Author's response to reviews

Title: Physician attitude toward depression care interventions: implications for implementation of quality improvement initiatives

Authors:

Rachel EM Henke (mosher@hcp.med.harvard.edu)
Ann F Chou (ann-chou@ouhsc.edu)
Johann C Chanin (chanin@ncqa.org)
Amanda B Zides (aberke23@aol.com)
Sarah H Scholle (scholle@ncqa.org)

Version: 2 Date: 29 May 2008

Author's response to reviews: see over
May 29, 2008

Martin Eccles, MBChB, MD and Brian Mittman, PhD
Co-Editors-in-Chief
Implementation Science

RE: Physician attitude toward depression care interventions: implications for implementation of quality improvement initiatives

We are pleased to have the opportunity to revise our manuscript (#4083912251787696), entitled “Physician attitude toward depression care interventions: implications for implementation of quality improvement initiatives,” for your consideration. The reviewers’ comments were insightful and helpful. We addressed the reviewers’ concerns and made the recommended changes. The reviewers’ comments (numbered and in italics) and our responses are below.

In response to Reviewer 1’s comments:

**General Comments:**
(1) The article makes a solid contribution to an important and timely issue for the health service research community and for health care organizations. Many researchers, clinicians, clinical managers, and policy makers should find this study informative and valuable. Although there are a few problems with the current draft, I believe the authors can easily address them.

We thank the reviewer for the favorable review.

**Major Compulsory Revisions:**
(2) The authors should provide additional detail on their data analysis techniques. I have no objection either to their utilizing Atlas, a highly regarded qualitative data management package, or conducting a portion of their data analysis without software. Rather, I would like the authors to describe fully and justify how they utilized their two methods. For example, perhaps they utilized Atlas to link quotations to codes and worked without software to rate general qualities of
individuals or organizations (e.g., general receptivity to new practices). These are of course just two examples out of several possibilities. Without more detail, it is not possible to understand, evaluate, and replicate the authors' data analysis methods fully.

Based on the reviewers’ suggestions, we provided additional detail about our data analysis techniques in the methods section including an explanation of our use of both Atlas and manual coding. The text is now as follows “Our analysis followed a grounded theory approach, a systematic process that enables researchers to identify broad concepts from the data [23]. Two members of the research team independently reviewed notes and transcripts from the interviews as they were completed to identify key concepts and themes related to understanding barriers and facilitators to depression care. The team members met to develop a preliminary coding scheme, resolving any inconsistencies through discussion. All transcripts were then coded using this scheme by one team member with Atlas software v5 [24]. A second team member independently coded a subset of the transcripts manually; results were compared results to ensure reliability. The coding scheme was modified throughout the coding process to include additional concepts not captured by preliminary codes. Emergent themes were identified from retrieval of coded data and discussed during team meetings. Blocks of coded data were summarized in the results.”

(3) The information regarding scoring performance measures in the methods section is unclear and I am confused about why it is included. First, it is not clear exactly what the participants did or how the authors might have controlled possible basis resulting from participants rating their own performance. Further, there is no mention in the results or discussion of findings utilizing this data. The authors should do one of the following: 1) report and discuss relevant findings and explain how they handled potential bias, 2) explain why mention of this data collection but not its findings is relevant or appropriate here, 3) remove mention of this in the methods section.

We clarified the manuscript by removing mention of scoring performance measures. The scoring component of the study was unlikely to have affected the interviewees’ responses as the scores were presented and understood as preliminary and subject to how care was recorded and collected from the medical records.

(4) The informant selection criteria are not sufficiently justified. First, why is exposure to varying levels of performance measures important? Additionally, why in a study about depression did the authors seek physicians "who achieved recognition for diabetes care by the American Diabetes Association/National Committee for Quality Assurance Diabetes Physician Recognition Program (DPRP)?"

We expanded our sample and recruitment section to justify the informant selection. The section now includes the following text: “We recruited physicians using two methods to maximize the geographic and practice diversity of participants as well as the diversity in exposure and training in quality improvement interventions. The study group included both physicians who were high and low performers in depression care. First, we
recruited physicians from health care organizations in the Southeast, the Western, and the Mid-Atlantic regions participating in performance measurement programs. Organizations with greater exposure to performance measurement program were more likely to have practice tools available for caring for chronically ill patients.” We also added sentences to describe why we sought informants who had received DPRP recognition for diabetes care: “Second, we recruited physicians from a national database of physicians who had recently achieved or applied for recognition for diabetes care by the American Diabetes Association/National Committee for Quality Assurance Diabetes Physician Recognition Program (DPRP). Depression is a common co-morbidity of other chronic illnesses such as diabetes and heart disease, and physicians who have success in managing patients with diabetes were likely to have experience with models of care for chronically ill patients but were not necessarily high performers in depression care. ”

(5) In the discussion section, the authors state that "resistance" to interventions facilitating communication between primary care and psychiatrists, "must be overcome" before such interventions can be useful. The authors should explore what they mean by overcoming resistance, especially in light of a growing consensus that this may not be the most appropriate approach for fostering sustainable change. Many researchers and policy analysts now argue that rather on focusing on overcoming resistance, program implementers should partner with relevant stakeholders on the design and implementation of new practices and programs.

We thank the reviewer for this insightful comment. We agree that the phrase “overcoming resistance” requires additional interpretation in light of growing awareness in the research community that sustainable change is achieved by group decision-making, consensus, and motivation. We changed the relevant text in the discussion section and added the Greenhalgh et al reference. The text now states: “For practices considering implementation, initiating discussions to obtain and integrate physician input on implementation may be of value. To facilitate successful implementation, practices may partner with the community and relevant stakeholders on the program design, planning, and implementation [36] (page 18).”

(6) Although the authors state that their informants are diverse, they also state that they do not believe their findings will generalize merely because their methods are qualitative. This is somewhat dated thinking that I am not sure the authors completely believe. After all why would they conduct a study that they believed only applied to the specific individuals who participated? At the very least, the notion that we can never generalize from qualitative findings is controversial and I believe the authors can remove mention of this limitation. If they are uncomfortable with that, however, they should at least acknowledge the controversy (e.g., see: Morse, J. M., (1999). Qualitative generalizability. Qualitative Health Research, 9, 5-6.

We agree with the reviewer that the criticism that qualitative studies are not generalizable is dated. We removed mention of this limitation in the manuscript.

Discretionary Revisions:
(7) The results are generally well organized and clear but two of the quotations do not seem to be the best exemplars of the constructs the authors describe. Either the authors did not select the best quotations or they should find more apt names for these constructs they concurrently have. Specifically my concerns are:
Construct: The mental health system is fragmented and difficult to access
Supporting Quotation: Patients have to be at the top of their game to make an appointment with a psychiatrist.
Concern: The informant clearly believes that there are problems with accessing the mental health system but does not talk about fragmentation. Are there other quotations that support this second point?
Construct: Lack expertise
Supporting Quotation: We don't have a lot of time and we don't have a lot of resources. There's not somebody I can call and say: "Take over for me, please. Help me, please!"
Concern: The informant seems to be talking about resources here, not expertise.
These are two different constructs. The authors mention both in the subsequent text. Perhaps add "Lack of Resources" and use this quote to support that and then find another quotation to support "Lack of expertise."

We reviewed the quotations associated with the two constructs and chose supporting quotations that were better exemplars. The supporting quotation for the construct ‘The mental health system is fragmented and difficult to access’ is now: ‘There have been several times that I really thought somebody needed to see a psychiatrist and not a counselor, and I picked up the phone with the patient in my office to call the 1-800 number. I tell them I am the physician and I want them to see psychiatry, and they say, ‘Number one, we can't talk to you, and number two, they have to go to a counselor first.’ So, the only way that someone can see a psychiatrist is to go through whatever mental health number that the patient has to call and they have to see a counselor first and then only that person can then send them to psychiatry. So, it's a little bit difficult. You never know when this happens.” The supporting quotation for the construct ‘Lack of mental health expertise’ is now: “I have a patient that can't get the [psychiatric] care they need and I'm just not sure what to do next. I find it frustrating and I almost don't want to see them any more because, I’m embarrassed to say, I just don't know what to do.” We did not change the title of this construct to include a mention of resources because we wanted to differentiate it from the construct ‘Insurance coverage for depression treatment.’

(8) I would remove mention of ethnic background and training for patient education. The authors do not examine the potential effects of these characteristics. Further as there is only one non-white individual and only one individual with patient education training, the authors risk exposing the identity of these two individuals, at least within the participating facilities.

We removed mention of ethnic background and patient educators in the Table and in the results section.

(9) A strength of this study is that the authors included individuals from many different types of practice environments. They also indicate that there was variability in types of
record keeping. Did these characteristics relate to the providers' assessments of the interventions? It would be informative for the authors to explore such relationships if any or if there were none to indicate this. I realized the sample size is small but it is still possible to examine patterns and offer suggestions for future research.

We highlighted cases where physicians’ practice environment may have influenced their experience with interventions in the results section. For example, in the depression screening section, we added the following text: “One physician who worked in a practice with a co-located mental health specialist noted that the presence of this individual made “screening easier” because patients who screened positive would be seen by someone on site (the co-located mental health specialist) and managed appropriately (page 11).” In the structured assessments section, we revised the following text to clarify the possible importance of practice environment: “Notably, all providers who worked in the organization where the use of PHQ-9 was encouraged and easily integrated into the electronic medical record were routine users of the PHQ-9 (page 13).” In the patient education section, we added “Except for physicians who had access to care managers, education appeared brief and oriented toward convincing the patient of their diagnosis and describing treatments because of time constraints. Further, education appeared limited to the first visit since physicians said they had limited time to provide extensive follow-up visits (page 14).” In the care management section, we added the following text: “Physicians who had care managers located within their practice (rather than off-site) tended to endorse their benefits more strongly. Physicians who used an electronic medical record system that allowed them to view care management notes including severity assessments appeared to work more closely with and receive more benefits from care managers (page 17).”

We did not find variation in the assessments of the interventions stemming from other practice characteristics such as size, geographic location, and experience with performance measurement programs. This could be due to limited sample size and requires future study. We added a paragraph to the discussion section highlighting this: “Although we found physicians response to interventions varied by certain practice characteristics such as availability of a care manager and other staff support and organizational support of the PHQ-9, we did not find variation in the assessments of the interventions stemming from other practice characteristics including size, geographic location, and experience with performance measurement programs. This could be due to limited sample size and requires future study (page 20).”

In response to Reviewer 2’s comments:

**General Comments:**

(10) This is an interesting, useful article that could be helpful to people who are involved in implementation of depression QI programs. However, the treatment of some topics is too superficial, and there needs to be more detail on these before this would be publishable.
We appreciate the favorable review and the general suggestion to add depth to the manuscript.

**Major Compulsory Revisions:**

(11) *I think the recruitment strategy needs more detail. How were the physicians from the health care organizations chosen? Was it a convenience sample, or some other approach? Why did the authors also include the group from the national database? Were they randomly selected? If not, how were they chosen? Why were there two different groups interviewed? If we want to generalize from the findings of this study, we need to understand more about how this group might be representative of how other physicians might view these interventions. I understand that sampling can be more purposeful in qualitative research than in an RCT, it will still be important to provide enough detail about how you sampled for others to interpret the results and think about how they might apply to their own context.*

The reviewer has correctly pointed out that a more detailed description of the recruitment strategy is needed. We greatly expanded the sample and recruitment section and included information which addresses reviewers’ specific questions. The text is now as follows: “We recruited physicians using two methods to maximize the geographic and practice diversity of participants as well as the diversity in exposure and training in quality improvement interventions. The study group included both physicians who were high and low performers in depression care. First, we recruited physicians from health care organizations in the Southeast, the Western, and the Mid-Atlantic regions participating in performance measurement programs. Organizations with greater exposure to performance measurement program were more likely to have practice tools available for caring for chronically ill patients. We identified a clinical champion at each practice who helped recruit individual physicians within each practice. Each site chose to use a different recruitment method. One site recruited physicians by verbal contact, another site sent an email to all physicians who were using an electronic medical record system compatible with a care management database, while the last site identified and sent a recruitment letter to physicians that had a higher percentage of patients with a depression diagnosis. Physicians in these three organizations were interviewed at their clinical care site. Second, we recruited physicians from a national database of physicians who had recently achieved or applied for recognition for diabetes care by the American Diabetes Association/National Committee for Quality Assurance Diabetes Physician Recognition Program (DPRP). Depression is a common co-morbidity of other chronic illnesses such as diabetes and heart disease, and physicians who have success in managing patients with diabetes were likely to have experience with models of care for chronically ill patients but were not necessarily high performers in depression care. These physicians worked in large and small practices that ranged from rural to urban settings throughout the U.S. and were interviewed by telephone.”

(12) *The piloting of the interview guide is not described in any detail. Who were the subjects for the piloting of the interview guide? How many people were involved in the pilot? The authors say they "modified the guide after the first round of interviews." Does*
that mean that some of the 23 interviews were done with a different interview guide, or that there were others beyond the 23 that participated in the pilot?

We appreciate this feedback and have added a description of the interview guide piloting and modification in the methods section. The revised text reads: “A panel of mental health experts reviewed and critiqued the guide and we then piloted it by interviewing a primary care physician who was not included in our study cohort to ensure clarity of question phrasing. We made minor modifications to the guide after this interview as well as after interviewing physicians from the first site to eliminate unproductive questions and add questions that facilitated discussion of important constructs (pages 5-6).”

(13) The description of the comments about each component of a chronic care approach to depression is generally sparse. The limited use of quotes in the section about barriers was helpful in providing some flavor of participants responses. I would suggest more use of quotes in the results section to expand upon the information provided.

We thank the reviewer for the suggestion. We added representative quotations to the sections describing physicians’ assessments of the components of the chronic care model to add depth to these results. Please see quotations on pages 12, 13, 14, 15, 16, and 17.

**Discretionary Revisions**

(14) I don’t understand why basic demographic data cannot be reported for the DPRP physicians. That would be important information related to who you studied as discussed above regarding sampling strategy. I understand that it may not be possible to obtain that information, but it would be very helpful to have it.

In response to this comment, we reviewed DPRP participants and added information about sex, specialty, and years in practice (see Table). We added a sentence to the results section to explain why we did not report the age range for the DPRP physicians: “Slightly different demographic information was collected on the DPRP physicians because they did not participate in the data abstraction component of the study (page 7).”

(15) In results, the authors state that physicians had varied experience with the interventions. It would be useful to know if any were in practices that had been involved in a formal QI program related to the use of this model. The information presented suggests that practices only had experience with isolated components of a chronic care intervention for depression, not with the full model.

We appreciate the reviewer’s observation that the depth of our samples’ experience with the interventions is unclear. We added text to the results section to address this concern: “Several physicians worked in practices currently or previously involved in formal quality improvement programs related to use of the CCM (page 7).”

(16) The results related to use of structured assessments could be strengthened. I noted that physicians who had experience with them reported many benefits and those who did not use them were more skeptical. Rather than a solution of disseminating research about
the benefits, a more useful approach might be to use opinion leaders, academic detailing or other clinician to clinician contact to help spread the word.

We strengthened the results related to use of structured assessments by adding three supporting quotations (pages 13 and 14). In the discussion section, we added a sentence to describe how research about the tools could be best disseminated: “Use of opinion leaders and/or academic detailing approaches may also encourage use of these tools (page 18).”

(17) I also wonder if the points about the fact that certain systems barriers (the fragmented mental health system and payment system) serve as barriers to depression care might be strengthened. Solving these problems will require more than "future quality improvement interventions." I might have cited the President's New Freedom Commission or the Quality Chasm report on behavioral health that recommended payment and regulatory changes that would then facilitate successful implementation and sustainability of improved depression care.

We agree with this observation. We added a sentence to the discussion section in response: “Payment and regulatory reform including policies that align incentives to increase collaboration among these providers and mental health parity are also recommended approaches to address these obstacles and facilitate successful and sustainable implementation of improved depression care [37, 39] (page 19).”

Thank you again for your consideration. We are grateful for the reviewers’ thoughtful comments and suggestions. Our manuscript has been greatly strengthened by incorporating their ideas and addressing their concerns. We look forward to hearing from you regarding the status of the revised manuscript.

Sincerely,

Rachel Mosher Henke
Ann F. Chou
Johann C. Chanin
Amanda B. Zides
Sarah Hudson Scholle