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Further imprecision will not a clarity bring

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Abstract
In this rejoinder, we suggest that Professor Tweney's commentary on our article not only contains stark mischaracterizations of our analysis, but is rife with misunderstandings of basic quantitative theory, and loose, unfounded speculations. We conclude, in short, that it makes no contribution to the clarification of the phenomenon of regression towards the mean.

Keywords
conditional mean, linearity, measurement, quantitative scientific reasoning, RTM

Given the iconoclastic nature of our article (Maraun, Gabriel, & Martin, 2011), Professor Tweney's general support for the thesis we therein argue should be cause for celebration. However, a chief aim of our article was to provide the transparent, technically grounded account of the phenomenon of regression towards the mean (RTM) the adherence to which would leave no room for the loose, nontechnical talk that has been standard accompaniment to considerations of the topic, and that has proven to be fertile soil for the growth of both local confusions and science-undermining mythologizing.

So it is with some disappointment that we find ourselves replying to a commentary in which the technical foundation we offered up is bypassed, *en passant*, in favour of the very style of exposition—loose, rapid-fire, and nontechnical—that we had sought to ban, and in which the (predictable) by-products of this style of exposition—stark mischaracterizations of concepts and principles, misunderstandings of basic quantitative theory, and loose, unfounded speculations—are everywhere present.

What draws our eye, in the first place, is Tweney's contention that "Maraun et al. (2011) have not discussed two of the most important aspects of regression towards the mean: the constraints that it imposes upon the variance of the distributions and the fact

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that it is symmetrical for the two variables" (Tweney, 2013, p. 272). We did not discuss these "aspects" for the simple reason that they are not aspects of RTM. RTM is that property of a bivariate distribution wherein at least one of its conditional mean functions $E\,(Z_y \mid Z_x = \delta)$ and $E\,(Z_x \mid Z_y = \delta)$ satisfies both RTM-E.1 and RTM-E.2 (Maraun et al., 2011, p. 769). In respect to the first "aspect," as is evident from the elementary statistical identity $\mathrm{V(Y)} = \mathrm{V}(E\,(Z_y \mid Z_x)) + \mathrm{E}(V\,(Z_y \mid Z_x))$, for two continuous variates, the shape of a conditional mean function places no constraints on the conditional variance $V\,(Z_y \mid Z_x)$, hence, places no constraints on the unconditional variance V(Y). The fact of RTM-E.1 and RTM-E.2, then, places no constraints upon V(Y). In respect of the second, it is elementary statistical theory that $E\,(Z_y \mid Z_x = \delta)$ and $E\,(Z_x \mid Z_y = \delta)$ can have markedly different shapes, hence, markedly different properties.

Professor Tweney's erroneous belief that RTM both has the power to constrain variance and is symmetric seems to have its roots in the pairing of definitional looseness with non-technical thinking about a quantitative topic. In regard the former belief, he first invokes the ambiguous notion of "lawful regression." As we carefully explained in our article, *regression* is a synonym for conditional mean function. A conditional mean function, being as it is an extant, static feature of a bivariate distribution, can be neither lawful nor unlawful.

Even if we make the assumption that Tweney is, as seems to be the case, a participant in the inadvisable practice of taking the term *regression* to be a synonym for RTM (that particular state of affairs in which a conditional mean function—*regression*—satisfies both RTM-E.1 and RTM-E.2; note the induced circularity), the notion of lawful regression remains nonsensical: a particular conditional mean function either does or does not satisfy both RTM-E.1 and RTM-E.2, hence, either has, or does not have, the RTM property. These potential states of affairs can be said to be neither lawful nor unlawful. They simply do, or do not, obtain in nature.

Having situated his analysis within this field of definitional murkiness, Professor Tweney (2013) breaks free, altogether, of the definition of RTM, offering up a hypothetical scenario featuring a putative hereditary factor that has the power to "reduce the extremes of [a] distribution" (p. 272) as an *example* of RTM. It is but a small step from this conflating of RTM (a potential property of a conditional mean function) with a variance-impacting *entity*, to the illegitimate *ascription* to RTM of potential powers of the entity; in this case, the power to constrain variance.

To support the second claim, that RTM is a symmetric property—technically, that there does not exist a bivariate distribution of either empirical or theoretical type for which only one of $E\,(Z_y \mid Z_x = \delta)$ and $E\,(Z_x \mid Z_y = \delta)$ has the RTM property—what Professor Tweney owed was a *proof*. What he provided were a few calculations carried out on some of Galton's data. As intriguing as these calculations might be, they do not establish the truth of the universal quantitative proposition Tweney asserts.

It should be cause for rejoicing that Professor Tweney endorses the position that we take in respect to the contentious issue of the relationship between RTM and causality. It turns out, however, that this endorsement is predicated on Tweney's mischaracterization of our position—"What is the explanation of regression towards the mean? The authors reject causal explanations (rightfully so)" (Tweney, 2013, p. 273)—a mischaracterization that derives from his conflating of forms of causal explanation in which RTM is *explanandum* with forms in which it is *explanans*.

To recapitulate the position we carefully developed in our article: (a) RTM is certainly *not* a "statistical oddity" (Tweney, 2013, p. 272). When it happens to be a property of an empirical distribution, then it is an empirical (aggregate) property, hence, is a legitimate target of scientific explanation. Causal explanations of RTM (explanations in which RTM is explanans) are in no way illegitimate. They will, however, be notably difficult to formulate, as they are constituted of *both* an individual-level law that describes the dependency of *Y* on *X*, and a law that explains the distribution of X (Maraun et al., 2011, p. 778). On the other hand, (b) explanations in which RTM appears as explanans (notoriously, those in which the explanandum is the *individual entity*) are illegitimate for the reason that RTM is a static, aggregate property, and, as such, has no causal powers (Maraun et al., pp. 775–779).

On page 273 of the commentary, Professor Tweney (2013): (a) asserts that "Maraun et al. (2011) are in error in their claim that, to be applicable, regression towards the mean requires that both the X and Y distributions be linear and normal"; (b) asserts that RTM "will occur in any two distributions that have the same marginal distribution," citing Samuels (1991) in support of this claim; and (c) concludes, on the basis of (b), that "This renders moot, in most cases, their distinction between the theoretical and the empirical distributions."

There is not a grain of truth to (a). Our article contains no such a claim. To the contrary, a major accomplishment of the article was the construction of a *general* framework in which could be set Galton's *general* definition of RTM, the aim of this careful work being to sever the traditional "unwarranted tying of RTM to the linearity of conditional mean functions" (Maraun et al., 2011, p. 769). Our results make a firm, unambiguous separation between the *general* classes T_{RTM} and F_{RTM} of RTM-consistent bivariate distributions, on the one hand, and the sub-classes of bivariate distributions "RTM-E consistent by virtue of the fact that at least one of $E\,(Z_y \mid Z_x = \delta)$ and $E\,(Z_x \mid Z_y = \delta)$ is linear" (Maraun et al., 2011, p. 770), on the other.

Assertion (b) is erroneous, a mischaracterization of Samuels (1991), whose paper presents results that bear not on the property of RTM, but, rather, on a distinct property called *reversion towards the mean*. The result on identical marginals that Tweney proposes to employ in the service of rendering moot our insistence that the scientist must diligently observe the distinction between theoretical and empirical distribution, else risk having his or her work consigned to the scrap-heap of irrelevancy, *holds not for RTM*, but, rather, for the property of reversion towards the mean.

Conclusion (c), then, cannot be drawn on the basis of (b). We wish to add that, even if (b) *were* true, it would not warrant (c), for Tweney would be in a position to conclude neither that bivariate empirical distributions typically have identical marginals, nor that those empirical distributions that *do* have identical marginals can be adequately represented by particular theoretical distribution functions.

More generally, we find the spirit of (c) distasteful, as it urges the empirical scientist towards rash shortcuts and sloppiness. It should *never* be taken for granted that empirical distributions (legitimate targets of scientific investigation) can be unproblematically represented by theoretical distributions. The issue of representation should always be considered carefully and on a case-by-case basis.

In the final three paragraphs of his commentary, Tweney (2013) considers a pre-test/post-test design, and plunges with abandon into the murk of full-blown RTM mythologizing, invoking the notions of "RTM effects" and "RTM misconstruals," and re-issuing misguided warnings to the researcher that he or she must " 'watch out' for regression towards the mean" (p. 273) and "control for its effects" (p. 273). It seems that to resist the perspicuity that derives from definitional clarity, to prefer mythology to clear-headed scientific thinking, is the psychologist's wont.

Let us decompose the example: (a) a population P of individuals are scored on a variable X; (b) a selection of individuals "extreme on X," $X \in \omega$,[1] is made; (c) there is an interval of time, t_1, following which a treatment, T, is applied to the $X_i \in \omega$; and (d) there is a second interval of time, t_2.

Pause, now, and examine the situation. A bivariate distribution does not yet *exist*, hence, a Pearson Product Moment correlation, conditional mean functions, or potential properties of these functions such as RTM do not yet *exist*. Apart from T, (typically unknown) causal factors γ that impact upon the individuals in P are in operation during the interval (t_1+t_2), possibly changing these individuals in certain ways, and determining, in part, the scores that they will receive on any subsequent scoring of any variable.

Now, let it be that immediately following interval (t_1+t_2), the individuals in P are scored on a post-test Y. Instantly there exists a bivariate distribution, $F_{X,Y}$, of X and Y, hence: (a) conditional mean functions $E\,(Z_y \mid Z_x = \delta)$ and $E\,(Z_x \mid Z_y = \delta)$ that have whatever shapes nature has made them have; (b) properties (possibly, but by no means necessarily, RTM among them) of these conditional mean functions; and (c) a Pearson Product Moment correlation ρ_{XY} .

Features (a), (b), and (c) are instantaneously co-occurring, static properties of $F_{X,Y}$. They were not defined, hence, not even potentially extant, during the interval (t_1+t_2). Immediately following (t_1+t_2), they are defined under the scoring of post-test Y. Furthermore, $E\,(Z_y \mid Z_x = \delta)$ and $E\,(Z_x \mid Z_y = \delta)$ have whatever shapes nature has made them have, ρ_{XY} has whatever value nature has made it have, and RTM either is, or is not, a property of $F_{X,Y}$. These quantitative states of affairs neither *do*, nor *bring about*, anything. When they exist, they are simply quantitative features of the particular infinity of score-pairs that is $F_{X,Y}$.

If either of $E\,(Z_y \mid Z_x = \delta)$ or $E\,(Z_x \mid Z_y = \delta)$ *turned out to be linear*,[2] thereby making ρ_{XY} *relevant* (it would not have been so if both functions had been nonlinear), and if, furthermore, it turned out that $|\rho_{XY}| \neq 1$, then it would also be the case that RTM and $|E\,(Z_y \mid Z_x = \omega)| < \omega$[3] were properties of $F_{X,Y}$.

However, in the event that all of these properties obtained in nature, it would be nonsensical to suggest that either the fact of $|\rho_{XY}| \neq 1$, or the fact of RTM, *brought about* the state of affairs that $|E\,(Z_y \mid Z_x = \omega)| < \omega$. The linearity of conditional mean function, the fact of $|\rho_{XY}| \neq 1$, the property of RTM, the state of affairs that $|E\,(Z_y \mid Z_x = \omega)| < \omega$ … all would be static, co-incident properties of $F_{X,Y}$ (an infinity of score-pairs), as such, aggregate properties brought about by the (typically unknown) joint operation of T and causal factors $\underline{\gamma}$. RTM, a potential shape of a conditional mean function, *has no effects*, therefore, has no effects that must be controlled for.

The danger (present, note, *irrespective* of the degree of extremity of ω) inherent to taking the difference $\delta = \omega - E\,(Z_y \mid Z_x = \omega)$ as a quantification of the impact of T has nothing to do with RTM, ρ_{XY}, or any other static property of $F_{X,Y}$. The danger lies in the fact that δ is a property of $F_{X,Y}$, that is, an infinity of score-pairs, hence, is a product of the operation, during the interval (t_1+t_2), of *not only T*, but also the (typically unknown) causal factors γ, and pre-test/post-test designs (unlike analogous designs featuring a control condition) do not grant the researcher the capacity to disambiguate the respective operations of *T* and γ.

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Notes

1. *Extreme* defined in whichever way one pleases.
2. *In no way a necessity in nature*, and, also, not a necessary condition for RTM to obtain, but certainly a tacit assumption of Tweney's page 273 speculations, his easy adoption of which is a further testimonial to the imperative of distinguishing between empirical and theoretical states of affairs.
3. That is, "the post-test results are less extreme than the pre-test results" (Tweney, 2013, p. 273).

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