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Response: Commentary: Oil Palm Boom and Farm Household Diets in the Tropics

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A Commentary on

Commentary: Oil Palm Boom and Farm Household Diets in the Tropics
by Nurhasan, M., Pawera, L., Lo, M., Pratama, M. F., Rahmah, M., Utami, M. M. H., et al. (2020). Front. Sustain. Food Syst. 4:39. doi: 10.3389/fsufs.2020.00039

I thank Nurhasan et al. (2020) for their interest and comments on my recent paper, "Oil Palm Boom and Farm Household Diets in the Tropics (Sibhatu, 2019)." However, Nurhasan et al. (2020) selectively highlight some statements and disregard other crucial ones, misrepresent some of what I do, and misunderstand the approaches I use to evaluate technology adoption that is commonly used in the agricultural economics literature and the dietary measurements typically used in the context of developing countries. I address their comments below.

In their letter, Nurhasan and colleagues quote a statement from my paper, *"The results illustrate that land-use change through oil palm adoption significantly improves the diets of farm households in the tropics,"* and claim, *"the central conclusion [of my paper] is misleading and overgeneralized."* However, this selectively quoted statement is decidedly not the central conclusion of my paper. As I repeatedly state in the Abstract (p. 1), Results and Discussions (p. 11) and Conclusion (p. 12) sections of my article, the central message from my findings is this: *"Farm households in Jambi seem to adopt and expand land-uses that provide greater dietary benefits. Thus, researchers and policymakers interested in maintaining the tropical rainforests, regulating the existing and future oil palm plantations, and tackling nutritional problems in the study area should not overlook these dietary benefits for farm households."* In the Conclusion section (p. 12), I emphasize that *"The results here reflect the situation in Jambi and may not be generalized."* I also suggest that future studies should test and validate my findings with data from other regions and using different estimation techniques (p. 12). Thus, Nurhasan et al. (2020) appear to misrepresent the overall goals of my paper based on a narrow selection of quoted statements and disregarding crucial ones. Selective quoting a few remarks and ignoring other crucial ones within a given article is problematic and misleading for readers.

Nurhasan et al. (2020) also claim that my study *"overlooks the great diversity of non-oil palm adopters, including smallholders who were not previously engaged in any commercial plantation farming and households with customary (rather than formal) land rights, who practice diverse traditional food crop production systems across Indonesia."* However, as illustrated in Table 2 and explained in the Methodology section (p. 6) of my paper, on average more than 57% of the land owned by the sample respondents is actually under customary land rights. Claiming that my paper overlooks farmers with customary land rights is a clear misrepresentation of what I do and report in my paper. I would like to stress that the sample households are indeed relatively specialized, particularly compared with other smallholder farmers in developing countries (Sibhatu et al., 2015). Yet, some respondents cultivate neither oil palm nor rubber in the dataset that I use in my paper.

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I also clearly state in my article that some of the respondents supplement their production by growing horticultural crops, rearing livestock, cropping cereals like rice and maize, and practicing pisciculture (p. 3).

Nonetheless, I acknowledge that the households' inherent characteristics associated with customary land tenure systems in Jambi may be different than those found in other islands in Indonesia, such as Kalimantan, Sulawesi, or Papua. This would particularly influence the extent or capability of communities to appropriately adapt to the flow of market influence in different islands. The ability of communities in Jambi to adapt to a stronger market system brought by the oil palm and translate this opportunity for their welfare benefit might be different from those of communities in Kalimantan or Papua. In other words, the differences with customary land tenure could also partly explain why smallholders' participation in oil palm cultivation greatly varies in Indonesia. On the island of Sumatra, oil palm is mainly cultivated by smallholdings (smallholders manage 61% of the areas), while on Kalimantan large plantation companies are dominant (smallholders manage only 25% of the areas under oil palm) (BPS, 2019). Apparently, most of the adverse socioeconomic effects of oil palm are significantly observed in regions like Kalimantan, where large-plantation companies are dominant (Santika et al., 2019a). Therefore, the adverse socioeconomic effects associated with oil palm might be because of the nature of companies and institutional set-up than with the crop itself.

Nurhasan et al. also claim that the sample household represents a "*restricted sample of relatively specialized farmers in plantation crops, specialized rubber, oil palm, or both.*" And they argue that this is because only farm households who owned any agricultural land 5 or more years before the first survey round—of the panel dataset that I use in my paper—were considered during data identification in the study. In reality, accounting for those 5 years during data identification is a strength, not a limitation. When conducting socioeconomic impact assessment of a particular technology adoption with observational data, the standard approach is to take into account the time between adoption and use of this particular technology (Winters et al., 2011). In my study, this means that from the time a farmer plants oil palm saplings on her land for the first time until she starts earning income by selling oil palm fruit bunches. And oil palm typically takes about 4 years after planting to bear fruits that can be processed to produce vegetable oil (Corley and Tinker, 2016). Controlling the crop's 4 years yield-gap during data identification was in fact the only possible way to accurately observe and assess whether the crop's adoption has made an actual impact on the welfare of adopters through income pathway (p. 2, 3, 12). Moreover, I make explicit in my paper that Jambi province was chosen as a study area due to being globally known as a hot spot of oil palm production with significant smallholder farmers participation (Drescher et al., 2016). The sample respondents were randomly selected from the lowland areas of Jambi, covering all areas affected by oil palm expansion in recent decads (Drescher et al., 2016; Euler et al., 2016, 2017; Krishna et al., 2017). Further, I use robust analytical methods, including using a large number of outcome variables, employing an endogenous switching regression framework to control for selection bias and to deal with observable and unobservable factors, carrying out analysis with and without including transmigration households, and examining oil palm farmers who adopt the crop before and after 2012. Therefore, the best possible approaches were used to identify the study area and the respondents accurately (Drescher et al., 2016; Euler et al., 2016, 2017; Krishna et al., 2017), and I employ robust analytical methods to avoid possible biases in this kind of study. Still, diverse livelihood systems indeed occur across Indonesia, and this may limit the extrapolation of the current inference and conclusion from Jambi to other systems. That is why, in the Conclusion section of my paper, I caution not to overgeneralize my findings.

Concerning the claim, "*improvement to economic benefits from oil palm development across Indonesia has been widely documented.*" Indeed, as I summarize in my paper, several studies have already documented not only improvements in economic benefits but also negative ecological and socioeconomic consequences of oil palm expansion in Indonesia (p. 2). Yet, using a unique panel dataset and advanced analytical tools, for the first time, my analysis provides evidence that oil palm adoption leads to greater dietary benefits in farm households in Jambi. This important finding broadly corroborates with studies that reveal positive socioeconomic benefits of oil palm adoption at regional-level (e.g., Edwards, 2019) and micro-level (e.g., Dewi et al., 2005; Euler et al., 2017; Krishna et al., 2017; Kubitza et al., 2018), thus strengthens the implications of my study.

Nurhasan et al. argue that previous studies have reported mixed effects (divergent outcomes) of adopting oil palm on community welfare in Indonesia. In particular, they emphasize that oil palm adoption benefited most to villages with better access to markets and to those that had prior exposure to handling and marketing of a commercial crop such as rubber. Those findings are not surprising and not unique to oil palm, especially if one takes a cursory look at the history of the adoption of agricultural innovations (Feder et al., 1985; Feder and Umali, 1993; Rogers, 2003). Farmers who already are information-seeking, have better know-how, have better access to land and capital, reside in conducive agroclimatic environments, have better access to improved rural infrastructure and markets often resource themselves to benefit from innovation adoption. Conversely, farmers who lack such characteristics, but adopt an innovation through immense pressure from governments, companies, or other national and international organizations, such innovation adoption causes more damage than benefit. This dualistic pattern of agricultural technology adoption has been long studied and known since the 1950s, inspired by the influential "diffusion of innovation" theory of Everett M. Rogers (Rogers, 2003). Recent studies by Santika and her colleagues have elegantly captured this dualistic pattern of agricultural technology adoption in oil palm (Santika et al., 2019a, Santika et al., 2019b). Therefore, it is not oil palm *per se*, but policymakers, governments, companies, and responsible organizations that should ensure appropriate innovation adoption and provide a range of alternative technologies to farmers who are less likely to benefit and/or when environmental tradeoffs are wide. Nurhasan et al.

are right that access to better markets and infrastructure is vital to improve welfare outcomes. And I fully agree that providing improved rural infrastructure and markets, extension services, off-farm work, and a range of alternative technologies are crucial to enhance rural households' welfare, not only in Indonesia but also worldwide.

Nurhasan et al. (2020) also argue that my study should have considered "*the risk of overnutrition*" simply based on the quantities of calories consumed at the household level. I agree that obesity and non-communicable diseases associated with overnutrition are now global pandemics, while undernutrition and related infectious disease are still widespread problems (Demmler et al., 2017; Swinburn et al., 2019). However, examining overnutrition and associated dietary risks is not that simple as Nurhasan et al. put it. Besides quantities of calories consumed at the household level, several interlinked factors also strongly influence the dietary and health risks of overnutrition. For example, factors in a person's environment, physical activity, age, sex, health conditions, use of certain medications, and genetics are some of the crucial factors that may determine overnutrition and the resultant negative health outcomes (Demmler et al., 2017). However, examining those factors is beyond the scope of my paper.

I thank Narhasan and colleagues for recognizing that the adjustment I made on food categorization could be appropriate to the study area. However, they also argue that I should have provided "*solid justification in implementing non-standard or modified dietary diversity for comparison purpose*." This is what I have exactly done in my paper, even if expressed in a few words. In the Methodology section (p. 3, 4), I explain that I calculate food groups based on the Minimum Dietary Diversity for Women that contribute strongly to micronutrient adequacy (Martin-Prével et al., 2015; FAO and FHI 360, 2016). I also justify, "*Food groups that have little or undesirable nutritional and health effects when consumed in large quantities are excluded (sugars and sweets, oils and fats, and condiments) from HDDS [household dietary diversity scores]*." Excluding food groups that indicate more of economic access than dietary quality and diversity (Swindale and Bilinsky, 2006) is a commonly used technique in the literature (Jones, 2017). Indeed, Verger et al. (2019) put those studies that exclude such food groups from HDDS under "*correct[ly]*" conducted studies. The most recent studies also reveal that household-level dietary measurements are strongly correlated with individual-level measurements (Minimum Dietary Diversity for Women (MDD-W) and Women Dietary Diversity Score (WDDS), thus can be used to draw conclusions about quality and adequacy of diets (Fongar et al., 2019). After all, as I mention in my paper, a woman can access and consume only foods available in her household (p. 12). Furthermore, in my study, I use in total 9 dietary indicators as outcome variables see Table 2 of my paper. The positive dietary effects observed in my analysis are captured not only by the HDDS but also by the other 8 indicators, which strengthens the robustness and inter-study comparability of my findings. All respondents (100%) in my dataset reported consuming the excluded food groups, which means that no variation was observed in the entire sample. Therefore, I do not see how my results might differ by including those food groups in the HDDS.

Nurhasan et al. state that *"this study uses unvalidated methods to measure dietary adequacy and states that: food waste and foods consumed outside the home are not included in the calculation of the dietary outcomes."* Unfortunately, Nurhasan et al. do not provide citations to support this claim, and this complicates the justification of what is and is not validated within the dietary indicators for observational data collected in the context of developing countries. To the best of my knowledge, despite the notable progress in developing dietary metrics (INDDEX Project, 2018), there are currently no measures (particularly at household level) that validate food waste and foods consumed outside of home (as against foods from own production, purchases, and gift). In developing countries context, the standard method to collect food consumption information is by interviewing a person responsible for preparing and purchasing food (Kennedy et al., 2011; FAO and FHI 360, 2016; Gibson et al., 2017; Fongar et al., 2019). Interviews are sometimes assisted by displaying food containers and plates; the interviewee is often an adult female member of a household. Unfortunately, this interviewee (as well as the household member eating outside of the home, who usually is an adult male member) has limited information regarding the ingredients, weight, and preparation mechanisms of foods consumed outside home (Gibson et al., 2017). Therefore, tailoring the existing dietary measures to capture the quantity and composition of foods consumed outside home accurately is subject to improvement and validation through future studies. Regarding food waste, none of the existing household or individual level dietary indicators considers and validates uneaten leftover food waste in the measurement. Apart from taking into account for the preparation methods for some foods, questions related to food waste and leftovers are not even included in the questionnaires behind the currently validated dietary indicators. Therefore, the criticism for not using dietary indicators that validate food waste raised by Nurhasan et al. (2020) is simply irrelevant for my study.

To end, Nurhasan et al. argue that the "*tradeoff, as opposed to the comparison between different cash crops should be the central research question*." I echo the sentiment regarding the need to boost research on the economic, social, and environmental tradeoffs of rapid land-use change in the tropics. My paper contributes to this direction by discovering important knowledge that farm households in Jambi seem to adopt and expand land-uses that provide greater dietary benefits. This is indeed vital information for researchers and policymakers interested in maintaining the tropical rainforests, regulating the existing and future oil palm plantations, and tackling nutritional problems in the study area. Moreover, as part of an interdisciplinary German-Indonesian Collaborative Research Centre (Drescher et al., 2016), my colleagues and I are actively investigating the tradeoffs between farmers' wellbeing and ecological consequences of rapid land-use change in Indonesia (e.g., Clough et al., 2016; Euler et al., 2017; Kubitza et al., 2018; Darras et al., 2019; Grass et al., 2020; Qaim et al., 2020). I reemphasize that Jambi might be a particular case given that Indonesia is such a massive country with different development stages occurring in many parts of the archipelago and communities with diverse ethnic features and cultural values (with different propensities and outlooks toward development and modernity). Some inferences or conclusions derived from

Jambi may be transferable to other parts of Indonesia (and other tropical areas in Asia, Africa, and the Americas), and some may not, and readers should interpret my findings having in mind the common caveats of local household-level studies. Finally, I hope that our studies motivate further research on how to improve farm households' livelihoods while keeping the remaining tropical rainforests intact.

AUTHOR CONTRIBUTIONS

The author confirms being the sole contributor of this work and has approved it for publication.

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