# Preregistration Does Not Improve the Transparent Evaluation of Severity in Popper's Philosophy of Science or When Deviations are Allowed

*Mark Rubin*
*Durham University, UK*

15th September 2024

*Citation*: Rubin, M. (2024, September 15). Preregistration does not improve the transparent evaluation of severity in Popper's philosophy of science or when deviations are allowed. *arXiv*. https://doi.org/10.48550/arXiv.2408.12347

## Abstract

One justification for preregistering research hypotheses, methods, and analyses is that it improves the transparent evaluation of the severity of hypothesis tests. In this article, I consider two cases in which preregistration does not improve this evaluation. First, I argue that, although preregistration can facilitate the transparent evaluation of severity in Mayo's error statistical philosophy of science, it does not facilitate this evaluation in Popper's theory-centric approach. To illustrate, I show that associated concerns about Type I error rate inflation are only relevant in the error statistical approach and not in a theory-centric approach. Second, I argue that a preregistered test procedure that allows deviations in its implementation does not provide a more transparent evaluation of Mayoian severity than a non-preregistered procedure. In particular, I argue that sample-based validity-enhancing deviations cause an unknown inflation of the test procedure's Type I (familywise) error rate and, consequently, an unknown reduction in its capability to license inferences severely. I conclude that preregistration does not improve the transparent evaluation of severity in Popper's philosophy of science or when deviations are allowed.

*Keywords*: critical rationalism; error statistics; *p*-hacking; preregistration; Popper; severity

Preregistration involves the time-stamped documentation of a study's planned hypotheses, methods, and analyses. The preregistered document is then made available with the final research report to allow others to identify any deviations from the planned approach (Nosek et al., 2019; Nosek et al., 2018).

Previous justifications for preregistration have argued that its primary role is to distinguish between "confirmatory" and "exploratory" analyses (e.g., Nosek & Lakens, 2014). However, there are unresolved questions about the meaningfulness of this distinction. For example, the distinction does not appear to have a formal definition in either statistical theory or the philosophy of science. In addition, critics have questioned related concerns about the "double use" of data and "circular reasoning" (Devezer et al., 2021; Rubin, 2020a, 2022a; Rubin & Donkin, 2022; Szollosi & Donkin, 2021; see also Mayo, 1996, pp. 137, 271-275; Mayo, 2018, p. 319).

More recently, Lakens and colleagues have provided a justification for preregistration based on Mayo's (1996, 2018) error statistical philosophy of science (Lakens, 2019, 2024; Lakens et al., 2024; see also Vize et al., 2024). In particular, Lakens (2019) argues that "preregistration has the goal to allow others to transparently evaluate the capacity of a test to falsify a prediction, or the severity of a test" (p. 221).

In this article, I consider two cases in which preregistration does not improve the transparent evaluation of severity. First, I highlight some differences between Mayo's (1996, 2018) error statistical conceptualization of severity and Popper's (1962, 1983) theory-centric conceptualization. I argue that, although preregistration may improve the transparent evaluation of Mayoian severity, it does not improve the transparent evaluation of Popperian severity. To illustrate my argument, I show that associated concerns about Type I error rate inflation are only relevant in an error statistical approach and not in a theory-centric approach.

Second, I argue that a preregistered test procedure that allows deviations does not provide a more transparent evaluation of Mayoian severity than a non-preregistered procedure. In particular, I argue that a preregistered test procedure that allows sample-based validity-enhancing deviations in its implementation will include an unknown number of deviations during a hypothetical long run of random sampling. These deviations will cause an unknown inflation of the procedure's Type I (familywise) error rate and, consequently, an unknown reduction of its capability to license Mayoian severity. I conclude that preregistration does not improve the transparent evaluation of severity in Popper's philosophy of science or when deviations are allowed.

## Preregistration Does Not Improve the Transparent Evaluation of Popperian Severity

In Mayo's (1996, 2018) error statistical approach, a hypothesis passes a severe test when there is a high probability that it would not have passed, or passed so well, if it was false (Mayo, 1996, p. 180; Mayo, 2018, p. 92; Mayo & Spanos, 2006, pp. 328, 350; Mayo & Spanos, 2010, p. 21; Mayo & Spanos, 2011, pp. 162, 164). Hence, severity is a characteristic of "the test *T*, a specific test result $x_0$, and a specific inference *H* (not necessarily predesignated)" (Mayo & Spanos, 2011, p. 164, italics are in the original text for all quotes).

A test procedure's error probabilities play an important role in evaluating severity (Mayo & Spanos, 2006, p. 330). In particular, "pre-data, the choices for the type I and II errors reflect the goal of ensuring the test is capable of licensing given inferences severely" (Mayo & Spanos, 2006, p. 350; see also Mayo & Spanos, 2011, p. 167). For example, a test procedure with a nominal pre-data Type I error rate of $\alpha = 0.05$ is capable of licensing specific inferences with a minimum "worst case" severity of 0.95 (i.e., 1 – α; Mayo, 1996, p. 399). Importantly, low error probabilities are necessary but not sufficient to license inferences severely (Mayo, 2018, pp. 13-14, 236, 396; Mayo & Spanos, 2011, p. 163). Error probabilities must also be relevant to the current inference (Mayo, 2018, pp. 194, 236, 429; Mayo & Spanos, 2006, p. 349), and statistical model assumptions must be approximately satisfied (Mayo, 2008, pp. 863-864; Mayo, 2018, p. 94; Mayo & Spanos, 2006, p. 349; Mayo & Spanos, 2011, pp. 189-190).

"Biasing selection effects" in the experimental testing context can lower the capability of a test procedure to license inferences severely by increasing the error probability with which the procedure passes hypotheses (Mayo, 2018, pp. 40, 196). For example, data dredging, fishing, cherry-picking, selective reporting, and *p*-hacking can represent biasing selection effects that increase a test procedure's error probability and, consequently, lower its capability for severe tests (e.g., Mayo, 1996, pp. 303-304; Mayo, 2008, pp. 874-875; Mayo, 2018, pp. 92, 274-275).

From an error statistical perspective, the goal of preregistration is to allow a more transparent evaluation of the capability of a test procedure to perform severe tests (e.g., Lakens, 2019; Lakens et al., 2024). In particular, preregistration reveals a researcher's *planned* hypotheses, methods, and analyses and enables a comparison with their *reported* hypotheses, methods, and analyses in order to identify any biasing selection effects in the experimental testing context that may increase the test procedure's error probabilities and lower its capability for severe tests.

Note that, the more precisely specified a preregistered plan, the greater its potential to identify biasing selection effects. A vaguely specified preregistration that allows a lot of flexibility in the implementation of a planned test procedure has less potential to identify biasing selection effects and so will be less effective in allowing a transparent evaluation of the procedure's capability to perform severe tests (Lakens, 2019, pp. 226-227; Lakens et al., 2024, p. 16). Hence, it has been proposed that, ideally, preregistered research protocols should include machine-readable code that limits analytical flexibility by automatically analyzing the data and evaluating the results (Lakens & DeBruine, 2021; Lakens et al., 2024, p. 16).

Importantly, Mayo's (1996, 2018) error statistical approach provides only *one* of *several* different conceptualizations of severity. Other conceptualizations have been proposed by Bandyopadhyay and Brittan (2006), Hellman (1997, p. 198), Hitchcock and Sober (2004, pp. 23-25), Horwich (1982, p. 105), Lakatos (1968, p. 382), Laudan (1997, p. 314), Popper (1962, 1983), and van Dongen et al. (2023). Furthermore, preregistration may not improve the transparent evaluation of these other types of severity. In the present article, I illustrate this point by showing that preregistration only improves the transparent evaluation of Mayoian severity, not Popperian severity. I begin by explaining the concept of Popperian severity and considering its differences with Mayoian severity.

## Popperian Severity

Popperian severity is measured as the conditional probability of a statement of supporting evidence ($e$) given the conjunction of a hypothesis ($h$) and background knowledge ($b$) divided by the conditional probability of $e$ given $b$ alone. In other words, severity ($e$, $h$, $b$) = $p(e, hb)/p(e, b)$ (Popper, 1962, p. 391; Popper, 1966b, p. 288; see also Popper, 1983, pp. 238-239). Hence, the more probable is $e$ given $hb$ relative to $e$ given $b$ alone, the more severe the test of $h$. Note that "background knowledge" ($b$) refers to the initial conditions of a particular test together with relevant auxiliary hypotheses and theories that have been tentatively and temporarily accepted as being unproblematic during the test (Popper, 1962, pp. 238, 390; Popper, 1966b, p. 287).

As explained previously, a test procedure's capability to license inferences with Mayoian severity can be reduced by biasing selection effects in the experimental testing context. In particular, unplanned changes to the way in which a researcher constructs and selects hypotheses and evidence during the implementation of a test procedure may increase its error probability and decrease its capability for severe tests. Hence, the evaluation of Mayoian severity requires a consideration of the impact of any biasing selection effects (e.g., Mayo, 1996, pp. 303-304; Mayo, 2008, pp. 874-875; Mayo, 2018, pp. 274-275; Mayo & Cox, 2010, p. 267-270). In contrast, the measurement of Popperian severity does not take account of the potentially biased way in which a researcher constructs or selects *e*, *h*, and/or *b* in the experimental testing context. As Mayo (1996) explained, in the case of Popperian severity, "there is no demand that a specific testing context be delineated, there are just…requirements in terms of the logical relationships between statements of evidence and hypotheses" (p. 209; see also Mayo, 1996, pp. 206-207, 255, 330; Lakatos, 1978, p. 114).

Popper would likely agree with Mayo's assessment. He argued that a researcher's private psychologically biased reasons for constructing and selecting particular *e*, *h*, and *b* for inclusion in a test (e.g., "because I want to get a significant result") belong to the subjective "World 2" context of discovery and, consequently, they do not enter into a deductive method of testing. In contrast, a researcher's public scientific justification for the formal specification of their test (e.g., "because it is the most valid test") belongs to an autonomous objective "World 3" context of justification, which is open to logical and critical scrutiny (Popper, 1962, p. 140; Popper, 1974a, pp. 74, 118; Popper, 1983, p. 36; Popper, 1994, pp. 149-150; Popper, 2002, p. 7; see also Reichenbach, 1938, pp. 6-7).[1] Of course, a researcher's public World 3 justification may also be biased. However, as Popper (1994, pp. 7, 93) pointed out, "it need not create a great obstacle to science if the individual scientist is biased in favour of a pet theory," because science proceeds on the basis of *collective* criticism by other scientists (e.g., peer review, further tests, etc.). Hence, "if you are biased in favour of your pet theory, some of your friends and colleagues (or failing these, some workers of the next generation) will be eager to criticize your work – that is to say, to refute your pet theories if they can" (Popper, 1994, p. 93). This collective "critical rationalist" approach is a central aspect of Popper's philosophy of science (Popper, 1994, p. 159), and it may be described as "theory-centric" because it occurs between and within relevant theories in World 3 (Popper, 1962, p. 26; Popper, 1974a, p. 82; Popper, 1983, pp. 28-30, 32; see also Musgrave, 2010).

Although Popper was not concerned about *psychological* bias in the construction and selection of hypotheses and evidence, he was concerned about an *epistemological* bias during testing. In particular, he argued that evidence *e* could only corroborate hypothesis *h* "if *e* is the result of genuine or sincere attempts to refute *h*" (Popper, 1983, p. 235; see also Popper, 2002, pp. 437-438). Note that *sincerity* "is not meant in a psychologistic sense" (Popper, 1974b, p. 1080). Hence, it is not intended to address psychologically motivated biasing selection effects (Popper, 1974b, p. 1080). Instead, Popper's "requirement of sincerity" (Popper, 2002, p. 437) represents a "methodological rule" (Popper, 1974b, p. 1080) that is intended to support his falsificationist epistemology over that of verificationism (Popper, 1983, p. 235). He proposed two ways of implementing this rule.

First, "we can partly formalize…[the requirement of sincerity] by demanding that our empirical test statements should be unexpected or improbable in the light of our background knowledge; that is to say, their probability, given the background knowledge, should be (considerably) less than ½" (Popper, 1983, p. 253). In other words, "$p(e, b) \ll ½$" (Popper, 1983, p. 238). The more improbable *e* given *b* alone, the more severe and sincere the test.

Second, we should design “crucial” tests in “which the theory to be tested predicts results which differ from results predicted by other significant theories, especially by those theories that have been so far accepted” (Popper, 1983, p. 235). In particular, we must pit our primary hypothesis $h$ against a significant, accepted, rival hypothesis $h'$ that predicts conflicting results given the same background knowledge (Popper, 1962, pp. 112, 197, 246; Popper, 1974a, pp. 13-15, 354, Footnote 7; Popper, 1974b, p. 995; Popper, 1983, pp. 188, 233-236; Popper, 1994, p. 7; Popper, 2002, p. 277; see also Bandyopadhyay & Brittan, 2006, p. 276; Lakatos, 1968, p. 380; Lakatos, 1978, p. 24, Footnote 1). It is only the refutation of $h'$ that allows a sincere corroboration of $h$ (Popper, 1974a, pp. 14-15; Popper, 1974b, pp. 995, 1009; Popper, 2002, pp. 66-67, 82).

In summary, Popper’s “requirement of sincerity” represents a methodological rule that we should “try to construct *severe* tests, and *crucial* test situations” (Popper, 1974a, p. 14). The more severe and crucial the test, the more it is “sincere,” and the less biased it is towards a “cheap” (verificationist) corroboration (Popper, 1983, pp. 130, 163, 257).

Popper argued that “the severity of our tests can be objectively compared; and if we like, we can define a measure of their severity” (Popper, 1962, p. 388; Popper, 1966b, p. 287; see also Popper, 1962, pp. 220, 390-391; Popper, 1983, pp. 238-239). However, he conceded that “the requirement of sincerity cannot be formalized” (Popper, 2002, p. 437), because “sincerity is not the kind of thing that lends itself to logical analysis” (Popper, 1983, p. 236; see also Popper, 1962, p. 288; Popper, 1983, pp. 244, 254; Popper, 2002, p. 419). Nonetheless, it remains possible to undertake an *informal* assessment of sincerity given the current state of World 3 knowledge (Popper, 1974b, p. 1080). In particular, collective critical rationalism may be used to evaluate the sincerity of a test by assessing the extent to which (a) $p(e, b) \ll ½$ and (b) $h'$ represents a significant accepted theory that predicts contradictory results to $h$ (e.g., Lakatos, 1968, 1978; Laudan, 1997, pp. 314-315; see also Bandyopadhyay & Brittan, 2006, p. 264; van Dongen et al., 2023, p. 521). Hence, in Popper’s approach, an informal critical rational evaluation of sincerity can be used to contextualize and interpret a more formal measure of severity.[2] Importantly, and in contrast to the error statistical approach, neither assessment requires a consideration of researchers’ private World 2 construction or selection of $e$, $b$, $h$, or $h'$ in the experimental testing context.

## Mayoian Severity

Mayo was not satisfied with Popper’s measure of severity (e.g., Mayo, 1996, p. 207; Mayo, 2006, p. 11), and she felt that his “theory-dominated” critical rational assessment of sincerity was inadequate (Mayo, 1997, p. 331; see also Mayo, 1996, pp. 59, 264; Mayo, 2006, p. 11; Mayo, 2018, pp. 40-41; Mayo & Spanos, 2006, p. 328). In her view, “it is impossible to assess reliability or severity with just statements of data and hypotheses divorced from the experimental context in which they were generated, modeled, and selected for testing” (Mayo, 2006, p. 36).

In response to these perceived deficiencies, Mayo (1996) developed an account of severity that refers to a test procedure’s experimental testing context and its frequentist error probabilities in a hypothetical series of its repetitions (Mayo & Spanos, 2010, p. 21; see also Mayo, 1996, p. 72; Mayo, 2018, pp. 72-73; Mayo & Spanos, 2006, p. 328).[3] As she explained:

> “We must look at the particular experimental context in which the evidence was garnered and argue that its fitting a hypothesis is very improbable, if that hypothesis is false. This relativity to an experimental testing model and the focus on (frequentist) probabilities of test procedures distinguish my account, particularly from others that likewise appeal to probabilities to articulate the criterion for a good or severe test – even from accounts that at first blush look similar, most notably Popper’s” (Mayo, 1996, pp. 206-207).

Importantly, Mayo's (1996) concept of an "experimental context" includes the potentially unreported and psychologically biased process by which researchers might construct and select hypotheses and evidence during the implementation of a test procedure. To be clear, like Popper, Mayo (1996) accepts that it does not matter that a researcher's psychologically biased intentions may influence the *specification* of a test procedure (Mayo, 1996, p. 409; see also Mayo, 1996, pp. 148, 263; Mayo, 2018, pp. 9-10). However, unlike Popper, she argues that it *does* matter that the researcher's psychological intentions may cause biasing selection effects during the *implementation* of that procedure. It matters, she argues, because biasing selection effects may increase the procedure's frequentist error probability and lower its capability to license inferences severely (e.g., Mayo, 1996, p. 349). Consequently, we must check the entire experimental testing context, including its unreported parts, in order to pick up on any biasing selection effects during the implementation of the test procedure and meet a minimal requirement for severity (Mayo, 1996, p. 298; Mayo, 2018, pp. 5, 9, 49, 92). In contrast, Popper (1967, pp. 34, 39) argued that hypothetical probability distributions are defined relative to a researcher's formal public World 3 specifications of an experiment and, consequently, they are not affected by unspecified (unreported) issues that may occur during the implementation of an experiment.

More generally, Mayo rejects Popper's "theory-dominated" approach and develops a philosophy of science in which "what we rely on…are not so much scientific theories but *methods* for producing experimental effects" (Mayo, 1996, p. 15; see also Mayo, 1996, pp. 12, 17 Footnote 3, 59; Mayo, 1997, p. 331). These experimental effects support local claims that are limited to specific experimental contexts (Mayo, 2006, p. 37-38). Hence, contrary to Popper's approach, the error statistical approach does not permit single tests to logically refute or corroborate universal theories (e.g., "all swans are white"; Chalmers, 2010, pp. 60-63). Instead, it tests claims about local effects in "parochial" populations (e.g., "all swans in this experiment are white"; Rubin, 2021a, p. 5825). This approach is consistent with the New Experimentalist view that "experiments, as Ian Hacking taught us, live lives of their own, apart from high level theorizing" (Mayo, 1996, pp. xiii, 12, 17, 190, 213; Musgrave, 2010, pp. 108-109). However, it conflicts with Popper's theory-centric view that "theory dominates the experimental work" (Popper, 2002, p. 90; see also Popper's, 1983, pp. 47-48, 50, contrast between "inductivist" and theory-centric styles of reporting research; cf. Mayo, 1996, pp. 59, 264; Mayo, 1997, p. 331; Mayo, 2006, p. 11; Mayo, 2018, pp. 40-41). For this and other reasons, the error statistical approach "does not find its home in a Popperian framework" (Mayo, 1996, p. 412).

## Illustrating the Differences Between Mayoian and Popperian Severity

To provide a more concrete illustration of the differences between Mayoian and Popperian severity, consider a researcher who conducts multiple uncorrected and unreported tests in order to find and report a single significant result (i.e., *p*-hacking). They then secretly hypothesize after their significant result is known in order to construct a hypothesis that they report as if it was generated before they conducted their analysis (i.e., HARKing). In this case, the researcher has been biased in the selection of their reported evidence (i.e., in favor of a test that yields a significant result) and the construction of their reported hypothesis (i.e., in favor of a hypothesis that is corroborated by that result). These biasing selection effects have made it easier for the researcher to pass false hypotheses during repetitions of their (partially unreported) test procedure. Consequently, their method has a relatively low capability to license inferences with Mayoian severity.

In contrast, biasing selection effects do not affect Popperian severity or sincerity because they occur in the experimental testing context, and Popper's approach does not take account of the experimental testing context (Mayo, 1996, p. 209). Popperian severity is measured as the conditional probability of a result ($e$) given the conjunction of a hypothesis ($h$) and background knowledge ($b$) relative to the probability of $e$ given $b$ per se. The researcher's privately biased selection of $e$, $h$, and/or $b$ via $p$-hacking and HARKing does not enter into this measurement.

Popperian sincerity requires that (a) $p(e, b) \ll ½$ and (b) $h'$ represents a significant accepted theory that predicts contradictory results to $h$. Certainly, this requirement may not be met, leading to a "cheap" (verificationist) corroboration (Popper, 1983, pp. 163, 257). However, the resulting bias is epistemological rather than psychological, and it can be evaluated via a critical rational discussion of publicly available information independent from any biasing selection effects that may be hidden in the experimental testing context (Popper, 1974b, p. 1080).

More generally, the researcher's private psychological reasons for conducting and reporting their specific test (i.e., because their previous tests did not yield a significant result) and for constructing their specific hypothesis (i.e., because it was passed by the current result) belong to Popper's World 2 of subjective intentions rather than his World 3 of objective justifications (Popper, 1974a, pp. 74, 108-109, 299). From a Popperian perspective, what is relevant is the researcher's formal public scientific justification for their reported approach, and they can rationally reconstruct this justification in World 3 in a way that is epistemically independent from their private psychological intentions in World 2 (Popper, 1974a, pp. 179, 242; see also Rubin, 2022a, pp. 540-542; Rubin & Donkin, 2022, pp. 5-6). Furthermore, the researcher's psychologically biased reasoning and behavior in World 2 does not imply incorrect, invalid, or unsound epistemic reasoning or research methods in World 3. Consequently, in the Popperian approach, even unplanned and subjectively biased hypothesis tests may be objectively severe and sincere.

Mayo and Cox (2010, pp. 268, 271-272) provide a further example. A researcher intends to conduct a linear regression analysis of $y$ on $x$. However, they make the post-data decision to conduct a regression of log $y$ on log $x$. According to Mayo and Cox, if the researcher makes this decision because they conducted both tests and the second test provides "the more extreme statistical significance…, then adjustment for selection is required" (p. 271). On the other hand, if they make this decision because "inspection of the data suggests that it would be better to use the regression of log $y$ on log $x$,…because the relation is more nearly linear or because secondary assumptions, such as constancy of error variance, are more nearly satisfied" (p. 268), then "no allowance for selection seems needed…[because] choosing the more empirically adequate specification gives reassurance that the calculated $p$-value is relevant for interpreting the evidence reliably" (p. 272). However, consider a situation in which the selected test provides *both* the more extreme statistical significance *and* the more empirically adequate specification. In this case, an error statistician should attempt to ascertain the researcher's subjective World 2 intentions because those intentions may indicate the operation of a biasing selection effect that alters the test procedure's capability to perform severe tests in its hypothetical repetitions (Mayo, 1996, pp. 348-349; Mayo, 2018, pp. 49, 286). Hence, critics have argued that error statisticians need to take account of private intentions "locked up in the scientist's head" (Mayo & Spanos, 2011, p. 186; see also Mayo, 1996, pp. 346-350; Mayo, 2008, pp. 860-861). In contrast, a theory-centrist would only refer to the researcher's public objective World 3 reasoning because "science is part of world 3, and not of world 2" (Popper, 1974b, p. 1148). Consequently, following the principle of "*relativity to specification*" (Popper, 1967, p. 35), they would limit their inference to the formally reported test and its associated hypothetical sampling distribution (Fisher, 1955, p. 75; Fisher, 1956, pp. 44, 77-78, 82; Rubin, 2024b).

Mayo (1996, 2018) would argue that Popper's approach is insufficient to address his requirement of sincerity in the above examples. As she explained:

> "If you engage in cherry picking, you are not 'sincerely trying,' as Popper puts it, to find flaws with claims, but instead you are finding evidence in favor of a well-fitting hypothesis that you deliberately construct – barred only if your intuitions say it's unbelievable. The job that was supposed to be accomplished by an account of statistics now has to be performed by *you*. Yet you are the one most likely to follow your preconceived opinions, biases, and pet theories" (Mayo, 2018, pp. 40-41).

However, from a Popperian perspective, even a "cherry-picked" hypothesis that is deliberately chosen because it is corroborated can be said to have undergone a sincere test as long as peers agree that (a) $p(e, b) \ll ½$ and (b) $h'$ represents a significant accepted theory that predicts contradictory results to $h$. The "cherry-picking" that concerned Popper occurs in a public, transparent manner at the rational, theoretical level in World 3's context of justification, rather than in a private, unreported manner at the personal, psychological level in World 2's context of discovery.

Finally, it is important to reiterate that the Popperian requirement of sincerity is established via a public, theory-centric, critical rationalist appraisal given the current state of World 3 knowledge rather than via "your intuitions" (Mayo, 2018, p. 40). Of course, Mayo (2018, p. 41) is correct that this appraisal may be affected by "your preconceived opinions, biases, and pet theories." However, it is not only "performed by *you*" (Mayo, 2018, p. 40). Again, critical rationalism is a *collective* process in which researchers' opinions and biases are pitted against those of other researchers in an ongoing critical discussion in the scientific community (Popper, 1974b, p. 1080; Popper, 1994, pp. 7, 93; Schaller, 2016, p. 111). As Popper (1974b) explained:

> "I have always tried to show that *sincerity in the subjective sense* is not required, thanks to the social character of science which has (so far, perhaps no further) guaranteed its objectivity. I have in mind what I have often called 'the friendly-hostile cooperation of scientists'" (p. 1080).

This friendly-hostile cooperation "does not require that scientists be unbiased, only that different scientists have different biases" (Hull, 1988, p. 22; see also Popper, 1962, pp. 14-18; Popper, 1994, pp. 22, 93). Hence, for Popper, an objective evaluation of the requirement of sincerity is not "supposed to be accomplished by an account of statistics" (Mayo, 2018, p. 40); it is supposed to be accomplished through a well-conducted critical discussion among scientists (Popper, 1966a, pp. 415-416; Popper, 1974a, p. 22; Popper, 1983, p. 48). As Popper (1983) explained, "objectivity is not the result of disinterested and unprejudiced observation. Objectivity, and also unbiased observation, are the result of criticism" (p. 48; see also Popper, 1994, p. 93).

## Using Preregistration to Transparently Evaluate Severity

The differences between Mayoian and Popperian severity are particularly relevant in the context of preregistration. A well-specified preregistration allows others to transparently evaluate Mayoian severity because it helps to reveal a researcher's biasing selection effects in the implementation of a planned test procedure by revealing the otherwise hidden parts of the experimental testing context (e.g., Lakens, 2019, 2024; Lakens et al., 2024; see also Mayo, 1996, p. 296; Mayo, 2018, p. 319; Staley, 2002, p. 289). However, the same rationale does not apply in the case of Popperian severity.

A valid measurement of Popperian severity can be made using a potentially *p*-hacked result ($e$), a potentially HARKed hypothesis ($h$), and potentially biased background knowledge ($b$). In addition, the requirement of sincerity can be transparently evaluated via a public, collective, critical rational discussion of $p(e, b)$ and $h$ vs. $h'$ given the current state of World 3 knowledge. Preregistration does not facilitate transparency in either case because neither evaluation requires knowledge of the researcher's planned approach or unreported biasing selection effects. Indeed, Popper's "theory-dominated" approach may be applied retrospectively via a rational reconstruction of an unplanned test based on the $e$, $b$, $h$, and $h'$ that we have in hand and in the context of the current state of scientific knowledge (Mayo, 1996, pp. 67-68; see also Lakatos, 1978, p. 114; Popper, 1967, pp. 35-36; Reichenbach, 1938, p. 5; cf. Mayo, 1996, p. 17).[4]

## Type I Error Rate Inflation

The error statistical and theory-centric approaches can also be contrasted in relation to Type I error rate inflation. The error statistical approach distinguishes between two types of Type I error rate. The "computed" error rate is based on the number of formally reported tests, and the "actual" error rate is based on the number of reported *and* unreported tests in the experimental testing context. If some tests are unreported, then the "actual" error rate will be higher than the "computed" error rate. Hence, uncorrected multiple testing and selectively reported significant results (i.e., *p*-hacking) may cause an inflation of the "actual" Type I (familywise) error rate above the "computed" error rate (Mayo, 1996, pp. 303-304; Mayo, 2008, pp. 874-875; Mayo, 2018, pp. 274-275; Mayo & Cox, 2010, p. 267-270).

From this error statistical perspective, a well-specified preregistered plan is helpful because it can be used to transparently verify the number of tests in the planned experimental testing context, which may include some tests that were conducted during the implementation of the test procedure but not reported in the research report (Nosek et al., 2019, p. 816). The "actual" number of tests ($k$), can then be used to compute the test procedure's "actual" familywise error rate (i.e., $1 - [1 - \alpha]^k$) and determine whether it is higher than the "computed" error rate.

For example, imagine that a researcher preregisters three tests (i.e., $k = 3$), each with a nominal alpha level of 0.05, but they then selectively report only one of the tests because it was the only one to yield a significant result (i.e., a biasing selection effect). In this case, the "actual" familywise error rate would be 0.14 ($1 - [1 - 0.05]^3$) even if the "computed" (reported) error rate for the specific test was 0.05 ($1 - [1 - 0.05]^1$). Correspondingly, the "actual" minimum level of severity ($1 - 0.14 = 0.86$) would be lower than the "computed" minimum level ($1 - 0.05 = 0.95$; Mayo, 1996, p. 399). Preregistration is useful in this situation because it allows the identification of the Type I error rate inflation and the associated reduction in the test procedure's capability to perform severe tests.

Importantly, however, this error statistical perspective does not apply in a "theory first" approach (Rubin, 2022b). In this case, the "actual" Type I error rate does not necessarily refer to the "experiment-wide significance level" of the "entire experimental testing context" and its associated "experimental distribution" (Mayo, 2018, p. 275; Mayo, 1996, pp. 143, 298) because not all of the tests that are performed within the experimental testing context may be logically related to a reported statistical inference (Rubin, 2021b, 2024b). Instead, the "actual" (relevant) Type I error rate is the familywise error rate of the tests that are formally used to make a statistical inference about a (potentially unplanned) hypothesis, and the number of these tests can be logically deduced from the formally reported statistical inference.

For example, if a statistical inference is made about a single *individual* null hypothesis $H_{0,1}$ based on a single test of that hypothesis (i.e., $k = 1$) using an $\alpha$ of 0.05, then the "actual" Type I error rate for that inference will be the same as the "computed" (reported) nominal error rate (0.05 or $1 - [1 - 0.05]^1$), even if the researcher performed multiple other planned or unplanned tests, secretly or explicitly, during their implementation of the experiment (Hitchcock & Sober, 2004, pp. 23-25; Rubin, 2017a, 2021b, 2024a, 2024b). In this case, it would be illogical to argue that these other tests (e.g., tests of $H_{0,2}$, $H_{0,3}$, $H_{0,4}$, etc.) contribute to the error rate for the statistical inference about $H_{0,1}$ because they are not logically related to this inference, which is about $H_{0,1}$ per se.

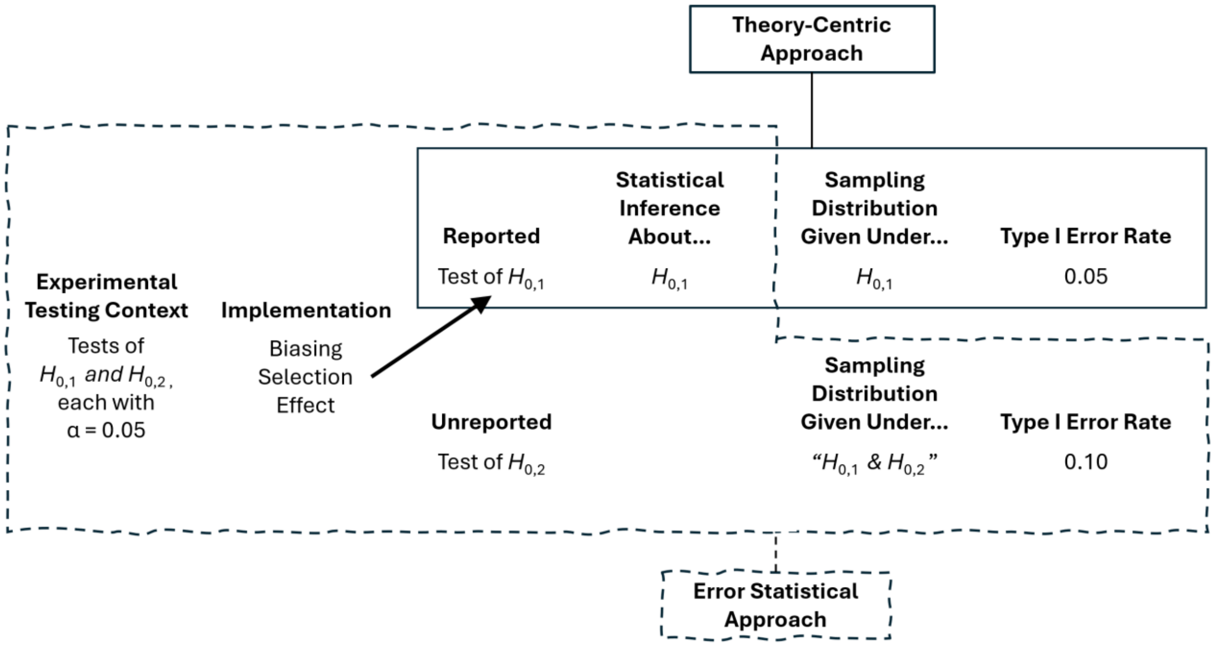
Similarly, if an inference is made about a *joint* intersection null hypothesis that is composed of three constituent null hypotheses, "$H_{0,1}$ & $H_{0,2}$ & $H_{0,3}$" (i.e., $k = 3$), then the "actual" familywise error rate can be logically deduced from the formally reported statistical inference as being 0.14 (i.e. $1 - [1 - 0.05]^3$). In this case, it would be illogical to use a familywise error rate that included a test of $H_{0,4}$, even if $H_{0,4}$ was planned and/or conducted, because a familywise error rate that includes $H_{0,4}$ warrants a different statistical inference to the one that is reported (i.e., an inference about "$H_{0,1}$ & $H_{0,2}$ & $H_{0,3}$ & $H_{0,4}$" rather than "$H_{0,1}$ & $H_{0,2}$ & $H_{0,3}$"; Rubin, 2024a, 2024b).

Error statisticians might argue that theory-centrists are "using the wrong sampling distribution" here because it does not reflect the "actual" test procedure (Spanos & Mayo, 2015, p. 3546). However, this argument depends on how we define the "actual" test procedure (Mayo, 1996, p. 304). In the error statistical approach, the "actual" procedure includes a researcher's unreported tests in the experimental testing context. For example, if a researcher tests null hypotheses $H_{0,1}$ and $H_{0,2}$ and then makes an inference about $H_{0,1}$ (because $p < 0.50$) but does not report their test of $H_{0,2}$ (because $p > 0.50$), then their "actual" test procedure includes the unreported test of $H_{0,2}$, and the "right" sampling distribution is given under the joint intersection null hypothesis "$H_{0,1}$ & $H_{0,2}$" (the "global" or "universal" null; Mayo, 2008, p. 875; Mayo & Cox, 2010, p. 269; Mayo, 2018, p. 276). In contrast, in a theory-centric approach, the "actual" test procedure only includes tests that are logically related to the formally reported statistical inference. If that inference is restricted to $H_{0,1}$, then the test of $H_{0,2}$ is not part of the "actual" test procedure, and the "right" (logically relevant) sampling distribution is given under $H_{0,1}$, not "$H_{0,1}$ & $H_{0,2}$" (Rubin, 2024a, 2024b; see also Popper, 1967, p. 36). Figure 1 illustrates this point.

In summary, in the error statistical approach, the "actual" Type I error rate is based on the tests in the experimental testing context, and it is inflated above the "computed" error rate when the computed error rate does not refer to all of the tests in that context. Consequently, preregistration is a useful way of revealing the planned testing context to identify any biasing selection effects, Type I error rate inflation, and corresponding reduction in a test procedure's capability to perform severe tests. In contrast, in a theory-centric approach, the "actual" Type I error rate can be logically deduced from the formally reported statistical inference, even if that inference was unplanned and selectively reported. In this case, any Type I error rate "inflation" will be the result of a logical inconsistency between a formally reported statistical inference and a formally computed familywise error rate rather than the result of undisclosed tests in the experimental testing context. This inconsistency can be identified and rectified through a logical analysis of the relation between the reported inference and error rate without needing to consult a preregistered plan (for examples, see Rubin, 2024a, 2024b).

**Figure 1**

*Illustration of Error Statistical and Theory-Centric Approaches to Type I Error Rates*




# Preregistration Does Not Improve the Transparent Evaluation of Mayoian Severity When Deviations are Allowed

I have argued that preregistration does not improve the transparent evaluation of Popperian severity, but that a well-specified preregistration may improve the transparent evaluation of Mayoian severity. In this section, I add a caveat to this argument: A well-specified preregistration may improve the transparent evaluation of Mayoian severity *provided that the test procedure does not allow deviations in its implementation.* Specifically, I consider deviations that are intended to maintain or increase the validity of a test procedure in light of unexpected issues that arise in particular samples of data. I argue that a test procedure that allows these sample-based validity-enhancing deviations in its experimental testing context will suffer an unknown inflation of its Type I error rate and, consequently, an unknown reduction of its capability to license inferences with Mayoian severity.

## Sample-Based Validity-Enhancing Deviations Cause an Unknown Inflation of the Type I Error Rate

A researcher may deviate from their preregistered plan because they encounter an unforeseen event, violated assumption, or falsified auxiliary hypothesis in their current sample, and they wish to adapt their test procedure in order to maintain or increase its validity in light of this unanticipated issue (Lakens, 2024, pp. 2, 7; Mayo & Cox, 2010, p. 268, Example 4; Rubin, 2017b, p. 326; Rubin & Donkin, 2022, p. 12). For example, a researcher may adhere to a preregistered Student's *t*-test when the assumption of homogeneity is met in one sample. However, they may deviate from their plan and use Welch's *t*-test when this assumption is unexpectedly violated in another sample because Welch's test provides a more valid approach in this situation (Lakens, 2024, p. 8). Consequently, in a hypothetical long run of random sampling, the researcher's test procedure would use *two* different tests to test the same joint intersection null hypothesis (i.e., Student's *t*-test & Welch's *t*-test). This "forking path" in the experimental testing context inflates the test procedure's "actual" Type I (familywise) error rate due to the multiple testing problem (Gelman & Loken, 2013, 2014; Rubin, 2017b; see also García-Pérez, 2012, pp. 4-5).

Note that sample-based validity-enhancing deviations do not represent Mayoian biasing selection effects because they are based on reasonable and/or conventional analytical principles with the aim of maintaining or enhancing validity rather than reaching "a desired inference" (Mayo, 2018, p. 105; see also Mayo & Cox, 2010, pp. 271-272). Nonetheless, during a long run of repetitions of a test procedure that allows these deviations, the experimental testing context will include multiple tests that inflate the procedure's Type I (familywise) error rate. Hence, as Gelman and Loken (2013) explained, this multiplicity "can be a problem, even when there is no 'fishing expedition' or '*p*-hacking'" (p. 1; see also Rubin, 2017b, p. 324).

Also note that the forking paths issue is separate from the concern about the double use of data when checking test assumptions (e.g., the assumption of homogeneity). Mayo is correct that, when researchers check test assumptions, the data can be considered to be "remodelled" to address a different question to the one addressed by the primary hypothesis (Mayo, 1996, pp. 137, 271-275; Mayo, 2018, p. 319). Hence, there is no contravention of the "use novelty principle" in this case (see also Rubin & Donkin, 2022, p. 5). Nonetheless, it remains the case that the introduction of a new test (e.g., Welch's *t*-test) based on sample-specific (data-dependent) information creates a forking path in the experimental testing context, and that uncorrected multiple testing in repetitions of the forked test procedure inflates its "actual" Type I (familywise) error rate.

A single sample-based validity-enhancing deviation opens up a single forking path in the experimental testing context. However, the error statistical approach operates on the basis of counterfactual reasoning that considers how a test procedure would perform given different samples of data (Mayo, 2008, p. 876; Mayo, 2018, pp. 52-53). Consequently, we must consider that, during a hypothetical long run of random sampling, different samples may require different validity-enhancing deviations based on different unforeseen events, violated assumptions, and falsified auxiliary hypotheses. A test procedure that allows such deviations in its implementation will include an unknown number of deviation-based tests in its experimental testing context. Given that $k$ is unknown in this case, we cannot compute the test procedure's "actual" Type I (familywise) error rate using $1 - [1 - \alpha]^k$ (for related points, see Nosek & Lakens, 2014, p. 138; Nosek et al., 2019, p. 816; Rubin, 2017a, 2024b). Consequently, we cannot transparently evaluate the test procedure's capability to license inferences with Mayoian severity (for related points, see Mayo, 1996, p. 313; Mayo, 2018, pp. 200-201; Staley, 2002, p. 289).

Contrary to this view, Lakens (2024) proposed that peers can evaluate whether sample-based validity-enhancing deviations increase or decrease Mayoian severity by considering (a) a researcher's flexibility with regards to other "plausible" and "defensible" analyses and (b) the results that follow from these alternative analyses (i.e., sensitivity analyses). Certainly, from a Popperian perspective, theories that allow a wide "range" of predictions in any given study will have low "empirical content" and should therefore be downgraded a priori as being less "severely testable" (Popper, 2002, pp. 95, 108; see also Szollosi & Donkin, 2021, pp. 2-3; Rubin, 2017c, p. 316; Rubin, 2020a, p. 378; Rubin & Donkin, 2022, p. 17). However, from an error statistical perspective, if a test procedure allows sample-based validity-enhancing deviations in its implementation, then its capability for severe tests will be reduced by an unknown extent because each new sample may necessitate the addition of a new test in the experimental testing context, leading to an unknown inflation of the procedure's Type I (familywise) error rate. It is simply not feasible to license Mayoian severity via pre-data error probabilities across "a long series of trials of this experiment" (Mayo, 1996, p. 181) when the specifications of "this experiment" may change from one trial to the next in response to sample-based information.

In summary, a preregistered test procedure that allows sample-based validity-enhancing deviations in its implementation will suffer an unknown inflation of its "actual" Type I (familywise) error rate. Consequently, it will not provide a more transparent evaluation of Mayoian severity than a non-preregistered procedure. The best we can do in these cases is conclude that Mayoian severity is "low because we don't have a clue how to compute it!" (Mayo, 2018, p. 201; see also Mayo, 1996, p. 313; Mayo, 2018, p. 280).

## Conditional Inference

One solution to the forking paths problem is to limit inferences to the single analytical path that has been followed in the current sample (e.g., Welch's *t*-test) rather than the two paths that would be followed in a hypothetical long run of repeated random sampling (e.g., Student's t-test & Welch's *t*-test; Rubin, 2017b, p. 327; 2020a, 2024b). This *conditional inference* approach is consistent with Fisher's (1955, 1956) theory of significance testing, which refers to a hypothetical population that represents the currently observed sample "in all relevant respects" (Fisher 1955, p. 72). To allow the opportunity for scientific progress and "learning by experience" (Fisher, 1955, p. 73; Fisher, 1956, pp. 99-100), this population is assumed to contain undiscovered "relevant subsets" (subpopulations) that represent exceptions to the current conditional inference (Fisher, 1956, pp. 32–33, 55, 57, 80, 85-88; Rubin, 2020b, 2021a).

Fisherian conditional inference is incompatible with the Neyman-Pearson theory of hypothesis testing, in which Type I and II error rates are assumed to apply *unconditionally* across a fixed, fully-known, and well-specified population (Fisher, 1955, p. 71; Rubin, 2021a, p. 5825). As Lehmann (1993) explained, the issue of conditional versus unconditional inference "seems to lie at the heart of the cases in which the two theories disagree on specific tests" (p. 1246).

In an attempt to bridge the gap between the two theories, Mayo (2014) proposed that we can use a "weak conditionality principle" to condition Neyman-Pearson long-run error rates on "the experiment actually run" (p. 232; see also Mayo & Cox, 2010; Mayo, 2018, pp. 171-173). However, unlike Fisherian conditionality, "there is no suggestion…that only the particular data set be considered" (Mayo, 2018, p. 172; cf. Rubin, 2021a, p. 5822). Instead, consistent with the Neyman-Pearson approach, we must continue to consider "how the procedure would behave in general, not just with these data, but with other possible data sets in the sample space" (Mayo, 2018, p. 53; see also Mayo, 2008, p. 876). Consequently, even "conditional" Neyman-Pearson error rates may be inflated to an unknown extent when "the experiment actually run" allows sample-based validity-enhancing deviations in its implementation, because other possible data sets may necessitate other deviation-based tests in the experimental testing context.

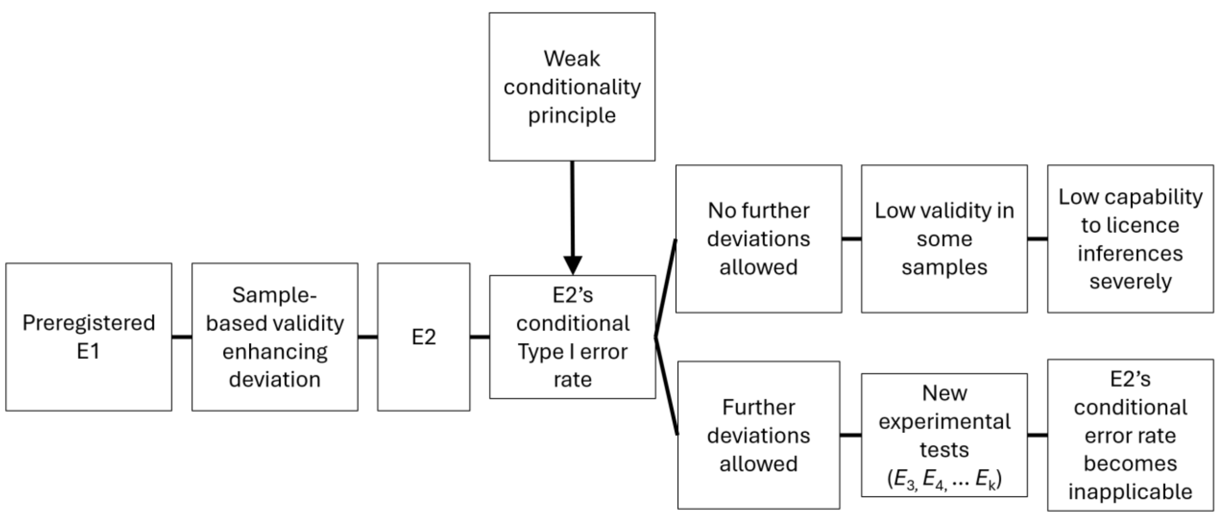
To illustrate, imagine that a researcher deviates from a preregistered experiment $E_1$ in order to maintain the validity of their test following an unexpected event in the current sample. Based on the weak conditionality principle, they may exclude $E_1$ from their test procedure and condition their Neyman-Pearson long-run error rate on "the experiment actually run" (Mayo, 2014, p. 232), which we can denote as $E_2$ (e.g., Mayo & Cox, 2010, pp. 271-272, Example 4). Consistent with a theory-centric approach, this conditioning allows us to treat $E_2$ as an individual test rather than a union-intersection test of a "mixture experiment" (i.e., "$E_1$ & $E_2$"; Mayo, 2014, p. 228; Rubin, 2021b, 10973-10974). However, in this case, the researcher must also imagine that a long run of repetitions of $E_2$ would encounter random samples in relevant subsets of its population that require further validity-enhancing deviations. They might deal with this issue in one of two ways.

First, the researcher may prohibit any further sample-based validity-enhancing deviations during $E_2$'s implementation. In other words, they may permanently fix the current specification of $E_2$. This approach would preserve the applicability of $E_2$'s conditional long-run error rate. However, it is inconsistent with the researcher's previous rationale for changing from $E_1$ to $E_2$, and it implies that $E_2$ will lack validity in relation to some of the samples that it encounters during its hypothetical repetitions (e.g., Lakens, 2024, p. 5; Rubin, 2017b, p. 326; Rubin & Donkin, 2022, p. 12; see also García-Pérez, 2012, p. 5). Mayoian severity requires that a test procedure's statistical assumptions are approximately satisfied during testing because statistical misspecification may inflate the procedure's "actual" error probability (Mayo, 2008, pp. 863-864; Mayo, 2018, p. 94; Mayo & Spanos, 2006, p. 349; Mayo & Spanos, 2011, pp. 189-190). $E_2$'s fixed test procedure is designed to fail this criterion because it cannot be modified when statistical assumptions are violated in particular samples. Consequently, consistent with the error statistical approach's counterfactual reasoning, $E_2$ will have a low capability to licence specific inferences severely.

Second, the researcher may continue to allow sample-based validity-enhancing deviations of their test procedure. However, as discussed previously, this approach will open up an unknown number of forking paths in the experimental testing context, with each path specifying a new experimental test (i.e., $E_3, E_4, \ldots E_k$). $E_2$'s "computed" conditional Neyman-Pearson error rate will not apply across this "garden of forking paths" (Gelman & Loken, 2013; i.e., the mixture experiment "$E_2$ & $E_3$ & $E_4$ & … $E_k$"). Furthermore, it will not be possible to compute an "actual" unconditional Neyman-Pearson error rate for the test procedure because the number of tests in the experimental testing context ($k$) will be unknown (for related points, see Mayo, 1996, p. 317; Mayo, 2018, p. 39). Figure 2 illustrates this situation.

**Figure 2**

*Illustration of the Application of the Weak Conditionality Principle*



In summary, there is often a "trade-off" between the goals of Mayoian severity and validity (Lakens, 2024, p. 5). If a test procedure disallows sample-based validity-enhancing deviations in its implementation, then it will lack validity in some samples, and if it allows them, then its "computed" conditional error rate will become inapplicable and it will suffer an unknown inflation of its "actual" unconditional error rate. Mayoian severity will be compromised in both cases because it requires a method to be both valid and reliable (Mayo, 2008, pp. 863-864; Mayo & Spanos, 2006, pp. 349-350; Mayo & Spanos, 2011, p. 167).

# Summary and Conclusion

Preregistration represents a contentious solution to an ill-defined problem. Previous justifications for this research practice have focused on the distinction between "exploratory" and "confirmatory" research. However, this distinction lacks a clear statistical or philosophical basis (Devezer et al., 2021; Rubin, 2020a, 2022a; Rubin & Donkin, 2022; Szollosi & Donkin, 2021).

Lakens and colleagues provide a more coherent justification based on Mayo's (1996, 2018) error statistical approach (Lakens, 2019, 2024; Lakens et al., 2024; see also Vize et al., 2024). From this perspective, the goal of preregistration is to allow others to transparently evaluate the capability of a test procedure to license inferences severely.

However, Mayo's (1996, 2018) error statistical conceptualization of severity is only one of several conceptualizations (Bandyopadhyay & Brittan, 2006; Hellman, 1997; Hitchcock & Sober, 2004; Horwich, 1982; Lakatos, 1968; Laudan, 1997; Popper, 1962, 1983; van Dongen et al., 2023). In the present article, I focused on Popper's conceptualization and showed that preregistration does not improve the transparent evaluation of severity or sincerity in his theory-centric approach.

I also showed that preregistration does not improve the transparent evaluation of Mayoian severity when deviations are allowed. In particular, I argued that a preregistered test procedure that allows sample-based validity-enhancing deviations in its implementation will have an unknown inflation of its Type I (familywise) error rate and, consequently, an unknown reduction of its capability to license inferences with Mayoian severity.

In conclusion, Mayo's (1996, 2018) error statistical approach justifies the use of well-specified preregistered research plans to allow others to evaluate the capability of a test procedure to license inferences severely. However, preregistration does not improve the transparent evaluation of severity in Popper's philosophy of science or when sample-based validity-enhancing deviations are allowed.

# Endnotes

1. Popper (1974a, pp. 74, 108-109) distinguished between three worlds. World 1 is the physical world. World 2 is the private psychological world of subjective beliefs, intentions, and experiences. Finally, World 3 is the public epistemological world of objective problems, theories, and reasons. Similar to Worlds 2 and 3, Reichenbach (1938, pp. 6-7) distinguished between (a) "processes of thinking in their actual occurrence" in the psychological "context of discovery" and (b) a "rational reconstruction" or "logical substitute" of these subjective thought processes in an epistemological "context of justification."
2. Popper (1962, pp. 247-248) argued that, for scientific knowledge to grow, theories must replace one another based on their successful predictions of new effects in crucial tests. However, determining which effects are "new" ($p[e, b] \ll ½$) and which tests are "crucial" ($h$ vs. $h'$) requires a rational reconstruction of the historical problem-situation in which the test took place. There are two points to note here. First, different rational reconstructions at

different points in the history of science will yield different conclusions (Lakatos, 1978, pp. 79, 86). Hence, contrary to the error statistical approach, judgements of Popperian sincerity may vary over time (Chalmer, 2010, p. 60). Second, rational reconstructions in World 3 do not require a consideration of researchers' subjective experiences in World 2. Indeed, a researcher's conscious experience of a problem-situation may be quite different from a rational reconstruction of that situation (Popper, 1974a, pp. 179, 242).

3. Following the Neyman-Pearson approach to statistical hypothesis testing, Mayo (1996) conceptualizes error probabilities as occurring "in a long series of trials of this experiment" (p. 181; see also Mayo & Spanos, 2011, p. 162). The problem with this approach is that it assumes that we know the theoretically relevant and irrelevant aspects of "this experiment" (Staley, 2002, pp. 288-289). Mayo (1996, pp. 172-173, 298-299) did not consider this *reference class problem* to be a problem for her approach. However, Fisher (1955, p. 71; 1956, pp. 77-78, 82, 91) saw it as a fatal flaw in the application of the Neyman-Pearson approach to scientific investigations because, as scientists, we must concede that we do not fully understand the relevant and irrelevant aspects of our experimental procedures (Rubin, 2020b; Schaller, 2016, p. 108). Popper would agree that our experimental procedures are "impregnated" with fallible theories (Popper, 1974, p. 145; Popper, 2002, p. 94; see also Lakatos, 1978, p. 54; Popper, 1962, p. 238). Consequently, we must imagine that "a long series of trials of this experiment" will contain currently unknown, theoretically relevant variations of "this experiment" (relevant subsets; hidden moderators) that imply different error probabilities (Fisher, 1955, p. 71; Fisher, 1956, p. 33; see also Popper, 1967, pp. 38-39).
4. Popper argued that "a hypothesis can only be empirically *tested*…*after* it has been advanced" (Popper, 2002, p. 7). However, he made this point to distinguish between inductivism and deductivism rather than the pre- and post-designation of specific tests, and predictions can be deduced from hypotheses *after* their corroborating or refuting results have been obtained (Rubin & Donkin, 2022; see also Lakatos, 1978, pp. 35, 72-73, 114, 185). Popper also argued that a study's methodological decisions require agreement among investigators about what counts as unproblematic background knowledge (Popper, 2002, p. 86) and acceptance of an intersubjectively testable experiment (Popper, 2002, p. 63), including "*criteria of refutation*…laid down beforehand" (Popper, 1962, p. 38, Footnote 3). However, a collective critical rational discussion can be used to agree on the *post-data* specification of an *unplanned* test as well as the *pre-data* specification of a *planned* test. Furthermore, even the pre-data specification of a planned test is temporary, tentative, and open to challenge and revision "at any time" (Popper, 1962, p. 238; Popper, 1994, p. 160). As Lakatos (1978, p. 42) explained, the interpretative theory represented by a test's "unproblematic" background knowledge may be replaced by a better one that implies different "criteria of refutation" and, therefore, different logical refutations and corroborations. In this respect, a preregistered plan represents a narrow out-of-date snapshot of a wider ongoing critical discussion that takes account of constantly evolving background knowledge and scientific standards (Lakatos, 1978, p. 36; Popper, 1983, p. 189, Footnote 3).

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*Peer review*: This article has not yet undergone formal peer review.

*Acknowledgments*: I am grateful to Deborah Mayo for her comments on a previous version of this article. Please note that she does not agree with all of the points I have made.

*Funding:* I declare no funding sources.

*Conflict of interest:* I declare no conflict of interest.

*Biography*: I am a professor of psychology at Durham University, UK. For further information about my work in this area, please visit https://sites.google.com/site/markrubinsocialpsychresearch/replication-crisis

*Correspondence:* Correspondence should be addressed to Mark Rubin at the Department of Psychology, Durham University, South Road, Durham, DH1 3LE, UK. E-mail: Mark-Rubin@outlook.com