This paper addresses an interesting question -- how mean residence time in patches depends in the distribution of patch quality, perhaps due to some sort of habitat conversion. I'm not sure whether it is mainly because of this particular publication venue, but I found the paper difficult to evaluate because of the extensive referencing of earlier and closely related work by the same authors (for example, in the paragraphs that follow Figure 1 on page 2). The same is true of the Conclusions, which focus on showing that this approach is "equivalent" to the earlier work, although in a sense that is not fully clear to me. The overall framework seems to me to be sufficiently familiar and clear to be able to stand on its own with the more usual reference to earlier work.

As a related issue, the motivation presented feels like "let's see what happens if we extend our earlier model" (for example at the bottom of page 2). The general point about travel frequency could be emphasized. In fact, why not be more explicit about the importance of movement rates due to habitat conversion, perhaps by looking at a very simple model of a pollinator? Another possible application would be as a way to investigate some of the effects of competition. The distribution of patch qualities would also change if competition for resources, or renewal rates of those resources, were to change. This could provide a simple mechanism for altering the underlying parameter x that controls the distribution.

The only technical issue that concerns me is the exclusion of unexploited patches. The set of exploited patches depends on the realized fitness value En* and will thus change with the overall distribution of patches. In my experience with models of this type, there isn't an easy way to deal with this issue up-front. I think this could make some of the derivations rather more complicated. This issue is more difficult to address when the gain curve is sigmoidal rather than concave down, where excluded patches can be recognized directly from the slope at t=0.

I also wonder whether the derivations would be more or less identical, although perhaps simpler, if patch types were drawn from a continuous distribution. This would show that the analysis here, of changing the probabilities, and the previous analysis, of changing the qualities, are really part of a larger whole, where the probability density function itself is changed. Identifying the key statistics of that pdf would perhaps give a unified approach to the general problem.

I found Figure 2 about positive and negative correlations of resource intake with residence time a bit confusing. Wouldn't this depend on the value of En*? And is there a simple family of curves which can show the full range of behaviors as an example?

On page 5, I found it peculiar to mention that the previous analysis of varying a single p is internally inconsistent. If there is a reason to include a simpler example before the more full analysis, it would make more sense to increase one of the p's, and decrease all the others by the fraction needed to maintain the constant sum.

The goal of the calculation on page 6 is unclear, and I got rather lost in working through the equations. The derivation of the first equation on this page just by reference to earlier work was a bit frustrating also.

Figure 3 does a good job of illustrating the key results. However, It didn't seem to me like region C was discussed in the rest of the paper.

3.2 line 113: It might be illuminating to give examples of what the "dummy habitat variable x" could represent, when it is first introduced.

3.2 line 123-124: Criterion (6) comes from seeing x as a "metric of habitat quality" iff dEn*/dx > 0. So the authors are specifying a habitat variable where increasing x increases En* too (by changing frequency distribution of patches). But couldn't there be a habitat variable we care about where decreasing x increases En*? That might still say something about habitat quality. I'm not sure why we want to ignore it.

3.3 line 133: I would have liked "average rate of movement" to be defined, rather than referring to an earlier work and stating it is inversely related to average residence time. But if this is a common term in the literature, maybe that is unnecessary.

3.3: I didn't feel that these results focused on the average rate of movement, even though this section was called that. In line 145, the authors mention that expression (7) is similar to (6) with a term added, which makes it sound like a mathematical similarity. I think it is rather a conceptual similarity, because in one En* is increasing with respect to x, and in the other t_j* is increasing with respect to x as summarized in lines 166-167.