

Review of: "A nicotinic hypothesis for Covid-19 with preventive and therapeutic implications"

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There are a lot of bias in literature especially for reporting references 40-48. These references show smokers and nonsmokers didn't differ significantly in covid19 prevalence or show that smokers are significantly more susceptible to covid-19 progression and transmission. There is a reference that shows ACE-2 is expressed in lungs based on histological stainings. Authors of the manuscript asserted that prevalence of covid19 in smokers is low while it is relative to other comorbidities. In fact many studies showed that smokers are more prone to covid19 progression and transmission. In this manuscript opponent viewpoints are rare and side effects of nicotine like withdrawal and dependence is not considered. Authors did not hedge in conclusion they used legitimate word while a lot of studies including In Silico, In Vitro, In Vivo needed and at least three phases of clinical trials should be done to accept a standard intervention. In only one reference smokers were low and they were medical staff and probably have more availability and awareness to higiene and treatments and a stigma exists for them in treatment environment and jobs. Authors did not mention references of some sentences and interpreted other reports in favor of their aim or in a wrong way while the fact was something else. There were many references that could reject at least some claims and basics in this manuscript. Its enough to see tables in references 40-48 about comparisons between smokers and non-smokers. A letter to editor is sendable about problems in this manuscript and other misjudgements about smoking. According to the special status of the worldwide its better to be very cautios about these suggestions. Addiction neurobiologists and proffessionals in addiction and Clinicians are facing a pseudoscience knowadays that threatean people. Unknown mechanisms exist and maybe in future authors can suggest a better way with more pros and less cons.