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**Title:** Variation in opioid prescribing after outpatient breast surgery: time for a streamlined approach?

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**Reviewer 1:** Dr. I. Kudrina

**Institution:** Faculty of Medicine, Family Medicine, McGill University

General comments (author response in bold)

Thank you for the opportunity to review this manuscript titled “Variation in Opioid Prescribing after Outpatient Breast Surgery: Time for a Streamlined Approach?” This study contributes important data on opioid prescribing in post-surgical period. I have several comments, questions and suggestions that I feel would be important to address.

**Thank you for your comment.** (No changes made.)

Key words. “opioid”, “prescribing” could be added

**Thank you for your comment. This change has been made.** (See changes to Keywords)

Please, explain 7-day prescribing benchmark. Why 7 days post-surgery were chosen? How long is the hospital stay? Overall, it remains unclear at what point these patients were discharged from the hospital setting and filled their outpatient Rx, especially if they filled more than one. Please, clarify.

**Thank you for your comment. Seven days was selected, as from a clinical standpoint, this is the period of time where patients experience acute post-operative pain. A similar timeframe has also been used by other investigators who have assessed post-operative pain in outpatient breast surgery patients (References: Hartford et al PMID: 31342371, Hartford et al. PMID: 30359828, Mitchell et al. 2012 PMID: 22713999).**

**The patients in the cohort underwent outpatient breast surgery and therefore there was no hospital stay associated with this.** (We have made changes to the manuscript Methods, Outcomes to justify our selection of the seven day timeframe.)

Please, divide 2ry outcome into two separate outcomes. Please report and discuss results of these outcomes separately (outcome 1, outcome 2 etc). How many Rx were filled for outcome #2?

**Thank you for your comment. We have made changes to better show that there were two separate secondary outcomes.** (We have made changes to manuscript Methods, Outcomes to better detail the two separate secondary outcomes. The number of prescriptions filled is in Table 1 and also outlined in the text.)

Page 2. Line 21-24. I did not understand why these numbers should warrant any change in prescribing.

**Thank you for your comment. We believe you are referring to the last line of the Abstract. We have made some changes to better reflect the message of this work.** (Please see the Abstract which now reads as follows: “*Most patients undergoing outpatient breast surgery fill an opioid prescription within seven days. Dosing varied by surgical procedure and patient factors. Further work is required to determine patient groups where opioids can be successfully minimized or eliminated.*”)

What does it mean "streamlined" (dose, time, prescriber)? Please, explain.

**Thank you for your comment. We appreciate the ambiguity associated with this terminology. We have removed this word entirely.** (The word "streamlined" has been removed from all aspects of the manuscript, including the title. The title now reads: "Variation in Opioid Prescribing after Same Day Breast Surgery in Ontario, Canada: A Population-Based Cohort Study")

Page 4. Line 3-5. What "lower prescribing" stands for? (lower rates, lower dose per prescription / per patient / per distribution, fewer prescriptions per person / per period).

**Thank you for your comment. We agree that this is ambiguous and more specific terminology can be used. We have changed this to "lower opioid doses can be safely prescribed"** (Please see manuscript Introduction.)

Please, specify Page 5. Line 41. Were there any missing data after linking databases? Did you perform any missing data imputation? Please, clarify.

**Thank you for your comment. Yes, a small amount of data are missing on hospital type and location of residence. We have added additional information in the manuscript to describe the missing data further. No patients were excluded based on missing data. See comments #13, #18, #20.** (We have made changes to manuscript Methods, Statistical Analyses and Ethics Approval and have also included a reference to support inclusion of patients with missing data.)

Page 7. Line 22. ? "higher filling". Did you mean a higher total daily dose? Please, clarify

**Thank you for your comment. Higher filling means higher OMEs filled by the patient, which we defined at  $\geq 75$ th percentile. We have made some changes to the sentence to make this clearer.** (See manuscript Methods, Statistical Analyses and Ethics Approval. This has been changed to "higher OME filled".)

Line 40. Suggest rephrasing "Only 4% of the study cohort filled more than one prescription within seven days of surgery (2% filled one, 0.2% filled two, and 0.1% filled three and more additional prescriptions." These numbers, however, do not come to the total of 100% - were there missing data?

**Thank you for your comment. We have made this change. Thank you for picking up on the differences in numbers. This has been corrected.** (See manuscript Results.)

Page 8 and further down. These results must be summarized. Please, do not repeat the data from the Table/s.

**Thank you for your comment. We have made changes to better summarize the data and eliminate extra words.** (See manuscript Results.)

Page 9. Line 10. Did you analyse socioeconomic status? Opioid dose pre-admission? Other medications?

**Thank you for your comment. We did incorporate socioeconomic status into the models. We did not examine opioid dose prior to surgery but our sensitivity analyses show no differences in the study results upon exclusion of those with opioid use disorder based on ICD-10 codes and filling of buprenorphine and methadone.** (See manuscript Tables 3, 4 and Appendix Table 4 for socioeconomic status, which has been incorporated into the models. See additional information on sensitivity analyses in the Methods.)

CCI is not enough. Most patients in this cohort have CCI 0-1, which is unlikely given different socioeconomic status, age and number of patients in this cohort. Distribution should be close to normal for most variables. If these other data are missing, this is a significant limitation. Please, explain.

**Thank you for your comment. This information is provided for your perusal. The Charlson Comorbidity Index (CCI) used is based on the Deyo adaptation of the CCI using administrative data.<sup>1</sup> CCI and its modified versions have been validated as morbidity and mortality measures in patient populations with various diseases or undergoing various surgical procedures.<sup>1-10</sup> C-statistics for the Deyo CCI's ability to predict mortality ranged from 0.64 to 0.86 for in-hospital mortality and 0.59 to 0.85 for 1-year mortality, depending on the study population.<sup>5-10</sup> In our cohort, CCI was determined based on hospitalizations in the five years prior to breast surgery as using more data has been shown to be more predictive of morbidity and mortality. As our cohort is not elderly, we would expect infrequent hospitalizations and thus the comorbidity measures to be generally lower as observed in other cohorts of similar age.<sup>11-13</sup> Those patients with CCI 2+ would be the most comorbid. Data are not missing as hospitalizations are mandatory submissions from hospitals to CIHI.**

1. Deyo RA et al. J Clin Epidemiol 1992 45(6):613-9
2. Quan H et al. Am J Epidemiol 2011;173(6):676-82
3. D'Hoore W et al. J Clin Epidemiol 1996; 49(12):1429-33.
4. Schneeweiss S et al. Health Serv Res 2003; 38(4):1103-1120
5. Melfi C et al. J Clin Epidemiol 1995; 48:917-26.
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7. Nuttall M et al. J Clin Epidemiol 2006; 59:265-73.
8. Hollenbeak CS et al. Arch Otolaryngol Head Neck Surg 2007; 133(1):24-7.
9. Sessler DI et al. Anesthesiology 2010; 113:1026-37.
10. Liu J et al. Kidney Int 2010;77(2):141e51.
11. Evans CD et al. J Trauma Acute Care Surg 2022. doi: 10.1097/TA.0000000000003583. Online ahead of print
12. Brogly SB et al. Epidemiology. 2021 May 1;32(3):448-456.
13. Patel SV et al. Can J Surg 2018; 61(6) : 412-7. (No changes made.)

Line 43. What is "higher prescription filling"? Please, re-word  
**Thank you for your comment.** (This text has been eliminated.)

Page 10. Line 33. What is "decreased opioid filling"?  
**Thank you for your comment.** (This has been changed to "decreased OME filled".)

Page 10. Line 38. "opioid analgesia can be successfully eliminated in post-surgical period" - Please, rephrase (optimized, limited?..)  
**Thank you for your comment. In the studies that support the statement, opioid analgesia was successfully eliminated. We agree with the sentiment that it cannot always be eliminated, but in these particular studies it was.** (No changes made.)

Page 11. Line 8. Same as above. Eliminating opioids should be a goal. I would say, using them judiciously.

**Thank you for your comment. We do feel that opioid analgesia can be successfully eliminated in certain patient groups. We feel that in appropriately**

**selected patients it should be the goal. Therefore, we have kept this statement the same.** (No changes made.)

Line 12. "opioid filled" - missing a word?

**Thank you for your comment.** (Changed to "OME filled")

Line 15-17. Given prescribed doses, this is rather surprising that one prescription lasted only few days. Doses of 50 MOE and higher are very unlikely to be initiated post-surgery. Therefore, this is equally unlikely that surgeons will taper them. So, you are essentially analyzing pre-surgical opioid prescribing practices and discharge prescriptions. Therefore, stratifying results based on the total opioid daily dose (0-50, 50-100, 100-200, > 200) is crucial. Consequently, the results must be stratified based on the presurgical patient characteristics (opioid naive vs. occasional user vs. regular user) and pre-surgical vs postsurgical total daily dose. Otherwise, this will look like your young healthy opioid-naive patients were prescribed very high opioid doses and almost immediately discharged.

**Thank you for your comment. We do not know how long one prescription lasted as we have no accurate way of knowing whether the patient took it as prescribed (we only know that the prescription was filled). As for patients with pre-existing opioid use disorder, this is an important point and we have examined this in the sensitivity analysis with no change in our study results.** (See Methods, Statistical Analyses)

Page 12. Line 4. This could also be related to low education, higher distress and lower support, insufficient local resources available to these patients etc. Please, elaborate.

**Thank you for your comment. We agree that these factors may contribute to higher prescribing in community hospitals. We have included an additional reference and have acknowledged the insufficient local resources as a contributing factor. Further elaboration of factors is limited by word count constraints.** (See manuscript Interpretation.)

Line 36-38. Do you mean these amounts were prescribed for procedures or upon discharge? Were they the same as upon admission, increased or decreased? 200-400mg are very high doses and were likely prescribed long before the admission. Please, clarify.

**Thank you for your comment. We are now reporting all results as median and IQR, based on reviewer comments. The primary outcome was filling an opioid prescription within seven days of outpatient surgery, so represents what was filled after surgery. These numbers are not being compared to admission values as these patients were never admitted to hospital.** (Please see Results.)

Line 47-54. "Streamlined" is unclear. Please, explain your idea. Did you find that some specific scenarios could be qualified as "over-prescribing"? If yes, how? You mention that opioid prescribing could be "appropriate", are there findings that help you identify this? What specific findings bothered you? Why?

**Thank you for your comment. Agreed that "streamlined" is ambiguous. This has been totally removed from the manuscript. Our manuscript reports the data in terms of what prescriptions are being filled and how much. We are not suggesting that there is over-prescribing based on this data. We have modified the Interpretation to better highlight this message.** (Please see changes to the Interpretation, final paragraph.)

Line 55. Describe the most "affected group", please. Who were these 105 patients that filled more than 3(!) prescriptions in 7 days?

**Thank you for your comment. We have looked at these 105 patients in greater detail and have not found any significant differences in terms of patient and disease variables that may explain the need for more than 1 prescription. The supplementary table summarizing the characteristics of these 105 patients is appended below for your reference.** (Please refer to Supplementary Table 1 at the end of this document.)

In general, the Interpretation section could be significantly shorter and the scope of discussion expanded. A more focused conclusion is required: What group is at a higher risk of over-prescribing, who is at a lower / no risk - What do you recommend focusing on in the future studies? What correlates and discrepancies do you see from your data?

**Thank you for your comment. We have modified our conclusion to better portray the main messages of this work and opportunities for future study.** (See changes to Interpretation, final paragraph.)

Page 13. Line 6. How much in mg is your 75th %-ile? Your numbers are quite high. What are the dose ranges for main opioids? What conversion approach into morphine equivalents did you use (reference, please)?

**Thank you for your comment. Please see revised Table 2 which shows the minimum and maximum values of OMEs. For the conversion approach, please see response to comment #19.** (Please see Table 2 and Methods)

Line 20. Opioid-naive patients?

**Thank you for your comment. Yes, these studies were in opioid naïve patients.**

Line 23-24. Initially prescribed by who? Re-phrase, please

**Thank you for your comment. These were initially prescribed by the same group of surgeons and residents.** (Please see changes in the sentence to improve clarity.)

Line 36. Missing discussion of bias. Please, add

**Thank you for your comment. We have now elaborated upon various forms of bias which may affect our study results.** (Please see Interpretation.)

Line 40 and in the main text. It is unclear how to interpret "one prescription". More data are needed, if possible:

- Prescription were signed for how many days on average?
- Initial and additional Rx - were they issued by the same MDs?
- Did the number of Rx filled in 7 days correlate with the arranged outpatient follow-up (oncology, GP etc), distance from the hospital (older and sicker patients would be given longer Rx to minimize travel)?

**Thank you for your comment. We cannot comment on signing of prescriptions. When > 1 provider provided a prescription, we randomly selected one for inclusion in the model which accounted for provider-level clustering (random intercept model). We have no information on outpatient follow up and therefore cannot correlate this.** (See changes to Methods and Results which describe the random intercept model.)

Page 14. Line 4-5. Please, cite any systematic literature on the known risks of prolonged opioid use postsurgery.

**Thank you for your comment. We appreciate the suggestion, but due to word constraints, have elected not to include this information. Some of the issues associated with opioid use are summarized in the Introduction (long-term use, misuse, diversion).** (No changes made.)

Line 22. "to manage post-operative pain" or to continue medication that was present on admission. The biggest discrepancy from this manuscript is the dose post-surgery that ranges in hundreds of mg in the mere 7 days and mostly healthy population with low CCI. How do the authors explain this?

**Thank you for your comment. We agree that some of the doses prescribed seem very high, almost unbelievable at times. While this may be explained, in part, by those with opioid use disorders, our sensitivity analyses showed that excluding these patients did not significantly affect the results. Some of the higher doses could also be due to the fact that patients were prescribed opioids over a longer period of time (i.e. a 30 day supply) which would make the total OME high. We did not examine OME by day as we feel the total OME prescribed is a more accurate measure of opioid exposure. In general, those who are prescribed a supply for 30 days will have higher OME than those who are prescribed a 3 day supply.** (Noted in limitations within the Interpretation.)

In conclusion, please discuss what your study contributes to the existing literature.

**Thank you for your comment. We have tried to better portray this in the last paragraph of the Interpretation.** (See last paragraph of Interpretation.)

Table 1. Column 2. N (%) is not clear what it stand for. What is (%) in columns 3,4 and 5 are for? Please, clarify

**Thank you for your comment. Agree that this is confusing. We have made the changes in Table 1 to improve clarity.** (See Table 1.)

Table 2. Please, discuss this and other similar findings (SD being much larger than the mean value).

**Thank you for your comment. The standard deviations are indeed correct and reflects wide variation in opioid prescribing (a key message of this study). As per other reviewer comments, we have removed means and standard deviations and now show the data using median, interquartile range and minimum and maximum values to better illustrate the variation in prescribing.** (See changes throughout manuscript.)

I wonder if you have high variation between values and abnormal distribution. If that's the case, please, use medians and ranges instead of the mean and SD.

**Thank you for your comment. Please see response to #73.** (See changes throughout manuscript.)

I am surprised that hospital type does not seem to correlate with the postal code (SES quintiles) findings. Wouldn't small hospitals be located in more rural / low SES regions? How do you explain this finding?

**Thank you for your comment. SES quintile is based on the person's postal code of residence, but it is not meant to be a geographic reflection. Rather, it is meant to**

**reflect a patient's SES, which has far greater potential to impact health than the residential place does.**

**Furthermore, SES quintile is based on the patient's postal code of residence which may not actually be the same postal code as where the hospital is located. Furthermore, SES quintile can be quite variable from postal code to postal code, even among adjacent postal codes, including adjacent hospitals. While there could be some correlation, the lack of accord between these two measures is not surprising to us. (No changes made.)**

After the Table with opioids. There is another table without a title. Is this the same table as above? Please, check and remove if inserted accidentally.

**Thank you for your comment. All the tables have titles. (No changes made.)**

**Reviewer 2:** Allison Maciver

**Institution:** Surgery, Schulich School of Medicine & Dentistry, Western University  
General comments (author response in bold)

The authors have tackled an extremely important public health issue as it intersects with ambulatory breast surgery, using a large provincial dataset. I think this work has great value as it adds to our understanding of the current "patchwork quilt" of postoperative pain management and prescribing practices across Ontario, and the findings are likely generalizable to the rest of the country. The use of narcotics for short-term, acute postoperative pain has rightfully been studied and scrutinized lately as we acknowledge the part that we as surgeons play in the opioid crisis; as examples, we know surgery can be an inciting event for chronic pain and opioid misuse in some patients, and overprescribing adds to diversion of unused medication. In the interest of public health, in order to take necessary next steps of reduction of reliance on opioids for acute postoperative pain relief, we need to understand which patients are receiving higher amounts of OMEs and which prescribers are providing the scripts and theorize why. I congratulate the authors on a thoughtful analysis of their findings in this regard. It is overall a concise and well written paper.

**Thank you for your comment. (No changes made.)**

The choice of primary and secondary outcomes is sound – filling an opioid Rx within 7 d of surgery, and total OME filled, and multiple Rxs needed within 7 d. Exclusion criteria are reasonable. I have some questions regarding detail in the Methodology:

**Thank you for your comment. (No changes made.)**

Subsequent breast surgery had to be at least 6 months after the index surgery to be considered as a second surgery. Can you provide your rationale for this? Given that lumpectomy for malignancy carries around a 15-20% positive margin rate, most often requiring re-excision as standard of care, I find it interesting that of 109,352 patients, only 6,586 were excluded due to having multiple breast surgeries in a 6-month period. Further to this, is "n" in Figure 1 then referring to patients, or to surgery events – it might be helpful to refine the Figure title.

**Thank you for your comment. The number of patients in the study is the same as the number of unique first surgeries during the study period and all analyses were performed based on this number (n=84,369).**

**The number of patients receiving additional surgery was indeed small and for clarity we have removed this information from the manuscript completely. (See**

changes to Figure 1 and Table 1 wording. We have also made changes to the Methods to remove 6 month time frame.)

Demographics reported do not include ethnicity/race data. Is there an explanation why this is not included or reported, as a lack of analysis of the intersection of this with other demographic data (e.g. age, sex, surrogate of income) would be considered a limitation of the work?

**Thank you for your comment. We agree that race/ethnicity could play a role in opioid prescribing/filling and this has been shown in some small studies. While some race/ethnicity data is available at ICES, the information is based strictly on surname to identify Asian ethnicity only. The lack of race/ethnicity data has been acknowledged as a limitation of this study in the Discussion.** (No changes made.)

I was confused as to what the difference was between the “T +/- axilla” and “R +/- axilla” categories. I.e.. what is the “radical” part denoting. What is radical surgery on the breast beyond a mastectomy? It would seem that axillary surgery would increase the invasiveness, ie. lumpectomy only < lump + SNB < mastectomy < mastectomy + SNB < modified radical mastectomy or a lump + ALND?

**Thank you for your comment. Agree that this is confusing and this limitation is acknowledged in the Discussion. Radical excision denotes removal of the breast and axillary lymph nodes; however, a small percentage of the patient’s undergoing “radical excision” seem to have separate codes for axillary intervention. This likely reflects miscoding and is a recognized limitation of health administrative databases.** (No changes made.)

I was surprised to note a pediatric hospital is performing ambulatory breast surgery on patients >18 (?)

**Thank you for your comment. We agree that this is somewhat surprising. Likely this reflects the performance of partial excisions for benign disease (fibroadenoma, cystic lesions) but we cannot comment any further on this practice. Perhaps some pediatric hospitals continue to see patients beyond 18 years of age in exceptional circumstances. I have seen this rarely in my own practice.** (No changes made.)

Why was 7 days chosen as the 'window' for repeat Rx fill?

**Thank you for your comment. Seven days was selected, as from a clinical standpoint, this is the period of time where patients experience acute post-operative pain. A similar timeframe has also been used by other investigators who have assessed post-operative pain in outpatient breast surgery patients (References: Hartford et al PMID: 31342371, Hartford et al. PMID: 30359828, Mitchell et al. 2012 PMID: 22713999). Please also see comment #42.** (No changes made)

For clarity I would appreciate more detail on the data used from the NMS database. I assume the database provides detail that a physician has ISSUED a narcotic prescription, and also information that the patient has FILLED it. Do we know how many surgeons did NOT give a narcotic prescription to their patient? This information would really enhance the discussion and analysis.

**The NMS database collects information from prescription dispensers, and therefore would include information recorded by the pharmacy where a prescription was filled. Therefore, if patients were prescribed medication, but did**

not fill the prescription, then this would not be captured by the NMS database. Unfortunately, there is no reliable method to verify surgeons who did not prescribe an opioid.

Please see here for information on the NMS database

[https://www.health.gov.on.ca/en/pro/programs/drugs/ons/monitoring\\_system.aspx](https://www.health.gov.on.ca/en/pro/programs/drugs/ons/monitoring_system.aspx)

(We have provided extra information on the NMS database in the Methods, Administrative Databases)

Some comments re: results/analysis. Great to see opioid use decrease over the study period – and fascinating to see the breakdown of the different types of narcotics prescribed: seeing codeine as #1 underscores that we have work to do.

**Thank you for your comment. Agree that utilization of codeine is concerning!** (No changes made)

The proportion of “partial” vs. “total” surgeries is quite a striking discrepancy, I assume about 90% lumpectomies vs. 10% mastectomies. Do the authors have any comment about this? I wondered is this due to many centres continuing to admit mastectomies overnight and so not included as ambulatory surgery.

**Thank you for your comment. Because a substantial number of patients had benign disease, likely these patients underwent partial excision, which would inflate the numbers of partial excision compared to mastectomy. We have also observed that rates of mastectomy in patients with breast cancer have been increasing over time, so likely the 90%:10% ratio is not accurate for breast cancer surgery.**

**Also we agree that some patients undergoing mastectomy may be admitted overnight; however, at our institution they are not. If patients were planned to be admitted to hospital after surgery, they would not be included in this patient cohort and therefore, would reduce the numbers of patients undergoing mastectomy.** (No changes made.)

Any comment as to why the specific demographic of females 30-39 most likely to fill Rx? Page 14 line 40 – the comment is made that “younger women may desire or require more extensive breast surgery”, however this does not apply to this patient population since reconstructive surgery was excluded. It would be helpful to clarify what is meant by this

**Thank you for your comment. The reference group for this comparison was women aged 18-29. We selected this group as the reference as we wanted to show differences in the women most likely to receive breast surgery (all the other age groups have higher numbers of patients receiving breast surgery, as expected). In the adjusted analysis of patients requiring more than one opioid prescription within seven days of surgery (Table 4), patients aged 30-39, 40-49 and 50-59 were all associated with increased risk, the highest being in the age 30-39 group. With respect to the 30-39 age group, this is a group that does not participate in routine mammographic screening and therefore presentation of cancer tends to be more advanced. As such, there is often a need for multimodality therapy and more extensive surgery. This age group also is more likely to select mastectomy due to advanced disease, preference and genetic mutation considerations, even in the absence of reconstruction. Our data shows that OME is higher in those undergoing more extensive surgery, and therefore it seems to make sense that**

**this age group may be more likely to select mastectomy and as a result have more OMEs prescribed. (No changes made.)**

Page 14 line 10 – “signal appropriate need for opioid medications”. This phrase suggests that “appropriateness” is being adjudicated. Rather, better to consider that multimodal or tailored approach to opioid prescribing may be best for a given patient.  
**Thank you for your comment. We agree with your suggestion. Changes have been made to eliminate the word “appropriate”.** (See changes to manuscript Interpretation.)

Page 14 line 48 – “patients with malignancy require more extensive surgery” – another point worth considering here is that the psychosocial considerations of a cancer diagnosis may well result in different prescription writing and filling practices, such as different perceptions of pain, confounding anxiety/depression, use of opioids as sleep aid, etc. - lots of references available on this point  
**Thank you for your comment. We agree with this statement. We have made changes to the manuscript to highlight this important point.** (See changes to manuscript Interpretation as well as an associated reference.)

Page 16 limitations -- Is there a way to identify patients who were current/chronic opioid users at time of their surgery? I.e., Can the NMS identify patients who had filled a Rx within X amount of time prior to surgery date? As the authors implied, this is an important consideration when identifying patients at risk of increased postoperative opioid use and opioid-related complications.  
**Thank you for your comment. We performed a sensitivity analyses excluding those with previous opioid use disorder (based on ICD-10 codes) and those prescribed buprenorphine and methadone. This did not show any significant differences in our study results.** (See changes in Methods, Statistical Analyses and Ethics Approval)

Page 16 line 31 – A matter of opinion perhaps but I disagree that the responsibility to examine opioid prescribing practices is on “institutions”. Individual providers writing prescriptions have a personal responsibility to their patients. Organizations such as surgical societies/associations, and bodies such as CPSO, CMPA should be leading and encouraging examination and education of its members to improve prescribing practices.  
**Thank you for your comment. We agree with your statement. Changes have been made highlighting the responsibility of individuals, the institutions they work in and professional organizations.** (See changes to manuscript Interpretation.)

The data gives a general picture, a bird’s eye view of the prescribing practices in this specific setting: it tells us about the amount of opioid that is dispensed and available to the public after an ambulatory breast surgery (whether the medication is in the hands of the patient or diverted). As the authors have identified, it does not tell us the granular information about individual patient use, unused amount, use of other analgesics or patient education etc. and so conclusions must be made carefully.  
**Thank you for your comment. (No changes made.)**

I suggest the conclusions might be ideally framed around surgeons’ prescribing patterns rather than patient opioid use, since that is really what the data is capturing and so the statements can be stronger. I would be curious to know more about the prescriber demographics (e.g., age and sex of surgeon, year of graduation from medical school

etc.) if possible as it would add to the greater picture. The current study is very compelling in that it starkly identifies the variability in prescribing despite a nicely summarized body of evidence that most patients do not need opioid prescriptions for pain control if they are given other alternatives and/or part of an engineered postop pathway. The reality is that the national opioid overdose crisis is a serious public health issue, worsening in the pandemic, and so I think the title should stand as a command, not a question!

**Thank you for your comment. While what you say is absolutely true with respect to provider prescribing, due to the limitations of the NMS database capturing prescription filling (as a proxy for provider prescribing), we feel our conclusions are appropriately stated. We agree that provider characteristics would be an important area to study and plan this as a feasible next step in our research program. Knowing the characteristics of the providers would allow for the development of targeted interventions to address opioid over-prescribing in providers.**

**Thank you for the comment about the title. We received feedback from Reviewer 1 as well and have since modified the title. (See changes in Title.)**