Response to Referee 1

Many thanks for your comments. They were indeed very constructive. You will find below some indication of how I will take them into account in preparing the revision of the paper.

1. I agree, the data certainly exhibits some limitations. Yet, to my knowledge, the data used for this analysis is the best available for Germany. Moreover, with respect to the dependent variable(s), i.e. the consumption of illicit drugs, the wording of the questionnaire has just marginally changed over time. The problem is more relevant for the right-hand-side variables. I should have made this clearer in the manuscript. But still, the revised version of the paper will include an appendix that provides information on the wording of the questionnaire.

2. I agree that aggregating drug use variables to one single indicator might be inappropriate, as different drugs certainly differ in terms of riskiness, availability, etc. Yet, in the data used drug consumption is dominated by cannabis consumption. There is only very limited number of individuals that report having consumed heroin, cocaine, amphetamines, etc but have not consumed cannabis. Focussing the analysis exclusively on cannabis yields quite similar results. Thus, addressing only cannabis might be an option for a revise version of the paper. (Other drugs do not allow for an equivalent analysis due to very small prevalence rates, i.e. a very small number of users in the sample.)

3. If the revised paper exclusively deals with the case of cannabis, using the information on frequency of use becomes an option. This would require using an ordered choice model instead of a binary choice model. I agree with the referee that this might offer for comparing different models and checking the robustness of the results. Yet, I am a bit skeptical whether lift-time prevalence provides an appropriate basis for any west-east comparison.

4. I agree with the referee. The lack of availability measures is a relevant caveat to the analysis. I have started to collect (regional) data on drug seizures, in order to create an exogenous measure of drug availability. I have not yet gathered the required data completely, but I hope that I can use it in the revision of the manuscript.

5. One may - of course - carry out similar analyses for licit drugs (alcohol and tobacco) and even for any other consumption good. But wouldn't this fundamentally shift the scope of the paper? There are certainly west-east differences in the consumption patterns of tobacco and alcohol and it might be interesting to address them in an empirical analysis. Yet, wouldn't this be a completely different story as prior to 1990 smoking and drinking was widespread in both parts of the country? I would prefer to stick to the original scope of the analysis.

6. I agree, since the regression-model is purely descriptive one can start with a specification that includes all available right-hand-side variables, including weight etc. This can be done in the revised paper.

7. Survey weights are used to ensure representativeness at the individual level. Since the analysis is purely descriptive and does not aim at estimating structural parameters, weights are required to yield results that are relevant to the population (German residents aged 18 to 39).

8. The issue of misreporting illicit behavior is clearly relevant. Referring to the relevant literature is certainly a beneficial suggestion. This will be done in the revised version of the paper.

9. The lack of family characteristics after 1992 is a serious caveat of the survey in my opinion too. Yet, I am not sure whether estimating models that include family characteristics, i.e. only use the survey carried out in 1990 and 1992, can serve as a robustness check. The 1992-wave is the only one that includes family characteristics and covers both parts of the country. Thus, east-west-convergence can not be addressed by this alternative model.

10. Constructing synthetic cohorts is potentially a promising alternative to the original approach of the analysis. Yet, this would shift the paper somewhat a way from a purely descriptive analysis. In this case, we were interested in convergence of the behaviour of a specific cohort, i.e. in changes in consumption patterns over time, yet we would ignore that new individuals enter the market for illicit drugs. The original analysis captures both aspects. I expect to find less convergence if synthetic cohorts instead of fixed age groups are used. One may have different views about the more appropriate approach for measuring wet-east-convergence. Thus, one may think of reporting results for both variants in a revised version of the paper.