Subject: Final response regarding the Louisiana infant-mortality preprint

Dear Steve,

This will be my final response, because the same question has now been repeated in several forms, and repetition does not change the methodological answer. Your central demand is that, before I may reject the paper's conclusion, I must identify a single confounder that reproduces all the reported patterns. That demand reverses the burden of proof and misunderstands the problem. The primary defect is not that I have failed to guess one hidden variable. The primary defect is that the study design cannot estimate the quantity that the paper repeatedly claims to have estimated.

I am not alleging that the Louisiana records were fabricated. I am not alleging fraud, and I do not need to. I am also not asserting that this analysis proves vaccines are safe. The point is narrower, more rigorous, and decisive: the records and calculations presented in this manuscript do not establish that vaccination increases infant mortality. Authentic data can be analyzed in a way that does not answer the stated question. Arithmetic can be correct while the scientific inference is invalid. A sincere observation can be as real as an observation and still fail to represent the causal reality of the population.

That distinction is the entire issue.

1. The manuscript does not estimate mortality risk

The paper begins with approximately 5,800 children who died before their third birthday. It then limits the data to 1,775 deceased children who could be exactly matched to an immunization record, excludes 550 who died before day 90, and analyzes 1,225 children. Every one of those 1,225 children died before age three. There are no surviving children in the analytic sample.

The manuscript nevertheless labels children who died between days 90 and 120 as having "died" and children who died after day 120 as "alive." That is not a valid mortality comparison. The second group is not alive in the sense required for a mortality analysis; it consists of later deaths. The study, therefore, compares earlier death with later death among a selected group of children who all eventually died. It does not compare death with survival among vaccinated and unvaccinated infants.

The quantity available from these data is approximately this: among children who survived to day 90, died before age three, had a successfully linked immunization record, and met the other selection rules, was death more likely to occur during days 90 through 120 rather than later? That is a conditional age-at-death question. It is not the probability of death among vaccinated infants compared with the probability of death among unvaccinated infants. It is not a mortality rate, a mortality risk, or a mortality hazard in the birth population.

A mortality risk requires a denominator containing the people at risk, including those who do not die during follow-up. A mortality rate is defined as deaths per unit of population-time. A hazard requires a properly defined risk set that is followed over time. None of those quantities can be recovered from a dataset that contains only future decedents. The absent survivors are not a cosmetic omission. They are the information needed to answer the paper's central question.

A simple example shows why this is not semantics. Imagine 100,000 vaccinated infants and 100,000 comparison infants. Suppose 1,000 infants in each group die before age three, so the overall mortality risk is exactly equal. Among the vaccinated deaths, suppose 300 occur during days 90-120 and 700 occur later. Among the comparison deaths, suppose 150 occur during days 90 through 120, and 850 occur later. A death-only analysis would produce an early-versus-late odds ratio of approximately 2.43, even though vaccination had no effect whatsoever on the probability of death.

The same conditional odds ratio could coexist with lower mortality among the vaccinated. For example, there could be 800 deaths among 100,000 vaccinated infants, with 240 occurring in the selected early window, and 1,200 deaths among 100,000 comparison infants, with 180 occurring in that window. The early-versus-late odds ratio among decedents would again be approximately 2.43, while the overall mortality risk would actually be lower in the vaccinated group. The reverse could also be constructed. In other words, the same pattern among decedents is compatible with harm, no effect, or protection in the full population.

That is non-identification. It means the observed death-only table cannot distinguish among fundamentally different realities because the required denominators and survivors are absent. No p-value, confidence interval, forest plot, subgroup analysis, or rhetorical description can recover information that was never included in the data.

Your hypothetical example of 50 percent of vaccinated decedents dying soon after vaccination does not change this principle. An extreme clustering pattern might warrant an urgent investigation with a valid design. It could be called an anomaly or a safety signal that generates hypotheses. But even a dramatic conditional pattern among people selected because they eventually died would not, by itself, establish that vaccination increased the probability of death in the population. The size of a pattern may change the urgency of follow-up; it does not change what the design is capable of estimating.

2. The demand to name one confounder is scientifically misplaced

You have repeatedly asked me to identify "the" specific confounder that explains every result. That is a category error. Confounding is only one possible source of error, and it is not the central problem here. The central problem is selection on the outcome, leading to an inability to identify the claimed estimand.

The analysis conditions on eventual death before age three, successful exact linkage to an immunization record, survival to at least day 90, and additional vaccine-specific inclusion and exclusion criteria. Conditioning on a future outcome can create associations among variables that would not exist in the source population. This is commonly described as selection bias or collider bias. The mechanism does not require one magical variable with a single, perfectly specified effect. It can arise from the combined structure of health status, healthcare utilization, vaccination timing, registry capture, age at death, calendar time, and the selection rules themselves.

There are many plausible contributors. Prematurity, congenital abnormalities, neonatal intensive-care admission, chronic illness, hospitalization, acute infection, clinician-directed delay of vaccination, access to well-child care, socioeconomic circumstances, insurance status, geographic mobility, out-of-state care, provider reporting practices, and completeness of registry records can

all influence whether and when vaccination is documented. Many of the same factors can influence age at death, cause of death, likelihood of exact linkage, and inclusion in this selected dataset. Calendar year and product availability can influence which vaccines appear in the record and the amount of follow-up available. None of these possibilities must operate alone.

More importantly, I am not required to prove which combination generated the observed table before concluding that the table does not identify a mortality effect. When a design admits multiple incompatible causal explanations, the authors bear the burden of showing that their preferred explanation is identified and that reasonable alternatives have been addressed. The burden is not on the reviewer to invent and prove a complete alternative causal history for every cell in the table.

The proposition "no one has named a single confounder that explains everything, therefore vaccines caused the pattern" is an argument from ignorance. Scientific evidence does not become causal by default merely because an opponent has not reconstructed the exact data-generating process. In observational research, unmeasured selection, misclassification, correlated exposures, differential follow-up, and model misspecification are not eliminated by demanding one named culprit.

The correct question is not, "Can a critic tell us exactly which unobserved variable produced every estimate?" The correct questions are: What population generated these data? What quantity does the analysis identify? Are the exposed and comparison groups valid? Were all individuals at risk included? Were exposure and outcome ordered correctly? Was the follow-up complete? Were important common causes measured? Were the analyses prespecified? Can the result be reproduced? Regarding those questions, the manuscript does not meet the required standard for its conclusion.

3. The repeated same-direction findings are not independent confirmation

The claim that 55 analyses point in the same direction sounds compelling only if those analyses are treated as independent replications. They are not.

The same selected cohort, the same death-window definition, the same general comparison group, and the same underlying vaccination visits are reused repeatedly. The individual vaccine indicators are highly correlated because the recommended products are commonly administered at the same visit. The five-vaccine and six-vaccine combinations are constructed from those same exposures. The branded combination-product analyses overlap with the same children and the same outcome. Race and sex stratification divide the same selected sample; they do not create new studies. Cause-of-death analyses are additional slices of the same small set of selected deaths.

When several vaccine indicators largely mark the same healthcare encounter, a common selection mechanism or a common feature of visit attendance can push all estimates in the same direction. That consistency is not surprising evidence of six independent biological effects. It may simply show that the variables are correlated and inherit the same bias. Counting correlated analyses as though each were an independent confirmation exaggerates the evidentiary weight.

The assertion of a dose-response relationship is also unsupported. The study does not measure a coherent biological dose and then demonstrate a graded response while holding other factors

constant. It compares different, overlapping vaccine patterns and products used in different periods and clinical settings. The Vaxelis result, for example, is based on only 62 children, with 19 deaths in the selected early window. Such a small product-specific group is vulnerable to random instability, calendar-time effects, changes in registry practices, product uptake, differential follow-up, and provider and population characteristics. A larger point estimate in that subgroup is not, by itself, a dose-response finding.

The sex and race claims are similarly overstated. A result that is statistically significant in females but not statistically significant in males does not establish that the effect differs by sex. A formal interaction test is required. The same is true for comparisons between Black and White children. Subgroup point estimates can differ because of sampling variability, small cells, unequal covariate distributions, or the same selection process operating differently across strata. Biological literature describing sex differences in immune response may make a hypothesis plausible, but it cannot validate this study's causal interpretation after the fact.

The cause-specific claims are even more fragile. They depend on very small numbers, including a comparison group of 19 females and categories with zero observed events. Zero events in a group of 19 does not establish zero risk. Post hoc grouping of causes, comparison with a national ecological chart, and counting a handful of infectious or nervous-system deaths do not establish a vaccine-specific causal mechanism. Sparse data require great restraint, not stronger language.

Thus, "same direction" does not rescue the study. The analyses share the same source population, the same selection, the same outcome definition, and correlated exposures. A repeated view of the same bias is not independent replication.

4. Invoking landmark analysis does not repair the source population

You have invoked the legitimacy of landmark analysis. A statistical method can be well established and still be inapplicable to a particular dataset or incapable of answering a particular question. The reputation of a method does not automatically transfer to every analysis that uses an age cutoff.

A valid landmark analysis ordinarily identifies a population alive and at risk at a defined landmark time, classifies exposure using information available up to that landmark, and then follows all members of that risk set forward to the outcome, with appropriate handling of censoring. It does not begin by selecting only individuals who are known, retrospectively, to have died before a later age. It does not treat people who die later as if they were non-death controls. It does not convert a conditional timing comparison among decedents into a population mortality effect.

At day 90, the relevant risk set would be all eligible infants alive and observable at that time, not merely those who would later appear in a death registry before age three. Those infants would then be followed, and both deaths and survivors would contribute information. The current manuscript lacks that risk set. It has been selected for a future event.

Moreover, the manuscript reports odds ratios from categorical tables and uses language such as "hazard" and "more likely to die" without presenting a population survival model. Calling the arrangement a landmark analysis does not make the reported odds ratio a hazard ratio, nor does it provide the missing denominator.

I do not need to rely on a dispute over the label "immortal-time bias" to reach my conclusion. Even if that term were removed from the discussion entirely, the death-only sampling and outcome-conditioned comparison remain sufficient grounds for rejection of the mortality claim. The central defect survives every terminological debate.

5. Reliable records are not the same as data fit for the question

You have emphasized that the data came from the State of Louisiana. The provenance of the records is relevant but not dispositive. A reliable source can provide data that are incomplete, selected, misclassified for a particular purpose, or simply incapable of answering the proposed question.

The paper reports that only 1,775 of approximately 5,800 deaths were exactly matched to an immunization record - fewer than one-third. That is a substantial selection step. The manuscript does not demonstrate that matched and unmatched children were comparable with respect to age at death, year, race, sex, geography, healthcare access, migration, provider type, underlying illness, or cause of death. Exact-match success is unlikely to be purely random. Restricting the analysis to matched records can therefore introduce an additional layer of selection bias.

The comparison group is also mislabeled. Children without a qualifying vaccine recorded between days 60 and 90 are called "unvaccinated," although the linked dataset requires at least one immunization record, and those children may have been vaccinated before day 60, after day 90, outside the state, through a provider with incomplete reporting, or with other vaccines. They may also have had vaccination delayed because of illness, hospitalization, prematurity, or medical advice. Those are clinically different circumstances. Combining them under a single label creates exposure misclassification and obscures the actual comparison.

The manuscript also does not provide the information necessary to evaluate linkage quality, missing data, complete vaccination histories, duplicate or implausible dates, same-day vaccination and death, calendar-year comparability, or complete follow-up through age three. If recent birth cohorts did not all have the opportunity to be observed to age three, early deaths can be counted before later deaths have had time to occur. Product-specific analyses are especially vulnerable because products enter use in different calendar periods.

Nor are the raw analytic data, full code, data dictionary, complete contingency tables, linkage specifications, and prespecified protocol made available in a form that permits independent confirmation. Mentioning a software package is not reproducibility. A result that cannot be independently reconstructed must be treated with additional caution, particularly when the public claim is strong and politically charged.

The distinction can be put simply: a perfectly accurate list of people who died cannot tell us the mortality risk among people who did and did not receive an exposure if the people who survived are missing. Data quality and design fitness are separate questions. A calibrated thermometer may be reliable, but it cannot measure blood pressure. Likewise, authentic death and immunization records do not automatically identify the effect of vaccination on mortality.

6. What statistics can and cannot establish

You have emphasized the statistical significance and consistency of the reported findings. Statistics are not a ceremonial stamp placed on a table after the data are collected. Statistical tests have meaning only under the sampling structure, outcome definition, model assumptions, and analytic plan that connect the observed data to the scientific question.

A small p-value addresses sampling variation under a specified model. It does not measure selection bias, eliminate exposure misclassification, correct uncontrolled confounding, repair differential follow-up, transform correlated tests into independent tests, or convert a conditional age-at-death comparison into a mortality risk. An estimate can be statistically precise and scientifically wrong. Greater precision around a biased estimate does not create validity; it can merely make the wrong answer look more authoritative.

The manuscript performs numerous overlapping comparisons across individual vaccines, vaccine combinations, branded products, sex, race, and causes of death. It does not identify a single prespecified primary hypothesis or adequately address multiplicity. When many related analyses are performed, some nominally significant findings are expected, and selective emphasis on the largest or most striking estimates magnifies the problem. Correlation among tests complicates the count, but it certainly does not justify treating each same-direction result as independent evidence.

Small sample sizes are relevant in several subgroups and cause-of-death categories, but the more fundamental point is that sample size cannot fix structural non-identification. Even one million selected decedents would not reveal the mortality risk among all vaccinated and comparison infants if the survivors and population denominators remained absent. More data of the wrong kind does not answer the right question.

From a Bayesian perspective, evidence should change our confidence only to the extent that the data-generating model distinguishes among competing explanations. Here, the observed conditional table is compatible with multiple underlying population realities: increased, unchanged, or decreased mortality. Because the selection mechanism and denominators are unobserved, these data have limited ability to discriminate among those causal hypotheses. A dramatic point estimate does not deserve a dramatic posterior conclusion when the design leaves the central question unidentified.

This is why an observation is not automatically a fact about causation. An observation is what appears in a dataset after applying specific inclusion rules, measurements, and classifications. The scientific task is to determine whether the observation reflects the target population and whether alternative explanations have been ruled out. The bridge between an observation and a defensible conclusion is the study design. Here, that bridge is missing.

7. Data limitations do not entitle authors to stronger conclusions

You have argued that the authors should not be criticized for lacking a full birth cohort because Louisiana did not provide one. I do not criticize them for failing to possess data they were not given. I criticize the manuscript for making claims that the available data cannot support.

The rule is straightforward: limitations in the data limit the conclusions. They do not lower the standard of evidence, and they do not create a concession under which an otherwise unidentified

causal claim becomes acceptable. Researchers are often constrained by the data available to them. The scientifically honest response is to narrow the question, narrow the language, and state precisely what can and cannot be inferred.

With this dataset, the authors could present a descriptive analysis of vaccination records and age at death among successfully linked decedents, accompanied by strong warnings about selection bias, linkage bias, exposure classification, and the lack of a population denominator. They could describe the work as hypothesis-generating and call for a proper cohort or risk-set study. What they cannot responsibly say is that vaccinated children were more likely to die, that mortality rates were elevated, or that the analysis demonstrates statistically significant harm. Those statements claim a population effect that was not estimated.

Scientific standards do not relax because a preferred dataset is unavailable. A bridge that stops halfway across a river does not become complete because the builder lacked materials. The appropriate conclusion is that the crossing has not been established, not that readers must accept the destination on faith.

8. Publication, rejection, and preprints do not determine scientific truth

You have framed the removal or rejection of the manuscript as censorship and suggested that a preprint platform should surface the result because it challenges conventional thinking. This confuses the freedom to advance a hypothesis with an entitlement to have a platform host, endorse, index, or preserve unsupported claims in their current form.

A preprint is not peer-reviewed, and it does not confer immunity from scientific standards. Early dissemination can be valuable, but it increases the obligation to accurately describe uncertainty, as unreviewed claims can be amplified before errors are corrected. A platform or journal may reasonably decline or withdraw a manuscript when the title, abstract, results, and conclusion materially overstate what the design can establish. That decision is not proof that the manuscript is false, but neither is posting the manuscript proof that it is true.

The history of acceptance, rejection, posting, or withdrawal is not the basis of my scientific judgment. Even a unanimous publication would not correct a missing denominator. Even a unanimous rejection would not, by itself, prove a hypothesis false. The relevant question remains whether the methods support the stated conclusion. In this case, they do not.

A reviewer is not required to accept a claim because the authors have been unable to obtain more complete data, because several venues have declined it, because the subject is politically controversial, or because the observation has attracted a large audience. Reviewers assess whether the conclusions follow from the evidence presented. Sincerity, persistence, popularity, and controversy are not substitutes for identification, validity, and reproducibility.

Nor must a reviewer prove fraud before recommending rejection. Most rejected scientific papers are not fraudulent. They may address an important question but use an inadequate design, measure the wrong quantity, overstate a limited result, or fail to exclude obvious alternative explanations. There is a large and ordinary category between "fraud" and "publish as established truth": honest data with unsupported conclusions. That is the category at issue here.

If the authors believe the critique is wrong, the appropriate response is a point-by-point scientific rebuttal. They should state the estimand, explain how a death-only sample identifies population mortality, provide the complete tables and code, address linkage and follow-up, correct the exposure labels, demonstrate valid interaction and multiplicity analyses, and show why alternative causal structures are excluded. Publicity cannot substitute for that work.

9. Science and politics must not be conflated

Science and politics inevitably meet when evidence informs public policy. But their roles must remain distinct. Science evaluates what the evidence can support. Politics decides what society will do after considering reliable evidence, values, resources, and law. Political authority cannot determine whether a comparison reflects a valid risk set, whether a comparison group is correctly defined, or whether a causal effect is identified.

Invoking Robert F. Kennedy Jr., the United States Senate, a press representative, a large readership, an advocacy organization, a journal's reputation, or a social-media audience adds no scientific information. No public official can supply the missing survivors. No press campaign can validate an incorrect outcome label. No number of followers can turn overlapping subgroup analyses into independent replications. No accusation of censorship can convert a conditional timing association among decedents into a population mortality effect.

This standard must be applied without regard to political direction. If a vaccine manufacturer used the same death-only design to claim that vaccination reduced infant mortality, I would reject that conclusion for exactly the same reason. If an anti-vaccine organization uses it to claim harm, the answer is the same. Methodological validity is not partisan. A result does not become stronger because it favors or challenges an established policy.

Political advocacy before scientific validation is especially inappropriate when the subject is infant death and vaccination. These issues carry enormous emotional and public-health consequences. Overstating a weak or unidentified result can frighten parents, distort clinical decisions, erode trust, and make genuine safety investigations more difficult. High stakes require higher discipline, not lower standards.

I therefore will not allow my review to be converted into a political dispute or a public relations campaign. I will not judge the manuscript by who supports it, who opposes it, or how much attention it receives. I will judge only the design, analysis, and conclusions presented. On that basis, the mortality claim fails.

10. What would be required to answer the question scientifically

The question of whether vaccination at a particular age affects mortality deserves a study capable of answering it. At minimum, such a study would require a population-based birth cohort that includes both survivors and deaths; a clearly defined time origin and risk set; complete or properly censored follow-up; vaccination treated accurately, potentially as a time-varying exposure; valid comparison groups; verification that exposure precedes outcome; and adjustment for prespecified clinical, demographic, socioeconomic, healthcare-utilization, and calendar-time factors.

The investigators would need to validate linkage between birth, clinical, immunization, and death records; characterize matched and unmatched individuals; account for migration and incomplete

registry capture; distinguish delayed, partial, out-of-state, and undocumented vaccination from true non-vaccination; and establish that each birth cohort had an equal opportunity to reach the relevant outcome period. Product-specific analyses would need to address year of introduction, provider uptake, coadministration, and collinearity.

The analysis should identify a prespecified primary exposure, outcome, and risk window; provide a causal model; report complete contingency tables and covariate balance; use appropriate survival, cohort, or risk-set case-control methods; test interaction directly rather than comparing significance labels; account for multiple comparisons; and use methods suitable for sparse cause-specific outcomes. The protocol, code, data dictionary, linkage rules, and analytic decisions should be available for independent review to the extent permitted by privacy law.

A nested case-control design could also be valid if controls were sampled from infants who were alive and at risk at the time each case died. Controls cannot be selected solely from children who died later, because that again conditions the comparison on future death. Alternatively, a self-controlled design might be considered for certain acute risk windows, provided its assumptions are justified and time-varying age and season effects are handled appropriately. The correct design depends on the precise hypothesis, but every valid option must preserve an appropriate risk set and include the information needed to compare event occurrence, not merely event timing among future decedents.

Replication in independent jurisdictions would then be essential. Replication means reproducing a prespecified analysis in an independent source population, not repeatedly subdividing one selected dataset. If a credible population-based analysis found a consistent association, it should be investigated seriously regardless of whether the result is politically convenient. That is how science should proceed.

Final position

My final position is therefore unambiguous.

The manuscript may contain a descriptive observation about the timing of death among a selected group of linked decedents. It does not demonstrate that vaccination increased the probability of infant death. It does not estimate a vaccinated-versus-unvaccinated mortality rate, risk, or hazard. The repeated analyses are not independent replications; the subgroup and cause-specific claims are overinterpreted; and the available statistics do not address selection, classification, linkage, follow-up, and multiplicity problems.

Even if the records are authentic and every reported calculation can be reproduced, the central conclusion still does not follow. The data source may be reputable, but the data are not sufficient to establish the claim. The state's inability or refusal to provide a full cohort is grounds for limiting the conclusion, not a license to draw an unsupported one.

At most, the observation could justify a carefully framed request for a better study. It cannot responsibly be used to tell parents that vaccination makes infants more likely to die, to alter clinical or regulatory policy, to establish a causal safety finding, or to support a political campaign. Calling it a "signal" does not change that boundary: a signal is a prompt for verification, not a verified conclusion.

I will consider a rigorous point-by-point scientific response, a corrected descriptive manuscript, or new evidence from a design that actually estimates mortality in an at-risk population. I will not reconsider the current conclusion in response to media pressure, political names, audience size, accusations of censorship, or repetition of the demand that a reviewer identify one hidden confounder.

Science is not advanced by insisting that an observation must be treated as reality until someone proves every alternative explanation. Science advances by designing studies that distinguish among those explanations. This manuscript does not do so.

Accordingly, I cannot endorse its conclusions, support its presentation as evidence of vaccine-caused mortality, or participate in its politicization. My scientific review is complete.

Best regards.
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