are invalid for the particular data, the reliability of inference based on such a model is usually undermined, giving rise to untrustworthy evidence.

The paper revisited Simpson’s paradox using the statistical misspecification perspective with a view to shed light on several silent features of the paradox. Using this perspective, it was argued that the key to unraveling the various counterintuitive results associated with this paradox is to formalize the vague notion of ‘spurious’ inference results into ‘statistically untrustworthy’ results which can be evidenced using misspecification testing. This enables one to distinguish between two different cases of the paradox as it relates to the reversal of statistical associations. Case 1, where the reversal is statistically trustworthy because the underlying statistical models are statistically adequate (example 1). Case 2, where the apparent reversal is statistically untrustworthy due to statistical misspecification (examples 2-5). The real issue is whether the inference results pertaining to statistical associations are statistically trustworthy or not, and the key criterion to appraise that is statistical adequacy. Hence, the statistical misspecification perspective puzzles out Simpson’s paradox because in both cases there is nothing counterintuitive to explain.

The statistical misspecification perspective is also used to revisit the causal dimension of the paradox by distinguishing between statistical and substantive inadequacy (spuriousness). To ensure the reliability of any inferences relating to testing whether a third variable constitutes a confounder, requires that the underlying statistical model is statistically adequate. This is particularly problematic for the causal resolution of the paradox when the third variable is related to a relevant ordering of interest which is revealed after the data are collected. In such cases one needs to account for any departures from the model assumptions as they relate to the ordering in question, and replace the generic terms used to capture the neglected statistical information with substantively meaningful explanatory variables.

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7 Appendix: The Linear Regression - implicit statistical parameterizations

The traditional specification of the LR model takes the form:

\[ y_t = \beta_0 + \beta_1^\top x_t + u_t, \]

[i] \( E(u_t|x_t=x_t)=0, \) [ii] \( E(u_t^2|x_t=x_t)=\sigma^2, \)

[iii] \( E(u_t u_s|x_t=x_t)=0, \) [iv] \( u_t \sim N(.,.), t \in \mathbb{N}. \)

**Theorem 1** Assumptions [i]-[iii] relating to the first two moments of the conditional distribution \( f(u_t|x_t; \theta) \), imply that the model parameters \( \theta:=(\beta_0, \beta_1, \sigma^2) \) have the following statistical parameterizations in terms of the primary parameters of the joint distribution \( f(y_t, x_t; \phi) \), \( \phi:=(E(y_t), E(X_t), Cov(X_t), Cov(X_t, y_t)) \):

\[
\beta_0 = E(y_t) - \beta_1^\top E(X_t), \quad \beta_1 = [Cov(X_t)]^{-1}Cov(X_t, y_t), \quad \sigma^2 = \text{Var}(y_t) - Cov(X_t, y_t)^\top [Cov(X_t)]^{-1}Cov(X_t, y_t) \quad (16)
\]

**Proof.** Assumption [i] implies that:

\[ E(u_t|x_t=x_t) = 0 \Leftrightarrow E(y_t|x_t=x_t) = \beta_0 + \beta_1^\top x_t. \quad (17) \]

The law of iterated expectations (Williams, 1991): \( E[E(Y|\sigma(X))] = E(Y) \), where \( \sigma(X) \) denotes the sigma-field generated by \( X \) implies that:

\[ E[E(y_t|\sigma(X_t))] = E(y_t) = \beta_0 + \beta_1^\top E(X_t) \Rightarrow \beta_0 = E(y_t) - \beta_1^\top E(X_t) \]

Substituting \( \beta_0 \) back into \( y_t = \beta_0 + \beta_1^\top x_t + u_t \) yields:

\[ y_t - E(y_t) = \beta_1^\top [X_t - E(X_t)] + u_t. \]

Post-multiplying both sides by \( [X_t - E(X_t)]^\top \) and taking expectations yields:

\[
Cov(y_t, X_t) := E \left( \left[ y_t - E(y_t) \right] [X_t - E(X_t)]^\top \right) = \beta_1^\top [X_t - E(X_t)][X_t - E(X_t)]^\top + E(u_t [X_t - E(X_t)]^\top).
\]

Since, the last term is zero: \( E(X_t^\top u_t) = E[E(u_t|\sigma(X_t))]=0, \) it follows that: \( \beta_1 = [Cov(X_t)]^{-1}Cov(X_t, y_t). \)

In the case of \( \sigma^2 \) we use a theorem analogous to the *lie* for the variance (Williams, 1991):

\[
\text{Var}(y_t) = E[\text{Var}(y_t|\sigma(X_t))] + \text{Var}[E(y_t|\sigma(X_t))],
\]

where, by definition \( E[\text{Var}(y_t|\sigma(X_t))] = \sigma^2 \). The mean deviation of (17) is:

\[
[\beta_0 + \beta_1^\top X_t] - E \left( [\beta_0 + \beta_1^\top X_t] \right) = \beta_1^\top [X_t - E(X_t)],
\]

and thus, by definition:

\[
\text{Var}[E(y_t|X_t)] = E \left[ \beta_1^\top [X_t - E(X_t)][X_t - E(X_t)]^\top \beta_1 \right] = \beta_1^\top [Cov(X_t)] \beta_1.
\]

From this, it follows that:

\[
\text{Var}(y_t) = \sigma^2 + \beta_1^\top [Cov(X_t)] \beta_1 \Rightarrow \sigma^2 = \text{Var}(y_t) - \beta_1^\top [Cov(X_t)] \beta_1,
\]

which yields the parameterization in (10). \( \blacksquare \)