Dear Editor,

Thank you for your willingness to consider our manuscript, titled ‘Why do patients want to have their blood tested? A qualitative study of patient expectations in general practice’ for publication. The reviewers’ comments were very valuable to us in improving the manuscript. We were happy to read that all three of them considered our paper interesting. Below we will systematically address how we applied their suggestions for improvements. First we will address your editorial comments.

Editor’s comments

A further literature search is encouraged
We read the papers of Kravitz as you suggested. We considered 1 applicable to this manuscript and added its reference. In addition we did a further search using the text words (patient* expectations) and (patient* perceptions) in combination with test*. We added some references to the text (Salmon, Peck, Hartley) and contrasted these with our findings.

Ethics and consent
This study was an attachment of the VAMPIRE randomised clinical trial, ISRCTN55755886, which has ethics approval of both Maastricht University and Academic Medical Center/University of Amsterdam. The study protocol has been published at BMC Family Practice: BMC Family Practice 2006, 7:20. In this study no measurements of patients’ bodies have been done, and patients received no questions about their actual health. Instead they were asked about their opinions. Therefore, ethics approval for this study is formally not necessary. All patients were asked informed consent before they received the questionnaire. At any stage of the study they were free to end their participation. As can be seen from the results some patients did indeed. We did not add this information to the manuscript. If you prefer to have a statement there as well, we are most willing to add it to the text.

Including an acknowledgement section is strongly encouraged.
We have no contributors to the study who do not meet the criteria for authorship.

Reviewers’ comments
Reviewer 1: McKinstry

No details are given of the sample in terms of social class, education or chronic ill-health (all of which may have influenced desire for tests)
We added a table in which we provide not only age and sex but also country of birth and highest level of education (table 1). Unfortunately we did not collect any data on chronic ill-health status. However, details about the patients’ reason for encounter are given in the results section and previous experiences with testing were an item during the interviews, so we expect that influences of chronic disease on the desire for tests will be visible in the results.

No details are given if any attempt was made to ensure a range of patients for the study.
We tried to select a range of patients by visiting a variety of different practice types, both in urban and rural settings. Unfortunately it was not very well possible to further select specific patients based on previously collected criteria as we found it important
to select patients with an actual wish to be tested and appointments are usually scheduled for the same day or the next. As a result there was not much time between scheduling the appointment and the patient’s visit to the practice. We added some reflections on the sampling method in the discussion section.

*It is not made clear if the practices taking part were typical of Dutch practices.*

We did not aim at a representative sample for the above mentioned reason. We added the remark ‘and therefore different patients’ to the methods section to clarify this further.

*The authors should be more critical in their discussion of the sampling method.*

We added some reflections on the sampling method in the discussion section.

*The fieldwork is well described as is the coding. I was a little disappointed that no attempt in the analysis was made to link ideas and findings rather than only list them or perhaps to see if there was any clear linkage to gender or age (although I realise in a qualitative study this is problematical).*

Upon revision we agree with the reviewer that we only listed codes within a theoretical framework, which we now think was not suitable for these data. We fully revised the results section and provide three main themes which emerge from the data. Concerning the analysis of response patterns: we did not discover any subgroups with specific characteristics that have different response patterns.

*In terms of rigour the analysis was repeated by more than one researcher to ensure reliability. The study describes no triangulation, possibly an opportunity was missed with the screening questionnaire.*

We agree this is an omission and added a critical remark to the discussion section.

*It is not clear if the investigators sought out observations that might have contradicted or modified the analysis. While they refer to some new data arising from a nurse led interview this is not described. It is not clear to what extent the interviews may have been influenced by the status of those conducting them (hinted at above) and perhaps this should be clarified.*

In our opinion we repeatedly described different opinions e.g. about the acceptability of watchful waiting and about actively asking to be tested. A nurse who visited her GP as a patient added remarks about her professional knowledge influencing her desire to be tested. This did not contradict but add to the analysis. We agree that we did not mention the nurse’s opinion (see also next remark) separately and added her remarks to the results section.

We think there has been some misunderstanding of the text. In contrast to the remark of the reviewer, the nurse did not lead the interview but she visited her GP as a patient and therefore was a participant. In the methods section it says ‘Each practice was visited 1 – 3 times for a full working day by one of the authors (MP)’ and ‘although the coding of one of the last interviews, with a patient who worked as a nurse, yielded a number of new themes…’

*The conclusions are set in relation to GPs views which are assumed and not part of this study. The conclusion would be better confined to what was found in this study and the need for research into the impact of education of patients in this domain.*

The reviewers disagree about the value of the dilemma presented in the discussion section. Therefore we decided not to leave it out completely. We agree that remarks...
about the need for research into the impact of patient education needs to be added and
did accordingly, both in the discussion and in the abstract.

Reviewer 2: Kravitz

1. Page 4: Some sense of the types of practice (context such as urban/rural location,
socioeconomic status of patients, etc.) and the number of doctors per practice would be most
helpful.

Location of the practices had been given, numbers of GPs per practice were added. We
asked about level of education as a measure of social status of the participants and
added this in a new table, table 1. Unfortunately, we do not have details about the
socio-economic status of the practice populations as a whole. Type of insurance used
to be a good measure of this in the past, but the system changed recently, so we are not
able to retrieve the necessary data from the practices.

2. Please provide further details on the coding process to include how the themes were generated
(from "nowhere" or from an underlying theory) and who reached consensus (just the two primary
reviewers or others).

We added information to the final paragraph of the methods section about how themes
were generated and who reached consensus.

3. Can you tell us anything else about the demographics of the 22 participants, eg race, ethnicity,
religion, occupation, education, social class, etc.?

We added a table in which we provide not only age and sex but also country of birth
and highest level of education (table 1). Unfortunately we have no information about
the patients’ religion and occupation. See also the remark after point 1.

4. The analysis often misses opportunities to go deeper. For example, the concept of "reliability" is
not fully explored. Tests can be "unreliable" in several ways: by not being reproducible (the
technical definition) and by producing false positive and false negative results, which simply
reflects the reality that tests are not perfectly sensitive and specific. Do the transcripts provide
any insight into whether patients are even aware of these different aspects of "reliability?"

We agree with the reviewer that the presented analysis is not optimal and think that’s
mainly due to the use of the Theory of Planned Behaviour (TPB) which we used to
categorise our findings. Therefore we abandoned the theory and went back to the data to
see which main themes emerge from the data. We think this improves the depth of the
analysis, for example about the concept of ‘reliability’. We asked patients about different
types of unexpected test results and as presented in the results section patients think that
false positives and negatives are rare or absent.

5. The division of themes into attitudes, cognitions, and social influences seems a bit arbitrary and
should be justified.

Upon revision, we fully agree. The main reason for using the TPB was that we were
encouraged to use it by other reviewers in a different paper on GPs’ determinants of
blood test ordering. As said in the previous response we left the TPB out and saw that
more appropriate categories emerged from the data. We used these to structure the
results.

6. Realizing the limitations of a study with n=22, were there any response patterns that could be
elaborated upon? For example, one might expect patients with a chronic medical condition (or
who have such conditions in their family) to view tests differently than those who are completely
healthy.
We did not discover any subgroups with specific characteristics that have different response patterns.

7. On page 11, the text twice emphasizes the comparability of respondents and non-respondents with respect to sex. The argument that the groups are comparable because they are gender balanced is weak when made once, and it doesn’t get stronger by saying it twice.

Though the groups that are compared are not completely the same, namely respondents to the questionnaire vs non-respondents and interviewed vs not interviewed patients, respectively, we realise that this may cause confusion. Therefore we left the latter comparison out.

8. On page 12, where is there support in the analysis for the conclusion that the group wants to be reassured and “attaches great, almost magical value to these tests”?

We think we can conclude this from especially the quotations and codes given in the paragraph ‘interpretation of blood test results’. Patients state that tests give them certainty and a proof of health. In addition they hardly see any restrictions of the tests’ qualities. The conclusion that patients want to be reassured comes from the ‘motives for wanting tests to be ordered’ paragraph. In this paragraph we quote patients who say tests are needed when patients are nervous and that a goal is to ascertain the patients’ health.

9. Please comment on the upside of “destroying the magic” -- patients may be less inclined to complain or litigate when they do not receive tests they think are necessary (even without indication).

We are not sure if we understood this remark of the reviewer well. Does he mean that patients who do not receive the tests they want are less likely to complain? In that case we disagree with the reviewer. Our data show that patients use very negative remarks to express their dissatisfaction when GPs refuse testing. The practicing GPs in the research team do not recognise this situation from their practices either.

Reviewer 3: Salmon

1. Using pre-existing concepts to categorise a qualitative analysis is epistemologically awkward and I think that it does not work in this instance. First, I see no reason to introduce the Theory of Planned Behaviour here. Secondly, the distinction between affect and cognition does not fit the transcripts – as the disproportionate number categorised as cognition rather than affect illustrates. Third, the section labelled ‘social influence’ includes a heterogeneous collection of findings that do not seem to me to cohere. The analysis could therefore be presented and illustrated in a way that does more justice to it.

Upon revision we fully agree with the reviewer. The main reason for using the TPB was that we were encouraged to use it by other reviewers in a different paper on GPs’ determinants of blood test ordering. We went back to the data to look which main topics emerged from them and abandoned the TPB. We restructured the results section, including replacement of table 2. The findings previously categorised as ‘social influence’ now are under different themes which we think better fit.

2. In particular, abandoning the affect/cognition/social influence distinction will help to develop the analysis. However, this will expose the need for different categories that evoke the important features of the patients’ comments. The statements currently under these headings certainly illustrate some useful findings, particularly about the different functions that blood tests have in people’s views of their illness and in the doctor-patient relationship. Tests provide a view inside
the body, into an area otherwise hidden from the GP. They can even show disease in the absence of any symptoms apparent to the patient. They provide certainty – not just about the presence of illness but about its absence. Having a blood test also means ‘doing something’. I wondered if having a test was also a way for patients to feel that the doctor was taking them seriously. Currently prevalent discourses of early detection are present in some of the comments, also.

See previous answer. We also added some references in the discussion that support our findings.

3. There is a mixture of ideas in the ‘social influence’ section. Some concern simply raised health anxiety, or comments on the doctor-patient relationship. Without more context or more transcript the relationship of these to blood testing is not apparent.

We agree with the reviewer. See answer to point 1.

4. The discussion presents an important dilemma – whether doctors should correct or work with the misconceptions that patients have. It is valuable that the authors raise this question, and also that they do not claim to be able to answer it simply. The discussion could also relate the findings on blood tests to broader literature on lay models of the body and of the ability of medical techniques to see into it.

We are pleased to read that the reviewer agrees with us that the dilemma we present in the discussion is important. As he suggested we added a reference about the lay view that diagnostics have the ability to see into the body.

Minor essential revision: The paper is generally clearly and concisely written. Table 1 is unnecessary - questions 5 and 6 could be described in the text. Table 2 will need to be removed or replaced by one based on a different way of presenting the analysis.

We removed table 1 and described the questions as suggested. We replaced table 2 by a more appropriate one.

Hopefully you will find these responses to the editors and reviewers’ comments satisfactory and consider our manuscript for publication.

On behalf of all co-authors,

Yours sincerely,

Loes van Bokhoven, MD