Authors’s Response
Female sex worker typology: too complicated to be used pragmatically
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We appreciate Haldar’s interest in our paper Devising a female sex work typology using data from Karnataka, India published in the IJE. We would like to clarify a few points in response to his letter. In our paper we classified female sex workers (FSWs) based on their reported main place of solicitation and main place of sex, and assessed which criterion best captured the variation in HIV risk observed among the sample studied. Our analysis suggests that in Karnataka state, classifying FSWs based on both the main place of solicitation and the main place of sex can best identify FSWs at high risk, and hence proposed that this typology is employed in HIV targeted interventions.

Our paper is based on secondary analysis of baseline data from the Integrated Biological and Behavioral Assessment (IBBA) surveys conducted among FSWs in Karnataka state with the primary purpose of measuring various programme outcomes. Haldar is correct to point out that the sample size statement we included was that which determined the size of the IBBA survey for its primary purpose. The sample size (over 2000 participants) is nevertheless substantial, has clearly allowed important differences and associations to be detected, and the precision of our findings is clear from the confidence intervals presented in Tables 4 and 5 in our paper.

Haldar’s main concern seems to be regarding the ‘definition’ of the FSW groups based on the place of solicitation and the place of sex. As part of the questionnaire, survey participants were asked to answer two questions measuring their main place of solicitation and their main place of sex: ‘Where do you generally solicit/pick up/get most of your clients?’ and ‘Where do you entertain most of your clients?’ Hence, survey participants were classified into categories by their main place of solicitation and/or their main place of sex by their answers to these two questions. These data have some limitations as the questions leave room for interpretation, in that they do not specify a time period or the type of clients referred to, and if the study participants changed their places of solicitation and sex in the recent past, they may find it difficult to answer. However, allocating participants to different categories based on their answers to these questions was straightforward. To avoid any doubt, the definition of place of solicitation used in our analyses is based on a direct question asked of the participants, and is not based on the information and operational definition that determined the sampling and which is presented by Haldar in Table 1.

Another concern raised by Haldar is confounding, and whether the differences seen between different places of sex and/or solicitation are ‘real’. Although we present the results of multiple logistic regression analyses in Tables 4 and 5 in our paper, in our view the issue of whether differences can be explained by confounding due to other factors measured (or not measured) in the IBBA is not central to the interpretation of our main findings. Our aim is to explore how the risk of HIV infection varies across different categories of place of sex and solicitation, to inform outreach activities, and while it is of interest to know how these differences arise, outreach activities can be planned based simply on the knowledge that certain women are at high risk.

The second half of the letter comments on the difficulties of taking into account the place of sex for ‘programmatic interventions’. While we agree that the main place of solicitation is easier to use in outreach compared with the main place of sex, our analysis shows that, in Karnataka, ignoring the second criterion results in considering street-to-lodge FSWs at medium rather than at high risk for HIV, which can have important implications for HIV transmission. Street-to-lodge FSWs can be reached specifically by targeting lodges operating in each locality, as Haldar acknowledges. In addition, outreach workers and peer educators can make additional efforts to find the places where street-based FSWs entertain their clients, and pay special attention to those who take their clients to lodges. Of note, in areas where programmes are mature, most peer educators and/or outreach workers have considerable knowledge about the women with whom they work on a daily basis, and
hence may already know of their places of sex. In such situations, the proposed typology of sex work could be introduced in current outreach efforts fairly easily, and attempts could be made to incorporate the main place of sex into pre-existing monitoring tools.

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**Instrumental variables in epidemiological research: an assessment of the adoption rate and future trends**

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In epidemiological research, the unavailability of possible confounders may lead to inconsistent estimates when a randomized experimental design is not feasible. A related challenge can be possible reverse causal orders between the dependent and the independent variables. This is a so-called endogeneity problem, which is well known in econometrics. However, if possible confounders are unavailable, or we suspect reverse causal orders, the appropriate use of instrumental variables (IVs) can generate consistent estimates. IVs have the properties that they are correlated with the explanatory variables and uncorrelated with the error term.

Here I map and compare the use of IVs in epidemiological and economics research over the last decades. My aim is to uncover what may cause the adoption of IVs in epidemiology. Is it related to previous adoptions in epidemiology or economics? If yes (or no), what may the implications be?

Figure 1 illustrates the relative use of the term ‘instrumental variable’ in the title, the abstract or the keywords on manuscripts indexed in the Social Science Citation Index within the subject areas economics and epidemiology (i.e. public, environmental and occupational health). I deleted Economics and Human Biology from the analyses since the journal is indexed within both subject areas. We observe an increasing trend of published items applying the term in economics, reaching a relative frequency of 1.05% in 2010. In epidemiology, the term first appeared in 1988, and at least since the mid-1990s we may observe a modest increasing trend.

To take account of the parameters’ trends, I model yearly change in the use of IVs in epidemiology, $\Delta y_t$, as $y_t - y_{t-1}$ (t is 1 year). I follow a similar procedure to model yearly change in economics, $\Delta x_t$. I apply $\Delta y_t$ as dependent variable. Unreported analyses show that both $\Delta y_{t-1}$ and $\Delta y_{t-2}$ are partially correlated with $\Delta y_t$, and I apply them as independent variables. Further unreported analyses show that among the parameters $\Delta x_t$, $\Delta x_{t-1}$, $\Delta x_{t-2}$, $\Delta x_{t-3}$ and $\Delta x_{t-4}$, only $\Delta x_{t-2}$ has an effect on $\Delta y_t$. Table 1 reports the results of $\Delta y_t$ regressed on $\Delta y_{t-1}$, $\Delta y_{t-2}$ and $\Delta x_{t-2}$.

The negative results of $\Delta y_{t-1}$ and $\Delta y_{t-2}$ indicate that the adoption rate of IVs in epidemiology varies around its trend within a time window of 2 years. Said differently, previous adoptions in epidemiology have no effect on later adoptions. The effect of $\Delta x_{t-2}$ is positive, which shows that a positive change in economics of a given year tends to have a positive effect on the change in epidemiology 2 years later. The variance inflation factors (VIFs) are low indicating no multi-collinearity problems. In an unreported model, I control for year trend, but this effect is low (Std. $\beta = 0.206; P = 0.145$) and the other parameters are substantially unchanged.

One possible explanation of $\Delta x_{t-2}$’s positive effect can be that methodological developments take place in economics, which are applied in epidemiology.

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**Figure 1** The relative adoption of IVs in economics and epidemiology