Since January 2020 Elsevier has created a COVID-19 resource centre with free information in English and Mandarin on the novel coronavirus COVID-19. The COVID-19 resource centre is hosted on Elsevier Connect, the company's public news and information website.

utilisation of physical rehabilitation health-care resources. In our view, critical illness myopathy might be the most likely explanation for this previously unrecognised, important finding.

JR reports honoraria from Alberta Psychiatric Association and has attended an advisory meeting with Promentis Pharmaceuticals, outside of the submitted work. DN is principal investigator on an NIH-funded randomised trial evaluating nutrition and exercise in acute respiratory failure and, related to this trial, is currently in receipt of donated amino acid product from Baxter Healthcare Corporation and an equipment loan from Reck Medical Devices, outside of the submitted work. All other authors declare no competing interests.

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Authors' reply

We thank Elizabeth Charlton and colleagues, Josef Finsterer, and Ella Burchill and colleagues for their comments on our Article in *The Lancet Psychiatry*.[1]

	Incidence within 6 months after COVID-19	Incidence within 6 months after influenza	Hazard ratio	p value
Alzheimer's disease (G30)	0·071% (0·050–0·10)	0·036% (0·025–0·054)	2·19 (1·29–3·70)	0·0029
Vascular dementia (F01)	0·063% (0·042–0·094)	0·041% (0·029–0·060)	1·59 (0·94–2·70)	0·082
Dementia in other diseases classified elsewhere (F02)	0·11% (0·081–0·15)	0·055% (0·040–0·076)	2·11 (1·37–3·23)	0·0005
Unspecified dementia (F03)	0·25% (0·20–0·31)	0·12% (0·094–0·15)	2·04 (1·52–2·75)	<0·0001

Data in parentheses are 95% CIs. Dementia subtypes are presented with their ICD-10 codes. The sum of incidences exceeds the total incidence of dementia because the same patient might be diagnosed with one subtype (eg, unspecified dementia) and then another (eg, Alzheimer's disease) within the follow-up period. No data can be shown for frontotemporal dementia and Lewy body dementia because they occurred in fewer than ten patients in each cohort (which is the minimum number to be returned by TriNetX to safeguard patients' anonymity).

Table: **Incidence and hazard ratio for dementia subtypes between matched cohorts of patients diagnosed with COVID-19 versus influenza**

Charlton and colleagues raise several interesting points. Regarding post-traumatic stress disorder (PTSD), we did not explore this specific diagnosis, although we did in an earlier Article.[2] We have now done so, extending the window for the index event to April 20, 2021. The risk of a first diagnosis of PTSD within 6 months of a COVID-19 diagnosis was 0·58% (95% CI 0·50–0·67). This risk was significantly higher than in the matched cohort of patients diagnosed with influenza (0·26% [0·23–0·31]; hazard ratio [HR] 2·12 [95% CI 1·74–2·59]; p<0·0001). Patients with COVID-19 requiring admission to an intensive care unit (ICU) were at a higher risk of PTSD than a matched cohort of patients with COVID-19 not requiring admission to an ICU (1·02% [95% CI 0·78–1·33] *vs* 0·20% [0·12–0·35]; HR 4·55 [95% CI 2·59–7·98]; p<0·0001).

Using the same matched cohorts of patients with COVID-19 and with influenza diagnosed between Jan 20, 2020, and April 20, 2021, we also investigated the incidence and HRs for subtypes of dementia (table). The majority of diagnoses were of unspecified dementia but the relative increase was broadly similar across categories. We did not exclude people with a history of mild cognitive impairment or delirium, and therefore some patients diagnosed with dementia might have been in this high-risk or prodromal group, as we noted in the Discussion of our Article.[1]

We have no data as to which of the COVID-19 cases had been asymptomatic, but we assume that this group is substantially under-represented in our dataset because there is a bias towards symptomatic people presenting for testing (especially early in the pandemic), and because we used the U07.1 ICD-10 code to define cases, which refers to a confirmed diagnosis. Asymptomatic COVID-19 might well be associated with lower rates of subsequent psychiatric or neurological disorder, and our results should be interpreted with this important possibility in mind. We agree that asking about COVID-19 should become a routine item in medical history questionnaires. The idea of reverse redeployment will be attractive to mental health professionals but we suspect rather less so to our general medical colleagues.

Josef Finsterer commented on the overlap between the influenza and respiratory infections cohorts. We agree that we could have made them mutually exclusive; however, we chose not to do this to enable the respiratory infection cohort to be sufficiently large to enable all the COVID-19 cases to be included after propensity score matching. Our study was observational, and we did not attempt to list or explore all the potential mechanisms that might be involved. For instance, we did not investigate the list of putatively neurotoxic compounds that some

patients might have received, since a comprehensive pharmacoepidemiological assessment was beyond the scope of the study. Similarly, we could have put various diagnostic combinations together, but we chose to present Guillain-Barré syndrome separately because of the previous suggestions of a specific association with COVID-19.[3]

We agree that undiagnosed COVID-19 in the control cohorts will have occurred, and we mention this and its implications in the Discussion of our Article.[1] To expect control cohorts in a real-world electronic health records study to be based on systematic negative PCR test data would be unrealistic. Finally, we acknowledge that we did not attempt to include every neurological syndrome. We have subsequently reported on cerebral venous thrombosis[4] and will be studying headache and some of the other diagnoses in future analyses.

Ella Burchill and colleagues rightly draw attention to the salient finding regarding myoneural junction and muscle disorders. They are correct that most diagnoses in this category were to G72.8 rather than to myasthenia gravis or other specific diagnoses, and we agree that critical illness-associated neuropathy and myopathy are indeed plausible explanations. We also agree that neuromuscular complications of COVID-19 merit attention both for research and rehabilitation.

SL is an employee of TriNetX. All other authors declare no competing interests.

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Long-acting injectable versus oral antipsychotics for schizophrenia

In their systematic review of long-acting injectable (LAI) versus oral antipsychotics, Taishiro Kishimoto and colleagues included randomised trials, cohort studies, and pre–post studies.[1] On the basis of their findings, which included a lower risk of hospitalisation or relapse (their primary outcome) with LAIs than with oral antipsychotics, they suggested that increased use of LAIs could improve outcomes in schizophrenia. I would like to comment on some methodological issues.

First, because randomised trials are more likely to provide unbiased estimates of the effects of interventions than are other study designs, they should generally be preferred over non-randomised studies, such as some of the studies included by Kishimoto and colleagues, when synthesising the evidence to guide appropriate patient care.[2] The validity of non-randomised studies, and hence syntheses including them, such as those done in this review, is inherently threatened by biases, especially confounding and selection bias.[2,3] On the basis of the randomised trials alone (27 studies, 7407 participants), the authors found no significant difference in relapse rates between participants receiving LAIs and those receiving oral antipsychotics—an outcome that is arguably more relevant to patients than the composite primary outcome of hospitalisation or relapse.

Second, Kishimoto and colleagues assessed the risk of bias of the randomised trials, but it was not clear for which outcome this assessment was made, and they did not make an overall risk-of-bias judgment for each trial. They judged 24 (75%) of the 32 trials to be at high risk of bias for at least one domain and 30 (94%) were judged as either at high risk of bias in at least one domain or at unclear risk of bias for multiple domains. Using the judgments by Kishimoto and colleagues, and following guidance in the *Cochrane Handbook for Systematic Reviews of Interventions* for reaching an overall risk-of-bias judgment,[4] 30 (94%) of the included randomised trials should potentially be judged as having an overall high risk of bias.

Third, the authors did not assess the certainty of the evidence for any outcomes. On the basis of the randomised trials alone and assuming that their risk-of-bias assessment applied to the outcome of relapse, the certainty of the evidence for that outcome using the GRADE framework[5] should arguably be rated as very low, due to downgrading for risk of bias, heterogeneity (I^2=54·5), and indirectness, since the antipsychotics differed between the LAI and oral antipsychotic groups in most included studies. A similar rating would probably apply to the primary efficacy outcomes. The certainty of the evidence from non-randomised studies is, if possible, likely to be rated even lower.

Taken together, the evidence presented by Kishimoto and colleagues[1] does not appear to support conclusions regarding whether outcomes in schizophrenia would improve by increased use of LAIs or not.

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