Article details: 2020-0067	
Title	The impact of a single-entry intake system on access to outpatient specialist visits: a systematic review
Authors	Milica Milakovic MSc, Ann Marie Corrado MSc, Mina Tadrous PhD PharmD, Mary Elizabeth Nguyen, Sandra Vuong MSc, Noah Michael Ivers MD PhD
Reviewer 1	David Urbach
Institution	Surgery, University of Toronto, Toronto, Ont.
General comments (author response in bold)	Peer review comment1. Suggest a different term than "multi-tier" to describe health care financing systems that are not "single-payer" (it can sound judgmental; probably best to call them "multi-payer") (page 4 line 46, page 6 line 17) **Thank you, it has been changed to multi-payer** **Changes to manuscript: "multi-payer" page 5.** Peer-review comment:2. In methods, when you say you measure "type" of SEM, please list the category names that were abstracted/analyzed. **Thank you, I have clarified this in data collection (page 5)** **Changes to manuscript: Under Data Collection, added "study design, type of single-entry model (central-intake, centralized approach to triage, pooled referral system)"** Peer-review comment:3. How were wait times analyzed and compared? mean waits? median waits? If SEMs don't increase the number of services provided (ie there is no *change* in supply or demand, only rebalancing) then one might wonder why average wait times will change rather than *variation* in wait times (e.g. why one patient waits 3 weeks and another waits 10 months) **The authors thank the reviewer for this comment. Wait times were analyzed and compared via an arithmetic mean; this information has now been added to the methods. Not enough sample size information was included across the included studies to be able to calculate a weighted mean. The authors completely agree that analysis of the variation in wait times would be an important variable to analyze, however this was scarcely reported across the included studies. As such, we have noted this suggestion as a future direction in the Interpretation.** **Changes to manuscript: Revision of the following sentence in Data Analysis (Methods): "For all outcomes, data was reported using arithmetic means, frequency and proportions as needed."Addition of the following sentence in the Conclusion: "As well, SEM may not only impact average wait times across study cohorts, but also the variation in wait times, which should be the focus of further investigations."** Peer Reviewer's Comment: 4. What is meant by "sustainability" of SEMs in the discussion? **The authors have now defined sustainability of SEMs per the reviewer's request.** **Changes to the Manuscript: The following sentence in the limitations section of the Interpretation has now been modified. The revised sentence now reads: "Sustainability of SEMs (i.e. the ability of SEM models to persist over time) was not adequately assessed in all studies."**
Reviewer 2	Pier-Alexandre Tardif
Institution	Department of social and preventive medicine, Faculté de médecine, Université

	Laval, Québec, Que.
General comments (author response in bold)	Thank you for the opportunity to review this study. The authors conducted a systematic review in order to assess the impact of «single-entry model» on wait times for specialist consultations. The manuscript is clear and its methodology usually sound and compelling. Lack of data and heterogeneity precluded a proper meta-analytic assessment so standard (unadjusted) statistics were reported instead, which limited the strength of evidence for the intervention of interest. I have a few comments and interrogations. Major points: Introduction Peer Reviewer's Comment: (I1) Last paragraph. Systematic reviews usually evaluate one association between an exposure (intervention) and an outcome. The objective could be reformulated to make it explicit that «wait time» is the (primary) outcome of interest, whereas referral volume and patient/provider satisfaction are secondary outcomes. Given the search strategy used in this review, it is possible that studies strictly about these secondary outcomes have been missed. **The authors thank the reviewer for their comment. In this project, the exposure was defined as the baseline wait time of the practice, as well as the practice specialty, while the outcome of interest was the absolute reduction of the wait time following implementation of a standardized entry model. This has now been clarified in the Methods.** **Changes to the Manuscript: Addition of the following sentence in the Methods (Data Analysis): "The exposure was defined as the baseline wait time of the practice, as well as the practice specialty, while the outcome of interest was the absolute reduction of the wait time following implementation of a SEM."** Materials and Methods (MM1) The manuscript is pretty much exhaustive in terms of reporting items suggested by PRISMA (note that if the PRISMA Checklist is going to be published alongside the article, in the section «Search», the mention «Figure 1» in the column «Reported on page #» should be replaced by «Appendix A [or 2]»). **The PRISMA checklist was included as supplementary material for the peer review process per journal requirements.** **Changes to the Manuscript: None currently.** Nevertheless, authors might want to add other information in order to satisfy the items #7 (list of excluded studies), #10 (reported funding of studies included in the systematic review) and even #15 (publication bias, which is only addressed in one short sentence at the end of the Interpretation) of AMSTAR-2. **Thank you, we have included the references of the 7 excluded studies and added a sentence to the Results section. We have included two columns in Table 2 to address funding and have included a sentence in the methods and results. Unfortunately, publication bias could not be formally assessed for the included studies given that there was a significant paucity of information related to standard error and other measures of variability across the included studies.** **Changes to the Manuscript:** **1. In Results we added "Seven articles were excluded in the full text**

screening.19-25" As well, the 7 studies have been added to the reference list (#19-25).
2. In figure 1 under Full-text articles excluded, with reasons, we changed "No referral to specialist" to "No generalize referring provider"
3. In table 2, we added two columns: Conflicts of Interest and Funding.
4. We added the following sentence to the Methods section "As well, risk of bias related to study funding and conflicts of interest were also assessed. This was completed by two reviewers (M.M., A.M.C) and discrepancies were resolved through consensus or by consulting the senior author (N.M.I.)." As well, the following sentence was added to the Risk of Bias section in the Results: "One study reported high risk of bias for conflicts of interest and no studies reported high risk of bias for funding.18"
5. Addition of the following sentence to the limitations section of the Interpretation: "Publication bias is also possibility in this review, but this is difficult to statistically assess due to limited reporting of standard errors within the included studies."

(MM2) Section «Study selection». Were there any specific study designs included/excluded?
Only non-original data was excluded (ex: reviews)
Changes to manuscript: In Methods under Study Selection, included sentence "Studies were excluded if they did not report on original data."

(MM3) Did the search extend to the grey literature, thesis/master repositories, and websites of healthcare organisations? How likely are studies on this topic to be conducted by healthcare organisations or governments and thus not published in mainstream medical journals?
The authors thank the reviewer for this consideration. We made a deliberate decision to not include grey literature on this topic. From a clinical perspective, it is unlikely that meaningful data relevant for this project exists in the grey literature. As well, to ensure that included studies were subject to rigorous peer review, the grey literature was not consulted. We have now specified this in the Methods.
Changes to the Manuscript: Addition of the following sentence in the Methods: "The search was not extended to include grey literature to ensure that included articles underwent a rigorous peer review process."

Results
Table 2 shows that with the exception of Hazlewood, the authors considered that all studies had low or moderate level of bias in all areas but «confounding». This triggers a few interrogations on how authors handled the information provided by the studies.

Firstly, it is a bit surprising that out of 12 «serious/critical» assessments (among 70 assessments), 10 of them were attributed in «confounding». Evaluation tools such as ROBINS-I are not always totally straight-forward in their use and interpretation. Could it be that authors either underestimated bias in most categories while overestimating its impact on one particular category (confounding)? Or have they considered that the impact of a confounding bias would be more detrimental to the robustness of study results, and thus justifying a more severe evaluation?

The authors thank the reviewer for this comment. We agree with the reviewer that the high number of studies with serious or critical risk of bias related to confounding is concerning. The authors tried in every possible way to be objective in their evaluation of the ROBINS-I tool, specifically by using the official ROBINS-I tool assessment checklist and by having 2 independent authors conduct each assessment. As well due to the study design of the included studies, the ability to estimate an effect size for the outcomes of interest was at high risk of bias because none of them accounted adequately for confounding. For this reason we emphasize the need for more rigorously conducted evaluations in future
Changes to manuscript: None currently.

Secondly, Table 2 shows that as soon as a given study has a serious bias in any domain, its overall score is automatically interpreted as «serious» (or worse), thus limiting the strength of its evidence. Under these circumstances, authors logically concluded (section «Conclusion») that efficacy remains uncertain and that future studies should «feature methodological or statistical methods to control bias». However, this suggestion itself remains vague and it would be relevant if authors could underline (in the section «Interpretation») which study design should be used to evaluate either the efficacy/cost-effectiveness/sustainability of SEM as well as specifying the factors which ought to be taken into consideration (in data collection and later in the statistical analytic, or per the study design) to minimize the potential of bias in the evaluation of the association between SEM and wait times.
The authors appreciate the Reviewer's comment, and accordingly have outlined the optimal study design and methods to control and minimize confounding bias in this setting.
Changes to the Manuscript: Addition of the following sentence in the Interpretation section: "Given the significant risk of confounding that exists secondary to patient, provider and system-level factors, pre- compared to post-SEM implementation studies should be conducted which control for confounding factors with randomization, matching, restriction, stratification or multivariate regression depending on context."

Interpretation
(D1) First paragraph, Lines 26-27. Considering that ideal statistical methods were not used, this conclusion might be too «strong» (unjustified). The authors reported a coefficient of determination and several p-values (relative to difference in absolute terms) in the Results section, but these are unadjusted statistics and no overall measure of association (meta-analysis) was reported. If the impact of the «bias due to confounding» is as serious as the authors reported in their Table 2, then this conclusion should be nuanced accordingly (the authors seem to acknowledge this in the last paragraph of the section «Interpretation»).
The authors feel that the reviewer's perception of the first paragraph of the Interpretation is fair. As such, we have tempered these findings by noting the associated variability in SEM implementation and reporting between studies, as well as associated risk of bias.
Changes to the Manuscript: Addition of the following sentence in the Interpretation: "These findings should be considered hypothesis-generating given the variability in SEM implementation and reporting between studies, as well as the significant associated risk of bias."

(D2) Second paragraph, lines 46-50. Similar to previous comment. I might have missed something in the Results but the «formulation» here is not compelling. While it is certainly relevant to synthesise available data on this topic, saying that the review «supports the implementation» and «adds further information» imply that the review brings new information beyond what is already reported in the original studies.

The authors agree with the reviewer, and have accordingly rephrased this section of the manuscript to ensure clarity.

Changes to the Manuscript: The following sentence in the second paragraph of the Interpretation section has now been rephrased. The revised sentence reads: "This current review builds upon those results, indicating the potential benefit of SEMs beyond surgical care and synthesizes the evidence related to implementation strategy, referral volumes, and both patient and provider satisfaction."

Minor points:
Abstract.
(AB1) Section Background. SEM is defined twice («SEM» should be used in the last sentence)

Thank you. I removed one sentence so that it is not redundant.

Changes to manuscript: Deleted "Single-entry models (SEM), which combine patients in a shared queue to see the first available specialist, is one strategy used to reduce wait times.

(AB2) Section Methods. If space allows, acronyms such as «OECD» could be explicitly define on their first use.

I have changed it to be explicitly defined.

Changes to manuscript: "Studies from The Organization for Economic Co-operation and Development (OECD) countries were included if they reported on the effects of SEM on wait time one (WT1),"

Methods
(MM1) Section «Search». Acronyms such as «Medline, EMBASE, CENTRAL, CINAHL, PRISMA» could be defined prior to their first use.

Thank you, we have now defined these acronyms.

Changes to manuscript: Under Search in the Methods section, we have added " A systematic literature search was conducted on Medical Literature Analysis and Retrieval System Online (MEDLINE, Ovid), Excerpta Medica dataBASE (EMBASE, Ovid), Cochrane Central Register of Controlled Trials (CENTRAL) and Cumulated Index to Nursing and Allied Health Literature (CINAH, EBSCO)"

(MM2) Section «Search», lines 14-15: add a reference to PRISMA guidelines.

Thank you, we have added a reference to PRISMA guidelines.

Changes to manuscript: Added reference #7 "Moher D, Liberati A, Tetzlaff J, Altman DG, The PRISMA-P Group. Preferred reporting items for systematic reviews and meta-analyses: the PRISMA statement. PLoS Med. 2008;6(7):e1000097."

(MM3) Section «Study selection. Same comment for 'OECD'.

I have changed it to be explicitly defined.
Changes to manuscript: To facilitate generalizability with the Canadian
healthcare system, studies were excluded if they were not conducted in The
Organization and Economic Co-operation and Development (OECD)
countries.[20]

Results
(R1) The term «Results» is not in bold characters.
thank you, changed.
Change to manuscript: Bolded

Changes to manuscript: (R2) lines 53-54, there are two opening parentheses but a
single closing one.
Thank you, this has been changed.
Change to manuscript: Changed: (group 1: 100%, group 2: 86.2% (p=0.009)).

Interpretation
(I1) Same comment as above for the section «Interpretation».
Thank you, changed.
Change to manuscript: Bolded.

(I2) Last paragraph, fourth sentence. Replace «were unable to be calculated» by
«could not be calculated».
It has been changed now.
Change to manuscript: As such, weighted averages could not be calculated
because either sample sizes were unknown or sample sizes for specific
categories (urgent, routine, moderate referrals) were not specified.

Figures
(F1) Figure 2. The differences in terms of absolute reduction of wait time across
included studies is huge and the legend specified either the type of surgery or the
type of referral for studies with more than one evaluation; it would be informative to
also document this for the remaining studies so the reader can try to understand
why Clark and van den Havel are so distinct from, say, Hazlewood or Wittmeier.
We have now specified the type of referral for the remaining studies in
Figure 2.
Change to manuscript: added: *Clark: chronic pain[11] *van den Heuvel:
surgery[18] *Bungard: cardiology[14] *Schachter: nephrology[17] *Goodsall:
gastroenterology[12] *Wittmeier: physiotherapy[13]
***Note: error bars represent the standard deviation where reported by**
included studies

(F2) Figure 4.
i) Add WT in parentheses after «Wait Time» in the title so the acronym is define for
use in the vertical axis.
Thank you, added.
Changes to manuscript: (WT)

ii) It would be informative to give some examples of specialties included in «Other»
in the figure legend.
Thank you, I have made the changes.

	Changes to manuscript: added "*Other= chronic pain and physiotherapy"
	(F3) Figure 5. I presume «Absolute reduction» is also in days? It could be specified (like the horizontal axis). **I added (days) to the vertical axis.** **Changes to manuscript: Added "(days)"**
	Tables (T1) In all tables and appendix, it is unclear in which order the studies are presented (publication year, study author, study design sample size…?). **Thank you, we changed to present the studies in chronological order based on publication year** **Changes to manuscript: Tables 1-3 have been changed.**
	Appendix 1. The table could be simplified a lot. 6/10 studies have «N/A» as information and could be removed from the table; however, to avoid not conveying information, a legend could enumerate these 6 studies and underline that they did not provide any information on the categories of interest. **Thank you, we have removed the 6 studies that reported N/A and included a legend under the table.** **Changes to manuscript: Removed Leach et al., Bichel et al., Bungard et al., Schacter et al., Clark et al., and Goodsall et al. from the table. Included " *Leach et al., Bichel et al., Bungard et al., Schacter et al., Clark et al., and Goodsall et al. were not included because they did not provide any information on the categories of interest"**
	Appendix A. i) Shouldn't it be renamed 'appendix 22' since the previous appendix is named «Appendix 1»? **Thank you, we have changed the name .** **Changes to manuscript: Changed Appendix 1 to Appendix B.**
	ii) Step 3 of the search strategy. On Ovid Medline, the " " do not seem to be recognised and could simply be removed. **Thank you, we have removed the quotations.** **Changes to manuscript: Changed to Appointments and Schedules/**
Reviewer 3	David G. Moores
Institution	Family Medicine, University of Alberta, Edmonton, Alta.
General comments (author response in bold)	Peer review comment: This is a solid piece of work and important to clinicians and the patients we serve. The Methodology, Results and Interpretation meet all reasonable standards for a Systematic Review/Meta-analysis. On reflection, it opens many doors/issues in terms of the consultation/referral process that need further investigation. In 1993/94 The College of Family Physicians of Canada and the Royal College of Physicians and Surgeons of Canada attempted to address some of the elements of consultation and referral and the relationship between family physicians and specialist consultants with the discussion paper - Relationship between Family Physicians and Specialist/Consultants in the Provision of Patent Care. In 2006 both Colleges followed up with the conjoint discussion paper - Family Physicians and other Specialists: Working and Learning Together. Despite this 27-year history of acknowledgement of these challenges, Canada, as the authors have identified,

"performed the worst in wait times for specialist appointment" of the eleven (11) countries in the Commonwealth Fund survey in 2016.

Single Entry Models (SEMs), although critical to improving wait times for consultation and/or referral, will not address the fundamental problems of limited or restricted consulting practices and levels of professionalism.

Clinicians in the same "speciality" often offer different variations of limited or restricted practice. Most often the limitations or restrictions are related to interest and possibly payment levels. They are always characterized by a diagnostic label. When a requesting physician is struggling with a diagnosis, he or she may be concerned about 'annoying' the consultant who has expressly documented that he or she will not see people with certain conditions (diagnoses). One need only to refer to the Alberta Urological Institute's website to see lists of urological conditions that are not "accepted or acceptable". Requesting an opinion or a procedure that a consultant will not consider is often referred to as an "inappropriate referral". These limited or restricted consulting practices are not limited to urology. "I don't do livers", a comment to me from a consultant gastroenterologist at the University of Alberta, suggests that the education and training for one to become a gastroenterologist has changed in the past 30 years. Since when have livers not been part of GI? Limited or restricted practice, both in consulting practices and to some degree general/family practice seems to be the reality today.

Since the death of Greg Price, a 31-year old Albertan, in 2012, we should all be more aware of wait times for requesting and responding physicians. Greg's cause of death was listed as a pulmonary embolus occurring three days post orchidectomy. More attention to consultation/referral processes is being paid. For one year Greg interacted with three (3) primary care providers, a general surgeon, three (3) urologists, a radiologist, an emergency physician and others. Greg's cause of death was the shambolic consultation/referral (request/response) process.

A critical review of levels of Single Entry Models (SEMs) in requesting consultation and /or referral is an important next step. SEMs are fundamental to facilitating the request for an opinion or a technical skill but they won't address the lack of professionalism and impact of severely limited or restricted scopes of practices.

The authors thank the reviewer for these valuable insights. We have summarized the reviewer's points in the Conclusion section of the manuscript, which now outlines the various scenarios in which SEMs may or may not function, and the need for further research in these areas.

Changes to the Manuscript: Revision of two sentences in the Conclusion. The two revised sentences read: "It remains uncertain if SEM can be applied to all specialties and across diverse settings (e.g. rural environments, restrictions to scope of practice, changes to subspecialty practice, different physician payment models, marginalized populations and developing nations). Future studies should investigate the implementation of SEMs in these settings."