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Effect of a Data-Driven Intervention on Opioid Prescribing Intensity Among Emergency Department Providers: A Randomized Controlled Trial

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ABSTRACT

Objective: Little is known about accuracy of provider self-perception of opioid prescribing. We hypothesized that an intervention asking emergency department (ED) providers to self-identify their opioid prescribing practices compared to group norms—and subsequently providing them with their actual prescribing data—would alter future prescribing compared to controls.

Methods: This was a prospective, multi-center randomized trial in which all attending physicians, residents, and advanced practice providers at four EDs were randomly assigned to either no intervention or a brief data-driven intervention during which providers were: (1) asked to self-identify and explicitly report to research staff their perceived opioid prescribing in comparison to their peers, and then (2) given their actual data with peer group norms for comparison. Our primary outcome was the change in each provider’s proportion of patients discharged with an opioid prescription at six and twelve months. Secondary outcomes were opioid prescriptions per hundred total prescriptions and normalized morphine milligram equivalents prescribed. Our primary comparison stratified intervention providers by those who underestimated their prescribing and those who did not underestimate their prescribing, both compared to controls.

Results: Among 109 total participants, 51 were randomized to the intervention, 65% of whom underestimated their opioid prescribing. Intervention participants who underestimated their baseline prescribing had larger-magnitude decreases than controls (Hodges-Lehmann difference -2.1 prescriptions per hundred patients at 6 months [95% CI -3.9 to -0.5] and -2.2 per hundred at 12 months [95% CI -4.8 to -0.01]). Intervention participants who did not underestimate their prescribing had similar changes to controls.
**Conclusions:** Self-perception of prescribing was frequently inaccurate. Providing clinicians with their actual opioid prescribing data after querying their self-perception reduced future prescribing among providers who underestimated their baseline prescribing. Our findings suggest that guideline and policy interventions should directly address the potential barrier of inaccurate provider self-awareness.

**INTRODUCTION**

Prescribing opioids in any clinical setting is complex and nuanced. The emergency department (ED) is certainly no exception, with patient and work environment factors contributing to wide variations in prescribing behavior.1-4 Accordingly, several educational and regulatory initiatives emphasize responsible prescribing.5-7 and 17 states have enacted ED-specific opioid prescribing guidelines.8 These interventions, however, have been met with varying success, and the optimal approach to influencing prescribing behavior remains unclear.9-14 Complicating matters is the fact that the “ideal” intensity of opioid prescribing from EDs is not known (if such a target even exists), nor is there evidence that benchmarking to the central tendency among a cohort of prescribers is inherently desirable. Thus, most guidelines have focused on reducing overall opioid prescribing, under the assumption that there exists some unknown but non-zero target of appropriate prescribing, with current prescribing being greater than the ideal state.

Most interventions are guideline implementations, predicated on providers inherently understanding this conundrum and being sufficiently motivated to enact the behavior changes required to reduce prescribing on the basis of pure rationality.5,11,12 Few studies have evaluated other interventions, either focusing on narrative vignettes15,16 or publicizing prescribing rates in a non-randomized fashion.17 A critical missing link in the literature is a description of how provider self-perception of opioid prescribing varies from actual prescribing practices and how this gap may affect responsiveness to interventions and subsequent prescribing changes.18

Our conceptual model is that most individual clinicians are already prescribing opioids in a manner they believe to be safe and appropriate, but in the absence of a valid external standard, providers’ views of what constitutes “normal” prescribing may vary considerably. Because most providers lack access to prescribing data for themselves and their colleagues, they have no quantitative basis upon which to evaluate or compare their prescribing or appreciate this variation. Knowing only that their own prescribing qualitatively feels reasonable, providers may be naturally inclined to believe that opioid overprescribing is a problem perpetuated by other people. Thus, attempts to reduce overall prescribing via broadcast methods (guidelines, policies, and commentaries) is unlikely to influence individual behavior change because most providers feel that they are not the problem.

Extrapolating from studies of clinician decision-making in other areas, we theorized that helping providers develop more accurate self-awareness may make them more likely to shift their opioid prescribing patterns.19,20 We hypothesized that employing an intervention to ask ED providers...
to self-identify their opioid prescribing practices—and subsequently providing them with their actual prescribing data and group comparisons—would alter their future prescribing beyond any effect achieved solely by being subject to general messaging related to the opioid crisis, including opioid prescribing guidelines. In accordance with our conceptual model, we postulated that some providers who previously may have characterized their prescribing as average may be surprised to learn that they prescribe opioids with far more intensity than their peers. Therefore, we sought to evaluate whether effects on future prescribing may be more pronounced among providers who underestimated their opioid prescribing, due to the cognitive dissonance experienced upon seeing their actual prescribing data incongruous with their self-perception.

METHODS

Study design

This was a prospective, multi-center, randomized trial in which all ED providers were randomly assigned to either the control arm (no experimental intervention) or a brief, structured, data-driven intervention during which providers were: (1) asked to self-identify and explicitly report to research staff their perceived opioid prescribing in comparison to their peers, and then (2) given their actual data with peer group norms for comparison. The study was approved by the University of Massachusetts Medical School Institutional Review Board (IRB) and prospectively registered as ClinicalTrials.gov NCT02665429.

Study Setting and Population

Four study sites included a 364-bed urban tertiary academic center with 92,000 annual ED visits, a 294-bed urban acute care hospital/non-primary teaching site with 42,000 ED visits, a 69-bed suburban community hospital with 26,000 ED visits, and a 41-bed rural community hospital with 14,000 ED visits. Board-certified/board-eligible attending emergency physicians continuously staffed all EDs, with supplementary coverage provided by advanced practice providers (APPs) with independent prescriptive authority. All were employed by a single group practice with a common management structure, and 64% worked at more than one study site. At the academic center, 80% of ED patients were seen by either emergency medicine or rotating residents. At the non-primary teaching site, residents saw fewer than 5% of patients. There were no trainees at the two community sites. At all sites, discharge prescriptions were generated via computerized provider order entry in the electronic health record (EHR), and handwritten prescribing was not permitted.

Identification of prescriptions

We queried the EHR at each site (ED PulseCheck, Optum Clinical Solutions, Inc., Eden Prairie, MN) for all medication prescriptions written for ED patients and identified the attending physician, resident, or APP who electronically signed the prescription. All providers prescribed under their own Drug Enforcement Administration (DEA) number. Prescriptions written by residents and APPs did not
require co-signature and were attributed to that prescriber, not the supervising attending, so as not to contaminate the attendings' data with variation driven by their supervisees. We identified opioids according to the DEA listings (21 C.F.R. § 1308.12-1308.14) but did not include those listed in DEA Schedule V, typically cough preparations or antidiarrheals. We cross-referenced the DEA list with the Food and Drug Administration National Drug Code database to identify opioid prescriptions written using trade names. To account for differences in formulation, potency, pill strength, and dosing frequency, we calculated the total morphine milligram equivalents (MME) for each opioid prescription using accepted equianalgesic conversion factors.

Selection of participants

Providers who wrote any pharmaceutical prescription in the prior 12 months were assessed for enrollment eligibility. We excluded providers who would no longer be prescribing in the ED following the intervention (non-emergency medicine residents who rotate for a single month or providers no longer practicing). We also excluded pediatric-only providers (due to differences in opioid prescribing in this population) and providers involved as study investigators (who were not blinded to the hypothesis).

We performed stratified permuted block-randomization using a computerized engine to allocate providers to control or intervention arms in a 1:1 ratio. We stratified the randomization by quartiles of baseline prescribing, measured by the number of opioid prescriptions written by each provider in the prior year, to evenly allocate high- and low-intensity prescribers to both study arms. This randomization stratification metric accounted for both absolute prescribing (i.e. providers who wrote a larger number of prescriptions were in higher quartiles) and relative strata of opioid prescribing intensity (i.e. providers with a larger number of patients discharged with opioids, relative to their peers, were in higher quartiles). Because our randomization scheme was conducted under a waiver of consent issued by our IRB, participants could opt-out of study participation after randomization (see Supplemental Material).

Intervention

To create the intervention instruments, we first calculated the proportion of patients discharged with an opioid prescription and the number of opioid prescriptions per hundred total prescriptions written by each provider. We then constructed graphical depictions of these distributions including data from all providers eligible for randomization, based upon prescribing data for the 12 months prior to randomization.

We timed the study to coincide with the pre-planned implementation of a state-wide opioid prescribing clinical practice guideline (CPG) because we wanted to avoid contaminating our comparisons between baseline prescribing and subsequent prescribing with a CPG implementation that occurred during the baseline period or follow-up periods. Participants in both study arms were subject to the CPG rollout together and identically between one and four weeks prior to study
intervention but after baseline data collection was complete. The CPG was implemented independently of this study, but synchronization allowed a pragmatic design in which controls and intervention participants would be equally subjected to background effects, including recency bias and priming associated with messaging related to the guidelines.

Study staff approached each provider randomized to the intervention arm and performed the brief intervention by privately showing the provider the anonymized graphical distributions, which depicted baseline opioid prescribing by all providers at their ED(s) in both absolute and relative terms (Supplement Figure 1). Scripting explained, “Each of these bars represents one provider in the group, including you. Which do you think is you?” Participants were then asked to self-identify their perceived individual position, which we recorded as the nearest decile with reference to the group. Because group distributions were fixed, each decile also corresponded to a discrete range of absolute prescribing on the y-axis of the graphic. We did not determine whether a given participant made their selection based upon relative criteria (x-axis position), absolute prescribing data (y-axis), or both.

Immediately afterward, staff provided the participant with their true profile, including absolute prescribing data and a visual display of where they fell within the peer distribution (Supplement Figure 2). Participants received the intervention only once. Controls were not surveyed as to their self-perception and did not receive individual or group data because each of these elements was central to the hypothesized mechanism of our intervention and would have been an intervention in itself that contaminated the controls. The groups were treated similarly in all other respects. For the subsequent 12 months, we passively observed prescribing patterns electronically and avoided further study-related contact to minimize the perception that participants were being observed.

Outcomes

Our primary outcome was the change in proportion of patients discharged with an opioid prescription by each provider. This outcome had face validity given its simplicity of interpretation, direct effect on community opioid burden, and prior use as a measure of opioid prescribing intensity. Secondary outcomes included change in: (1) opioid prescriptions per hundred total prescriptions written by each provider, and (2) total MME for all opioid prescriptions by a given provider, divided by the number of patients discharged (to account for variation in patients seen and better reflect each provider’s proportional contribution to opioids entering the community). Neither secondary outcome has been suggested as a key measure of opioid prescribing intensity, as far as we are aware, but we felt both to be rational alternatives that may indicate that providers were prescribing opioids more judiciously. We assessed each outcome at six and twelve months post-intervention, after a one-month washout period during the CPG rollout and intervention.

Our study focused on the data-driven intervention for providers, not on guideline implementation or adherence. Implicit in almost all ED opioid prescribing guidelines is a desire to reduce overall prescribing from EDs (with a recognition that some ED opioid prescribing is entirely appropriate), and while long-term trends suggest that population-level prescribing may be decreasing, our outcomes focused on individual prescribers to better understand mechanisms to
influence individual behavior change. In addition, the state CPG lacked objective prescribing targets or outcome measures and was poorly amenable to quantitative measures of guideline adherence. Therefore, we did not assess CPG adherence or overall guideline success *per se*, instead focusing on individual changes in prescribing attributable to the intervention.

**Statistical analysis**

The theoretical mechanism of our intervention relied upon actively querying providers as to their self-perceived baseline prescribing and then revealing their actual data, thus creating surprise or cognitive dissonance if reality diverged from their self-perception. Profile data that reinforced an accurate self-perception might be expected to have no effect. Therefore, our primary comparison stratified providers randomized to the intervention into those who underestimated their prescribing intensity and those who did not underestimate their prescribing. We compared each of these intervention subgroups to the control group. We had originally anticipated observing balance among providers who underestimated, overestimated, or were accurate in their self-perception. In reality, a small number of providers overestimated their prescribing, resulting in wide confidence intervals around this subgroup, whose prescribing changes were indistinguishable from those with accurate self-perception. Therefore, we decided *post hoc* to combine accurate and overestimating providers into a single comparator group to minimize the total number of comparisons and degrees of freedom.

Our secondary comparison was of the entire control group to the entire intervention group in an intention-to-treat analysis because we wished to assess the effects of data feedback alone, without considering the surprise or cognitive dissonance that may result from querying self-perception and identifying underestimation.

To determine accuracy of self-perception, we compared each intervention provider’s self-perceived decile to their actual decile for each intervention metric, testing for differences in the matched pairs. We considered intervention participants to have accurately estimated their prescribing for a given metric if their self-perception was within ±1 decile of their actual value. In other words, we did not require providers to have self-perception resolution to within the bounds of a single decile (often only a few percentage points wide, depending on the metric). Instead, we considered the provider to be accurate if their self-perception of their own position in the population distribution was accurate within a region encompassing 30% of their peers (3 deciles wide—one decile above and below their actual position). Given a dearth of literature in the area of quantifying accuracy of provider self-perception, we felt that this approach had face validity and created thresholds that were no more or less arbitrary than any alternative method.

We employed a variety of parametric and nonparametric techniques in testing for univariate differences between groups (see Supplemental Material). For multivariable analysis of the primary outcome and primary comparison, we used repeated-measures, mixed-effects modeling with an unstructured covariance matrix, where a random effect accounted for each individual’s prescribing trajectory over time (thus each provider was matched against him or herself over time). Fixed effects included time and intervention allocation (stratified by accuracy of self-perception—underestimation versus not), with gender, years of experience, training level, number of study sites...
worked, and interaction terms as covariates.

We assumed that variation in prescribing was attributable to individual provider factors, rather than patient factors, as all providers working at a given site would be expected to care for a similar patient population, and patients do not choose their ED provider within each site. Randomization and analyses were conducted using SAS 9.4 and JMP Pro 13 (SAS Institute Inc., Cary, NC) with alpha=0.05.

RESULTS
Characteristics of study subjects and baseline prescribing

We identified 290 providers who wrote at least one prescription in the 12 months prior to randomization; 109 were eligible for randomization (Figure 1). During those 12 months, eligible participants collectively discharged 119,428 patients and wrote 75,203 total prescriptions, 15,124 (20.1%) of which were opioid prescriptions. Table 1 reports baseline characteristics of participants and their baseline prescribing. There were no meaningful imbalances between groups at baseline. Years since professional school graduation ranged from 3 to 34 years (median 13, interquartile range [IQR] 14) for non-residents.

The proportion of patients discharged with an opioid prescription by each provider was stable in the six months immediately preceding randomization, compared to the prior six months (mean difference -0.4% [95% CI -1.2% to 0.4%]), with no evidence of a temporal trend prior to the study. Prescribing varied among sites, with 7.8% of patients discharged with an opioid prescription at the academic ED, 16.7% at the rural community ED, 17.8% at the urban non-primary teaching site, and 22.2% at the suburban community ED. Among the 70 participants who worked at more than one site, there were no significant between-site differences in quintile of individual prescribing, compared to their peers at each site. There were no differences in baseline prescribing among providers later lost to follow-up, compared to those analyzed at six and twelve months. Among the 4 participants who opted-out after randomization, percentage of patients discharged with an opioid prescription were 15.2%, 9.1%, 2.4%, and 6.4%, and opioid prescriptions per hundred total prescriptions written were 23.4, 66.7, 17.9, and 23.1, respectively.

Provider self-perception

Among all intervention participants, 73% of attending physicians and APPs and 27% of residents underestimated their prescribing rank among their peers by more than one decile in at least one metric. Only 5 providers (3 residents and 2 attendings) consistently overestimated their prescribing rank compared to their peers. As a group, attendings and APPs underestimated their position within the distribution of proportion of patients discharged with an opioid prescription by a median of two deciles (95% CI -2.3 to -0.9), while residents (as a group) did not exhibit a significant
difference. There were no significant differences by gender or study sites worked.

Providers underestimated their position within the distribution of opioid prescriptions per hundred total prescriptions written by a mean of one decile (95% CI -1.9 to -0.1), with no differences by training level, gender, or number of sites.

Overall changes in prescribing

Among all providers, and considering each provider’s proportion of patients discharged with an opioid prescription at 6 or 12 months compared to their baseline value as matched pairs, the mean decrease exhibited by individual providers was 3.5 opioid prescriptions per hundred patients (95% CI 2.7 to 4.3) at 6 months (a 30% decrease [95% CI 24 to 37]) and 4.3 (95% CI 3.2 to 5.4) at 12 months (a 40% decrease [95% CI 34 to 47]) versus baseline.

Table 2 reports cross-sectional aggregate provider prescribing for the entire study population at baseline, six months, and twelve months. Differences at six and twelve months, compared to baseline, were significant (p<0.001) for all metrics.

Main Results

On average, participants randomized to the intervention who underestimated their opioid prescribing compared to their peers had a larger decrease in proportion of patients discharged with an opioid prescription at both 6 and 12 months than either controls or providers who did not underestimate their prescribing (Figure 2a). Hodges-Lehmann estimates of the difference between groups were 2.1 (95% CI 0.5 to 3.9, p=0.007) fewer opioid prescriptions per hundred patients at 6 months among intervention providers who underestimated their prescribing versus controls and 2.2 (95% CI 0.01 to 4.8, p=0.05) fewer prescriptions per hundred at 12 months. Corresponding values for the intervention providers who did not underestimate their prescribing, compared to controls, were 1.3 more prescriptions per hundred patients at 6 months (0.4 decrease to 2.9 increase, p=0.15) and 1.2 more prescriptions per hundred (95% CI 1.3 decrease to 3.5 increase, p=0.44) at 12 months. Hodges-Lehmann estimates are simultaneous group-to-group comparisons, while figure 2 depicts mean changes over time among all individuals in each group using repeated-measures methods, so Hodges-Lehmann estimates should not be expected to correspond exactly to the distance between lines on the figures.

In the multivariable mixed-effects model, intervention allocation with underestimation of one’s prescribing relative to peers, physician level of training (compared to APP), and fewer years of experience were significant predictors of larger-magnitude six and twelve-month decreases in the proportion of patients discharged with an opioid prescription. Marginal contributions of each variable are given in Supplement Table 1.

Secondary Results
Figure 2b depicts that participants randomized to the intervention who underestimated their prescribing compared to their peers also had larger decreases in opioid prescriptions as a percentage of total prescriptions written at 6 months, compared to both controls and providers who did not underestimate their prescribing. There was no difference at 12 months. Figure 2c illustrates the differences in MME per total patients discharged. Intervention participants who underestimated their prescribing had larger MME decreases at 6 and 12 months than either controls or providers who did not underestimate their prescribing.

When considering the secondary comparison of all providers randomized to the intervention compared to controls in intention-to-treat analysis, there was not a significant difference between groups in the proportion of patients discharged with an opioid prescription at 6 or 12 months. Hodges-Lehmann estimates were 0.7 fewer prescriptions per hundred in the intervention group at 6 months (95% CI 2.0 decrease to 0.8 increase, p=0.34) and 0.8 fewer prescriptions per hundred in the intervention group at 12 months (95% CI 2.7 decrease to 0.9 increase, p=0.39).

**DISCUSSION**

The intervention of prompting providers to explicitly report their perceived baseline opioid prescribing practices, followed immediately by revealing to them their actual data compared to peer norms (query-reveal method), is designed to address what we see as a shortcoming of traditional models of influencing provider behavior change. If providers already perceive their prescribing to be appropriate and similar to their peers, they may be less likely to internalize educational and policy messaging and may perceive less need for individual practice change, even in the face of standardized guidelines. In this study, providers randomized to a brief query-reveal intervention who underestimated their own prescribing and were then shown their actual data the largest-magnitude prescribing decreases.

Background prescribing changes over time among all providers may be attributable to many factors, including guideline implementation, public and institutional scrutiny of prescribing on the backdrop of the national opioid epidemic, and individual introspection. Our randomized approach suggests that, despite these factors, the query-reveal intervention appears to create a differential effect among providers who underestimated their prescribing. Central to our conceptual model was the active, structured query of providers’ self-perceptions. If seeing their own data confirmed a provider’s pre-existing perception of acceptable baseline practice, their future prescribing changes were likely similar to controls—influenced more by macro-factors, such as guidelines and policy messaging. We believe that this explains our finding that there was no difference in future prescribing among providers with accurate self-perception or who overestimated their prescribing, compared to controls. We caution, however, that providers who did not underestimate their prescribing also had a trend toward slightly less intense baseline prescribing practices, leaving less opportunity to underestimate one’s relative prescribing and less opportunity for future prescribing decreases.

For most providers, however, we identified striking gaps in prescribing self-awareness, reflecting the likelihood that most providers lack a quantitative basis upon which to contextualize requested behavior change. When presented with a graphical depiction of the spectrum of opioid

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prescribing among their peers, most providers felt they were average, not at the extremes. Similar to
the cognitive bias of illusory superiority (for example, the majority of drivers think that they are
above average—a statistical impossibility), it is likely that most ED providers view their opioid
prescribing as similar to or less than their peers, especially when asked to explicitly communicate
their self-perception to another person.

Broadcast messaging, such as guideline implementation, is a common practice at both the
local and state/national level to attempt to influence provider behavior change. However,
prior reported CPG implementations have assumed that guideline adherence by providers is a
rational decision. Rationality alone may not drive behavior change, especially among individuals
with decreased self-awareness because self-awareness is a necessary prerequisite to intentional
change. Thus, an intervention to identify and unmask inaccurate self-perception—and correct that
perception using a provider’s actual data—appears to have enabled more robust behavior change
for a subset of providers who may have otherwise had difficulty internalizing the need to change.

When providers underestimated their prescribing, we postulate that seeing their data
discordant from their pre-existing notions caused them to experience cognitive dissonance (the
uncomfortable psychological state created when an individual holds two or more contradictory
beliefs at the same time), in turn creating a priming for behavioral change. Within the cognitive
sciences, the act of asking questions about a behavior is known to impact subsequent performance
of that behavior, possibly by exposing dissonance and preventing future rationalization that
justifies regression to past behavioral patterns.

The effects of cognitive dissonance on behavior change are described in other settings—
even beyond healthcare. It stands to reason, then, our hypothesized mechanism and
conceptual model are rooted more in general human cognition than in any specific clinical
environment. Thus, we anticipate that our findings may be generalizable beyond the ED to other
healthcare settings in which practice variation exists.

This is pertinent given that between 90 and 95% of all US opioid prescriptions are thought to
be generated from outside of EDs. Nevertheless, reducing acute care prescribing has received
substantial focus, in part due to the observation that initial opioid prescribing for acute pain has
numerous important downstream consequences for patients and society. We agree with other
authors who believe that collaborative, multidisciplinary, cross-societal dialogue about safe
prescribing and appropriate pain management is the ideal path forward and most able to mitigate
unintended consequences. We also accept that the ongoing parallel efforts to reduce
acute care prescribing are also probably helpful or are at least likely to continue, despite the open
question as to what constitutes “ideal” prescribing. In both pursuits, we feel that educational
programs and policy interventions should directly address the potential barrier of inaccurate
provider self-awareness, rather than simply dictating guidelines or policy and expecting provider
adherence.

We believe that this is the first study to prospectively validate an individualized intervention
for providers to influence opioid prescribing in a randomized manner. We are aware of only one
prior study that provided clinicians with quantitative opioid prescribing data. That study employed
a non-randomized methodology and created an environment where providers knew that their
prescribing would be tracked and their unblinded data made visible to their peers. Such an approach
precluded inference into specific effects of data reporting and did not assess self-perception or create dissonance, and observed prescribing decreases may have been driven primarily by a “big brother effect” because providers knew they were being watched.

LIMITATIONS

Limitations of this study included a lack of direct patient outcomes, such as undertreated pain or opioid-related complications. Similarly, we had little insight into the indications for each opioid prescription or if non-opioid alternatives were sought. We made no attempt to assess the appropriateness of any individual opioid prescription, instead focusing on individual providers’ aggregate prescribing decreases over time, in accordance with the goal of most ED prescribing guidelines. As discussed in the introduction, the ideal intensity of opioid prescribing is not known, and benchmarking to the central tendency among a cohort of prescribers is not necessarily desirable. Our study occurred on the backdrop of intense public scrutiny and media attention to the opioid crisis, culminating in passage of a state law increasing opioid regulations during the eighth month of follow-up, which may have introduced unmeasured confounders, although the effects would likely be balanced across study arms.

While the four study sites had diverse practice environments, patient populations, and local operational leadership, they were part of a single health system and shared senior leadership, which may limit generalizability. Nevertheless, the four sites do appear to represent a broad cross-section of patient populations and practice environments, as evidenced by the differences in aggregate prescribing among sites, despite little difference in individual prescribing among sites (by providers who worked at more than one site). By design, providers served as their own controls over time to mitigate limitations of prior studies, wherein providers who work in specific areas of the ED (e.g., “fast-track” or observation units) or particular times (e.g., overnight hours) may have introduced systematic differences in prescribing. This likely explains our observed differences in post-intervention prescribing by APPs, who work disproportionately more shifts in our observation unit, where they tend to discharge a higher proportion of patients with acute pain.

Local institutional practices did not require attending physician co-signature on opioid prescriptions written by residents, so prescriptions were attributed to the provider who signed the prescription. While supervisee prescriptions may have been contaminated by attending preference, all residents would have been expected to have approximately equal exposure to each attending physician over the course of the study period. Some attending opioid prescribing preferences, however, may have been masked by asking residents to write prescriptions, resulting in attribution to the resident. We plan to evaluate variation in supervised prescribing in future work.

Because the intervention instrument showed both peer-comparison on the x-axis and absolute prescribing on the y-axis, we could not determine whether a given participant’s self-perception selection was influenced more by how they thought they compared to their peers in relative terms or how they believed they prescribed opioids in absolute terms of the metric in question. Thus, it remains unclear whether underestimating one’s prescribing in the study reflects inaccurate self-perception about peer norms, inaccurate self-perception about absolute prescribing practices, or both.
Unfortunately, the prevalence of inaccurate self-perception among our control group is unknown and unknowable because, if our hypothesized mechanism is correct, the mere act of querying those providers as to their self-perception would be an intervention in itself.

While likely small in effect magnitude, there is a possibility of cross-contamination between the control and intervention groups if intervention participants shared their data profiles or experiences with control providers, but the profiles were deidentified (except for the single intervention provider who owned that specific profile). Therefore, control participants who saw intervention profiles would have no way to determine their own data or experience the self-perception intervention or associated cognitive dissonance.

Because our conceptual model was predicated on prompting providers to reflect upon their prescribing, we feel that the primary comparison based upon strata of self-perception accuracy is rational because it better tests the mechanistic underpinnings of our intervention. However, an intention-to-treat analysis, in which all providers randomized to the intervention were compared to controls, was included as a secondary comparison and would be well-suited to a larger follow-up study. Given our constrained subject population (there are a finite number of providers who worked at the study sites), a prospective sample size and power calculation would not have changed our approach for this study, but future studies may benefit from a more formal power analysis and comparison of intention-to-treat and stratified analyses.

Our study was also not powered to assess discordant hierarchical pairs (for example, comparing attendings who received the intervention with APPs or residents who did not). Finally, greater loss to follow-up occurred in the control group; however, our data do not suggest that the loss was unbalanced.

CONCLUSIONS

This randomized trial exposes important gaps in providers’ self-perceptions of opioid prescribing and demonstrates that a simple, data-driven intervention using query-reveal methodology may decrease future prescribing, particularly among providers who underestimate their own prescribing practices. Our findings imply that current and future interventions in which provider adherence to guidelines is expected, including opioid interventions outside the ED and other quality and safety initiatives, should directly address the potential barrier of inaccurate provider self-awareness.

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Table 1. Characteristics of study subjects and their baseline prescribing for the 12 months prior to intervention

<table>
<thead>
<tr>
<th></th>
<th>Control (n=58)</th>
<th>Intervention (n=51)</th>
<th>All Subjects (n=109)</th>
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<td>Level of training, No. (%)</td>
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<td>1124 (681)</td>
</tr>
<tr>
<td>Count of prescriptions written, median (IQR)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Opioids</td>
<td>110 (147)</td>
<td>89 (128)</td>
<td>98 (131)</td>
</tr>
<tr>
<td>Total</td>
<td>503 (594)</td>
<td>464 (610)</td>
<td>492 (564)</td>
</tr>
<tr>
<td>Percentage of patients discharged with an opioid prescription, median (IQR)</td>
<td>10.7 (6.4)</td>
<td>10.2 (6.9)</td>
<td>10.5 (6.9)</td>
</tr>
<tr>
<td>Opioid prescriptions per hundred total prescriptions written, median (IQR)</td>
<td>21.1 (7.9)</td>
<td>20.6 (10.0)</td>
<td>21.0 (8.5)</td>
</tr>
</tbody>
</table>

Table 1 footnotes
IQR=interquartile range, SD=standard deviation, MME=morphine milligram equivalents.
Table 2. Cross-sectional aggregate opioid prescribing over time among all subjects

<table>
<thead>
<tr>
<th></th>
<th>Baseline 12 Months Pre-Intervention</th>
<th>0-6 Months Post-Intervention</th>
<th>7-12 Months Post-Intervention</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Median (IQR) [Range]</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Percentage of patients discharged with an opioid prescription ( ^a )</strong></td>
<td>10.5 (6.9) [0 to 37.9]</td>
<td>6.4 (4.4) [0 to 53.1]</td>
<td>5.6 (4.8) [0.5 to 20.4]</td>
</tr>
<tr>
<td><strong>Opioid prescriptions per hundred total prescriptions written ( ^a )</strong></td>
<td>21.0 (8.5) [0 to 66.7]</td>
<td>14.7 (8.9) [0 to 39.7]</td>
<td>11.7 (8.0) [1.4 to 50.0]</td>
</tr>
<tr>
<td><strong>Total prescribed MME per patient discharged ( ^a, b )</strong></td>
<td>11.5 (9.5) [1.5 to 41.4]</td>
<td>6.1 (5.5) [0.7 to 39.4]</td>
<td>5.1 (4.6) [0.6 to 41.6]</td>
</tr>
<tr>
<td><strong>Median MME per opioid prescription ( ^a, c )</strong></td>
<td>100 (60) [50 to 225]</td>
<td>90 (38) [45 to 225]</td>
<td>90 (38) [50 to 150]</td>
</tr>
</tbody>
</table>

Table 2 footnotes

- IQR=interquartile range, MME=morphine milligram equivalents
- \( ^a \) p<0.001 for difference from baseline at both 6 and 12 months
- \( ^b \) The maximum observed MME in a single prescription in the baseline period was 2700 mg, and 6.3% of opioid prescriptions exceeded 1000 MME. Values were 1200 mg and 1.7% at 6 months, and 1800 mg and 0.2% at 12 months, respectively.
- \( ^c \) Six providers (5.5%) collectively cared for 9.4% of the total patients but accounted for 20% of total opioid MME entering the community.
FIGURES

Figure 1. Participant randomization and CONSORT study flow diagram

Assessed for eligibility
(Wrote at least one prescription in the prior year)
(n=290)

Not eligible for randomization (n=181)
- Rotating non-EM resident (n=155)
- No longer practicing (n=10)
- Saw only pediatric patients (n=8)
- Graduated EM resident (n=5)
- Study investigator (n=3)

Randomized
(n=109)

Allocated to intervention (n=51)
- Completed intervention (n=47)
  - Left the practice after randomization (n=2)
  - Opted-out/declined to consent (n=2)
- Leave of absence (n=1)

Allocated to control (n=58)
- Opted-out/declined to consent (n=2)
- Left the practice before 6 months (n=4)
- Leave of absence (n=1)

Analyzed at 6-month follow-up (n=46)
- Left the practice before 12 months (n=2)
- Graduating EM resident (n=1)
- Leave of absence (n=1)

Analyzed at 12-month follow-up (n=42)
- Left the practice before 12 months (n=8)
- Graduating EM resident (n=3)
- Returned from leave of absence (n=+1)

Analyzed at 6-month follow-up (n=51)

Analyzed at 12-month follow-up (n=41)

Figure 1 caption
EM=Emergency Medicine
Figure 2 panel a. Repeated-measures mean change in proportion of patients discharged with an opioid prescription by intervention subgroup

Figure 2 panel b. Repeated-measures mean change in opioid prescriptions per hundred total prescriptions written by intervention subgroup
**Figure 2 panel c.** Repeated-measures mean change in MME per total patients discharged by intervention subgroup
Each line represents one subgroup of providers in the primary comparison, either controls, intervention providers who underestimated their own prescribing with reference to their peers, or intervention providers who did not underestimate their prescribing (see text for expanded explanation). For panels 2a and 2b, underestimation refers to the provider’s self-perception of the same metric being depicted. As providers were not shown their MME data during the intervention, self-perception strata in panel 2c refer to accuracy of self-perception for proportion of patients discharged with an opioid prescription (the primary outcome). The figure depicts the mean change for each metric over time within each comparison subgroup, considering each provider’s 6- and 12-month prescribing matched against his or her own baseline prescribing in a repeated-measures design. MME=morphine milligram equivalents. Error bars represent standard error around the observed mean change.

Notes to editor: Figure 2 is intended as a 3-panel figure with a common caption.