Background section does a good job of showing why such an analysis is important. Additional strengths are the population-level database, large sample, multi-level conceptualization / analysis, and fairly thorough pharmaceutical data.

One major issue complicates the interpretation of these data. The researchers assume that refill discontinuation reflects nonadherence, when it could easily reflect medically-indicated discontinuation. This possibility is not even raised as a limitation. This is especially true for antidepressants, which are often physician-discontinued due to inefficacy (about 40-50% of cases), side effects (nausea, sleep disturbance) or the spontaneous normalization of mood (because depression -- unlike DM and HTN -- is just as often transient as chronic).

I thought that the abstract could better represent the conclusions. The most important conclusion is that there really was no general profile that clinicians should expect, except being a non-Western immigrant. The second was that between and within diseases, adherence is otherwise rather medication-specific. But the abstract lists 4 results, three of which are patient factors (number of consultations, ethnicity, and comorbidity) and one medication-specific (although without randomization, one might argue that this is probably a fourth patient factor - e.g., driven by presenting symptoms or past medication history). And then it is puzzling to me why the abstract then concludes that we need to research the prescriber so much more. I think this just needs to be made more clear, or perhaps shifted in emphasis.

The Intro notes that this study will improve upon prior studies because it can compare diseases using a common measure. Yet, little was said about the differences by disease, i.e., that antidepressants had the lowest adherence (78%) and oral hypoglycemics the highest (91%).

A few comments on the measures:

First, social support is a complicated variable and a whole subfield in itself. Thus, it seems too simplistic to use living alone (vs. other) as a proxy. Unless the researchers can demonstrate that this is a valid indicator (i.e., through a prior validation study) then they should not say that they are studying social support, but rather household size.

Second, the researchers do not seem to have considered coding for Western immigrant. Maybe being an immigrant is what is important, not where one immigrated from?

Related to that, wouldn’t non-Western immigrants be far more likely to leave the Netherlands during the study period? If so, then their nonadherence estimate will be highly biased (upwards).

TCA’s are often prescribed for other reasons than depression such as insomnia and neuropathic pain. To the extent that depression diagnoses are overrepresented among these conditions, which is probably significant, the medication may not really be an “antidepressant” but rather a trial of sedative-hypnotic or analgesic. They might be able to deal with this using available disease data.

Oral antidiabetics paragraph (results) – I’m not sure that this is the best term; consider oral hypoglycemics?

Also, is this analysis “multi-level” at this point? (I thought not until prescriber was included.)

Results, positive correlation between three drug groups. – Please state the correlations in the text.
Minor wording issues included indoctors (indicators?), easiness (ease), “bad” socioeconomic status (seems more appropriate to exclude the adjective), A prescriber can “find out” (determine), …are “less easy” (more difficult) to detect, Hawthorne (Hawthorne)

**What next?:** Accept after minor essential revisions

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.