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Treatment-administered drugs are reported as positive screens in a trauma registry: an audit of compliance with the National Trauma Data Standard exclusion rule

Akbar Ali

ali55@marshall.edu

Marshall University

Christopher Potts

Marshall University

Shivam Gandhi

Marshall University

Usman Alizai

The Ohio State University Wexner Medical Center

Dakota May

Marshall University

Mariam Ghafoor

Marshall University

Research Article

Keywords: trauma registry, data quality, National Trauma Data Standard, urine drug screen, substance use, medical record abstraction, older adults

Posted Date: September 1st, 2026

DOI: https://doi.org/10.21203/rs.3.rs-10768116/v1

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Additional Declarations: No competing interests reported.

Abstract

Background.

The National Trauma Data Standard (NTDS) Drug Screen element instructs abstractors to report "None" when the only positive results are attributable to drugs administered during care, and to report only those drugs not administered when multiple are detected. Whether this exclusion rule is applied in practice has not been reported. Because opioid and benzodiazepine analgesia is given more often to older injured patients, unapplied exclusion could bias the recorded age distribution of substance use.

Methods.

We audited consecutive trauma registry records at a single Level II centre from 1 January 2023 to 31 December 2024. Prehospital and emergency-department medication administration was parsed from free-text registry fields and cross-tabulated against the recorded Drug Screen value. Records were classified as noncompliant under three definitions of increasing stringency: a liberal class-level definition, the rule as written (primary), and an assay-restrictive definition requiring pharmacologically plausible analyte-to-agent correspondence. Prevalence was recomputed with the rule applied and compared with recorded prevalence across age bands.

Results.

Of 2,538 registry encounters, 1,421 had a Drug Screen value other than "Not Tested," and 914 (64.3%) were recorded as positive for at least one analyte. Under the rule as written, 569 of 914 positive records (62.3%, 95% CI 59.1–65.3) contained at least one analyte the standard directs be excluded, and 350 (38.3%, 95% CI 35.2–41.5) should have been reported as "None." Applying the rule reduced positivity from 64.3% to 39.7%. Noncompliance increased monotonically with age, from 25.5% of positive records at ages 16–45 to 51.4% at 81 years and older (odds ratio 1.27 per decade, 95% CI 1.18–1.36). The recorded prevalence ratio for ages 16–45 versus 81 and older was 2.21 (95% CI 1.89–2.59); after applying the rule it was 3.39 (95% CI 2.61–4.40). Findings were directionally unchanged under both alternative definitions.

Conclusions.

At this centre the exclusion rule was not applied in most positive records, and the failure was concentrated among older patients, so the recorded element understates the age gradient in substance use. Verifying that abstractors apply the rule is a low-cost, testable data-quality target.

Background

Trauma registries are the empirical foundation of injury epidemiology. Estimates of injury burden, risk-factor associations, and centre performance derive from abstracted registry variables, and the credibility of those estimates rests on the fidelity of abstraction [1, 2]. Systematic reviews of trauma registry data quality have repeatedly identified variability in coding practice as a principal threat, with agreement between abstractors and source documents differing substantially across data elements and across centres [1, 3, 5]. Validation programmes have accordingly emphasised objective, element-specific audit rather than global accuracy claims [4].

Substance use is among the most consequential registry variables. It informs prevalence estimates that motivate screening programmes, it is used to adjust for confounding in outcome analyses, and it carries direct clinical implications for airway management, analgesia, and withdrawal risk [6, 7]. Reported positivity among injured patients varies widely across studies and populations, and consistent age differences have been described, with older patients screening positive less often than younger ones [8, 9, 11]. Screening practice itself varies by patient age and sex, which complicates interpretation of any single centre's positivity estimate [11, 12, 10].

An underappreciated complication is that drugs administered during trauma care are themselves detectable on the screening panel. Opioid and benzodiazepine analgesia and sedation are given routinely in the prehospital and resuscitation phases, and these agents produce true-positive immunoassay results indistinguishable from pre-injury use on a qualitative panel [13, 17]. Immunoassay panels are further subject to cross-reactivity and to variable sensitivity across congeners within a drug class, so the relationship between an administered agent and a reported analyte is not always one-to-one [13, 14, 15]. The clinical corollary has been described: intoxication attributed to pre-injury substance use may in part reflect therapeutic administration, with consequences for how respiratory depression and altered mental status are interpreted [16].

The National Trauma Data Standard anticipates this problem explicitly. The Drug Screen element instructs that Element Value "14. None" be reported for patients whose only positive results are due to drugs administered at any facility treating the patient event, and that when multiple drugs are detected, only drugs not administered during the event be reported [21]. The rule is unambiguous, and it places the burden of exclusion on the abstractor, who must reconcile the screen result against the medication administration record.

To our knowledge, compliance with this rule has not been measured. Its failure would have a specific and predictable signature: because analgesic and sedative administration is more frequent among older injured patients, unapplied exclusion would inflate recorded positivity differentially by age and would therefore compress the apparent age gradient in substance use. We audited a single centre's registry against this rule, quantified noncompliance under three definitions of increasing stringency, and estimated the effect of correct application on recorded prevalence and on its age distribution.

Methods

Design and setting. We conducted a retrospective audit of trauma registry records at a single American College of Surgeons–verified Level II trauma centre. The study period comprised consecutive registry entries with arrival dates from 1 January 2023 through 31 December 2024. The analysis compares registry-recorded values against the coding rule specified for the data element itself; it is therefore a coding-compliance audit rather than a validation study against an external reference standard, and no measurement of true substance exposure was available.

Data source and elements. Records were extracted from the institutional trauma registry. Two substance-related elements coexist in the extract. The Drug Screen element carries the NTDS value list (AMP, BAR, BZO, COC, mAMP, MDMA, MTD, OPI, OXY, PCP, TCA, THC, Other, None, Not Tested) and is the element transmitted to the National Trauma Data Bank; it is the subject of this audit. A second field, labelled Drug Use Indicators, carries locally defined values and has no NTDS equivalent; it was not analysed. At this centre the "Other" channel of the screening panel is designated for fentanyl, a local mapping we tested empirically (see Statistical analysis).

Medication administration was captured from two free-text registry fields recording prehospital and emergency-department medications. Agent names and common trade names were matched by case-insensitive keyword search across nine opioids, five benzodiazepines, ketamine, and phenobarbital. The full analyte-to-agent mapping, including the pharmacological rationale for each pairing and its assay caveats, is provided as Supplementary Table S1.

Definitions of noncompliance. The NTDS rule directs that only drugs not administered during the patient event be reported, and that "None" be reported when all positives are attributable to administration [21]. Because the correspondence between an administered agent and a reported analyte is not always one-to-one, we operationalised the rule at three levels of stringency and prespecified the middle level as primary.

The *rule-as-written* definition (primary) treats a reported analyte as excludable when an agent of the corresponding class was administered: any opioid for OPI, OXY, or MTD; any benzodiazepine for BZO; phenobarbital for BAR; and fentanyl for Other. The *assay-restrictive* definition additionally requires pharmacologically plausible analyte-to-agent correspondence, so that OPI is excludable only when morphine, hydromorphone, hydrocodone, or codeine was given, and OXY only when oxycodone was given. The *liberal* definition classifies a record as attributable when all reported analytes fall within classes obtainable from treatment and any opioid or benzodiazepine was administered, without requiring class-level matching. Analytes with no plausible treatment source in this cohort (AMP, mAMP, COC, THC, PCP, MDMA, TCA) were never excluded under any definition.

Two outcomes were derived per record: whether at least one reported analyte was excludable, and whether all reported analytes were excludable such that the record should have been coded "None."

Statistical analysis. Proportions are reported with Wilson 95% confidence intervals. Age was analysed both continuously and in prespecified bands (16–45, 46–65, 66–80, 81 years and older). The trend in

noncompliance with age among positive records was estimated by logistic regression with age entered per decade. To test whether the exclusion rule affects the age distribution of positivity rather than only its level, we fitted models with an age-by-definition interaction comparing recorded against rule-compliant positivity, on both the odds scale (logistic) and the prevalence-ratio scale (Poisson regression with robust variance). We report both because the two scales did not agree, and we interpret the discrepancy in the Discussion rather than selecting the more favourable result.

The local designation of the "Other" channel as fentanyl was validated by testing its association with documented fentanyl administration. Analyses were performed in Python 3.11 using pandas, statsmodels, and scipy. No imputation was performed; denominators are stated for every proportion.

Cohort definition and exclusions. Of 2,538 registry encounters in the study period, 1,117 carried the value "Not Tested" for Drug Screen and were excluded, leaving 1,421 tested records for the compliance analysis. Age-stratified analyses additionally excluded 1 patient aged under 16 years, leaving 1,420. The registry extract does not contain a stable patient identifier, so encounters rather than unique patients are the unit of analysis; readmissions within the period, if present, are counted separately.

Ethics. The study was reviewed and approved by the institutional review board, which granted a waiver of informed consent for this retrospective analysis of de-identified registry data.

Results

Cohort. The registry contained 2,538 encounters during the study period, 1,117 of which carried "Not Tested" for the Drug Screen element, leaving 1,421 tested records (56.0% of encounters). Of these, 914 (64.3 (61.8–66.8)%) were recorded as positive for at least one analyte and 507 were recorded as "None." Medication documentation was present for nearly all records; both the prehospital and emergency-department medication fields were blank in 58 of 2,538 encounters (2.3%). Cohort characteristics by age band appear in Table 1.

Table 1
Cohort and recorded Drug Screen positivity by age band.

Age (years)	Tested (n)	Recorded positive (n)	Recorded positivity, % (95% CI)
16–45	398	329	82.7 (78.6–86.1)
46–65	368	269	73.1 (68.3–77.4)
66–80	373	211	56.6 (51.5–61.5)
81+	281	105	37.4 (31.9–43.2)

Validation of the local "Other" designation. Among the 410 records reporting the "Other" analyte, 338 (82.4%) had documented fentanyl administration, compared with 37.1% of positive records not reporting

"Other" (odds ratio 7.96, $p < 0.001$). This supports the centre's designation of the "Other" channel for fentanyl and its treatment as excludable when fentanyl was given.

Compliance with the exclusion rule. Under the rule as written, 569 of 914 positive records (62.3 (59.1–65.3)%) contained at least one analyte that the standard directs be excluded, and 350 (38.3 (35.2–41.5)%) contained no analyte that should have been reported at all, meaning the record should have been coded "None." Applying the rule reduced recorded positivity from 64.3 (61.8–66.8)% to 39.7 (37.2–42.3)%.

The finding was not an artefact of definitional choice. Under the assay-restrictive definition, 529 positive records (57.9 (54.7–61.0)%) contained an excludable analyte and 274 (30.0 (27.1–33.0)%) required recoding to "None," yielding compliant positivity of 45.0 (42.5–47.6)%. Under the liberal definition 421 records (46.1 (42.9–49.3)%) required recoding, yielding compliant positivity of 34.7 (32.3–37.2)%. The three definitions bracket the estimate; all are reported in Table 2.

Table 2
Records requiring recoding to "None" and resulting positivity under three definitions of attributable positive (n = 914 positive records; 1,421 tested).

Definition of attributable positive	Records requiring "None", n (%, 95% CI)	Rule-compliant positivity, % (95% CI)
Liberal (class-level)	421 (46.1, 42.9–49.3)	34.7 (32.3–37.2)
Rule as written (primary)	350 (38.3, 35.2–41.5)	39.7 (37.2–42.3)
Assay-restrictive	274 (30.0, 27.1–33.0)	45.0 (42.5–47.6)

Age distribution of noncompliance. Noncompliance rose monotonically with age (Table 3, Fig. 1b). The proportion of positive records requiring recoding to "None" increased from 25.5 (21.1–30.5)% at ages 16–45 to 36.8 (31.3–42.7)% at 46–65, 53.6 (46.8–60.2)% at 66–80, and 51.4 (42.0–60.8)% at 81 years and older. In logistic regression among positive records, each decade of age was associated with 1.27-fold higher odds of noncompliance (95% CI 1.18–1.36, $p < 0.001$); the corresponding estimate under the assay-restrictive definition was 1.20 per decade (95% CI 1.12–1.29, $p < 0.001$).

Table 3
Noncompliance with the exclusion rule and rule-compliant positivity, by age band.

Age (years)	Recorded positive (n)	Requiring "None" (n)	Noncompliant share of positives, % (95% CI)	Rule-compliant positivity, % (95% CI)	Noncompliant share, assay-restrictive, % (95% CI)
16–45	329	84	25.5 (21.1–30.5)	61.6 (56.7–66.2)	20.7 (16.6–25.4)
46–65	269	99	36.8 (31.3–42.7)	46.2 (41.2–51.3)	30.1 (24.9–35.8)
66–80	211	113	53.6 (46.8–60.2)	26.3 (22.1–31.0)	37.0 (30.7–43.7)
81+	105	54	51.4 (42.0–60.8)	18.1 (14.1–23.1)	44.8 (35.6–54.3)

Table 4
Prevalence ratios for Drug Screen positivity by age band, recorded versus rule-compliant.

Age (years)	Recorded PR (95% CI)	Rule-compliant PR (95% CI)	Ratio of PRs (95% CI)	p
16–45	2.21 (1.89–2.59)	3.39 (2.61–4.40)	1.53 (1.13–2.08)	0.006
46–65	1.96 (1.66–2.30)	2.55 (1.94–3.34)	1.30 (0.95–1.79)	0.104
66–80	1.51 (1.27–1.80)	1.45 (1.07–1.96)	0.96 (0.67–1.35)	0.801
81+	1.00 (reference)	1.00 (reference)	1.00 (reference)	—

Recorded positivity fell steeply with age, from 82.7 (78.6–86.1)% at ages 16–45 to 37.4 (31.9–43.2)% at 81 and older. Applying the exclusion rule lowered positivity in every band but lowered it most in the oldest bands, so that compliant positivity ranged from 61.6 (56.7–66.2)% to 18.1 (14.1–23.1)% (Fig. 1a).

Effect on the age gradient, and scale dependence. On the prevalence-ratio scale, the gradient steepened after applying the rule. Taking patients 81 and older as reference, the recorded prevalence ratio for ages 16–45 was 2.21 (95% CI 1.89–2.59); the rule-compliant ratio was 3.39 (95% CI 2.61–4.40). The ratio of these prevalence ratios was 1.53 (95% CI 1.13–2.08, p = 0.006), and the overall age-by-definition interaction was significant (Wald $\chi^2(3) = 24.5$, $p < 0.001$), indicating that the effect of the coding rule differs across age bands on this scale (Fig. 1c).

On the odds scale the same interaction was not significant (odds ratio per decade for the age-by-definition term 1.018, 95% CI 0.938–1.104, p = 0.67). We report both results. The discrepancy is expected when an exposure is common, as here, because the odds ratio and prevalence ratio diverge substantially at high baseline prevalence, and the two scales answer different questions about what "differential" means. We do not claim the bias is differential on every scale; we claim that on the prevalence-ratio

scale, which is the scale on which registry prevalence estimates are reported and interpreted, correct application of the rule materially changes the estimated age gradient.

Discussion

In this single-centre registry audit, the NTDS Drug Screen exclusion rule was not applied in 62.3 (59.1–65.3)% of positive records, and 38.3 (35.2–41.5)% of positive records should have been coded "None" entirely. Correct application would reduce recorded positivity from 64.3% to 39.7%. The failure was not uniform: it was roughly twice as common among the oldest patients as among the youngest, so the recorded element compresses the age gradient in substance use relative to what the standard's own coding rule would produce.

The distinction between this finding and a claim about measurement validity is worth stating plainly. We are not reporting that an immunoassay panel cannot distinguish administered from pre-injury drug exposure; that limitation is well described [13, 17]. We are reporting that the data standard already anticipates the problem, specifies a remedy, and that the remedy went unapplied. This makes the finding actionable in a way that a validity limitation is not. Abstractors at this centre had access to the medication administration record — it is captured in the registry itself — and the reconciliation the standard requires was evidently not performed, or was performed inconsistently.

The age pattern has a straightforward mechanism. Older injured patients receive analgesia and sedation more often and are more often intubated or sedated for imaging, so treatment-attributable positives accumulate disproportionately in the oldest bands. Prior work has consistently reported lower screening positivity among older trauma patients [8, 11], and our compliant estimates are lower still in those bands, which suggests that the true age difference in substance use is larger than registry-derived figures indicate. This matters for how screening policy is targeted: the case for age-based screening thresholds rests on the observed gradient, and the observed gradient is attenuated by the coding error we describe.

The scale dependence of the interaction deserves emphasis rather than concealment. Prevalence in this cohort is high, and at high prevalence odds ratios and prevalence ratios diverge; a coding change that shifts prevalence substantially can move the prevalence-ratio gradient while leaving the odds-ratio gradient near-constant. Investigators reporting differential bias should state the scale explicitly. Our claim is scoped to the prevalence-ratio scale, which is the scale on which registry positivity is customarily reported and on which screening policy arguments are made.

Practically, verification of this element is inexpensive. It requires only that abstraction quality-assurance review cross-reference the medication administration record against the reported analyte list for records coded positive, an audit that can be automated where both fields are structured. Registry validation programmes have advocated exactly this kind of element-specific, objective check rather than global accuracy assessment [4, 2]. The broader stewardship literature has argued that the value of a diagnostic test depends on whether its result is interpreted correctly downstream [18, 19, 17]; a screening result

correctly obtained and then incorrectly abstracted is a specific instance of that failure, and the administrative cost of an unexamined checkbox has been noted directly in the trauma literature [20].

Limitations. The most important limitation is temporal. The registry extract records arrival dates without time components, and neither specimen collection times nor medication administration times were available. We can therefore establish that a drug was administered during the encounter and that a corresponding analyte was reported, but not that administration preceded collection for any individual patient. This limits causal interpretation of the co-occurrence. It does not, however, undermine the compliance finding, because the standard's rule is written in terms of administration "at any facility (or setting) treating this patient event" rather than in terms of temporal precedence [21]; a record in which a corresponding agent was administered during the event and the analyte was nonetheless reported is noncompliant on the face of the rule. Determining what fraction of these positives is genuinely treatment-caused, as opposed to pre-injury use coincidentally matching an administered agent, would require timestamped data from the electronic health record and is the natural next study.

Second, medication exposure was ascertained from free-text fields by keyword matching. Agents documented under unusual abbreviations or misspellings would be missed, which would bias our noncompliance estimate downward. Both medication fields were blank in only 2.3% of encounters, so gross under-ascertainment is unlikely, but we cannot exclude selective under-documentation.

Third, we lack information on the specific immunoassay platform, its cutoff concentrations, and the centre's written screening indications. Cross-reactivity profiles and detection thresholds differ across platforms [13, 14], and the assay-restrictive definition we report is based on general pharmacological plausibility rather than on this laboratory's validated cross-reactivity data. The screening policy is likewise unstated, so we cannot characterise which patients were selected for testing; recorded positivity here should not be read as population prevalence.

Fourth, the registry extract lacks a stable patient identifier, so the unit of analysis is the encounter. Repeat encounters by the same patient would be counted separately, which affects precision more than point estimates.

Fifth, this is a single centre, and abstraction practice is known to vary substantially across centres [5, 3]. Our compliance estimate should not be assumed to generalise. Whether it is typical, unusually poor, or unusually good is precisely the question a multicentre replication would answer, and the audit is simple enough to be replicated wherever both elements are captured.

Finally, we audited compliance against the rule; we did not adjudicate individual records against source clinical documentation. A record we classify as noncompliant could in principle reflect an abstractor's documented judgement that a positive result predated care. We regard this as unlikely to account for the age pattern, since such judgements would have to be systematically more frequent in older patients to do so.

Conclusions. The NTDS Drug Screen element carries an explicit rule excluding treatment-administered drugs, and at this centre the rule was not applied in most positive records. Because analgesic and sedative administration is more frequent in older injured patients, the resulting error is concentrated among them and compresses the recorded age gradient in substance use. Registry-derived substance-use estimates should be interpreted with attention to whether this exclusion was performed, and verification of the element is a low-cost, testable target for registry quality assurance.

Declarations

Ethics approval and consent to participate.

This study was reviewed and approved by the institutional review board, which granted a waiver of informed consent for retrospective analysis of de-identified registry data.

Consent for publication.

Not applicable.

Competing interests.

The authors declare no competing interests.

Funding.

This research received no specific grant from any funding agency.

Author Contribution

Akbar Ali: Conceptualization, Methodology, Formal analysis, Data curation, Writing – original draft, Writing – review and editingChristopher Potts: Conceptualization, Investigation, Data curation, Writing – original draft, Writing – review and editingShivam Gandhi: Investigation, Data curation, Writing – review and editingUsman Alizai: Investigation, Data curation, Writing – review and editingDakota May: Conceptualization, Supervision, Writing – review and editingMariam Ghafoor: Conceptualization, Supervision, Project administration, Resources, Writing – review and editing

Availability of data and materials.

The de-identified analytic dataset and analysis code are available from the corresponding author on reasonable request, subject to institutional data-sharing policy.

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Figures

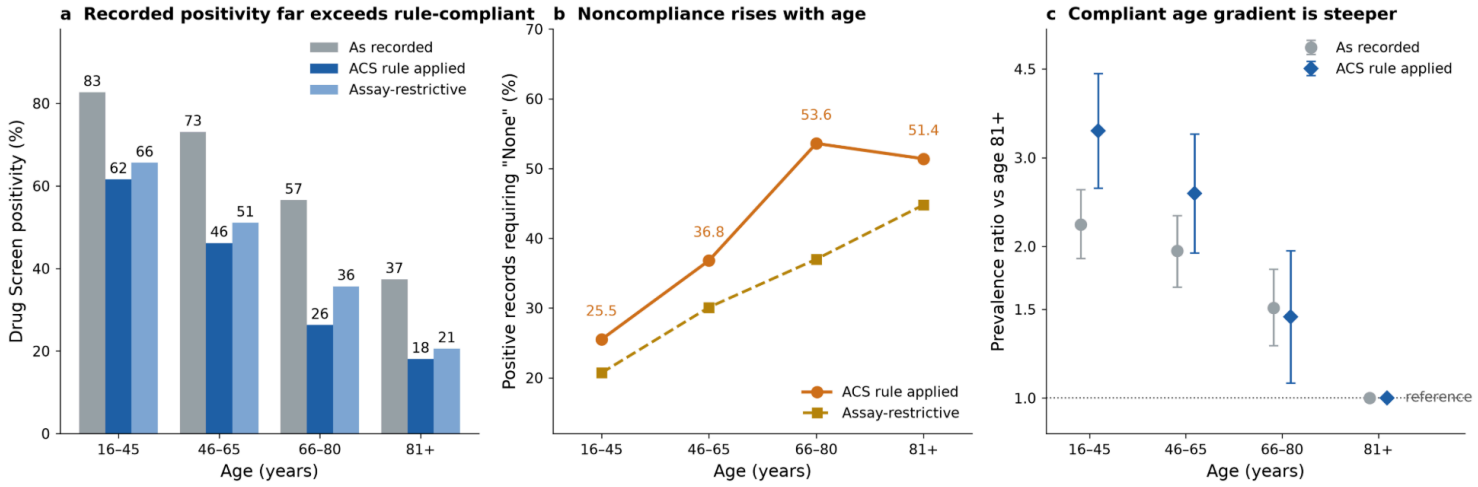


Figure 1

Compliance with the National Trauma Data Standard Drug Screen exclusion rule, by age band (n = 1,420 tested encounters aged 16 years and older). (a) Recorded positivity compared with positivity after applying the exclusion rule as written and under the assay-restrictive definition. (b) Proportion of positive records that should have been coded "None" because all reported analytes were attributable to administered drugs. (c) Prevalence ratios for each age band relative to patients 81 years and older, recorded versus rule-compliant, with 95% confidence intervals; note the logarithmic vertical scale.