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.

use of girls as young as 7 years as suicide bombers.[3] The effects of the COVID-19 pandemic have been far-reaching, but its impact on the health and wellbeing of young girls in areas of conflict and political instability deserves focused, urgent attention.

PHW was a contributor to the *Lancet* Series on Women's and Children's Health in Conflict Settings. We declare no other competing interests.

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Time to integrate congenital CMV testing into hearing screening for newborn babies

We congratulate the GBD Hearing Loss Collaborators for highlighting the magnitude of hearing loss as an important public health problem.[1] Early detection of hearing loss in children can substantially improve their academic performance.[2] Congenital cytomegalovirus (CMV) is the most common non-genetic and the only potentially treatable cause of sensorineural hearing loss; globally, it alone accounts for approximately 20% of moderate to profound bilateral sensorineural hearing loss in children.[3]

Diagnosis of congenital CMV in the first 3 weeks of life and starting treatment with oral valganciclovir within the first month will reduce the risk of hearing loss caused by this infection.[4] In the absence of any screening programme, the great majority of newborn babies with sensorineural hearing loss related to congenital CMV are missed at birth.[5] The diagnosis is often delayed into early childhood, by which time the condition is likely to have progressed and antiviral treatment has not been shown to be effective.

We fully agree with the Article authors' assertion that urgent attention is required to improve newborn babies' hearing screening programmes. It is now time for policy makers to optimise this pathway and to begin testing for congenital CMV in those who do not pass their newborn baby hearing screen. Addressing this condition will have immediate benefits for affected infants by improving developmental outcomes and to wider society by increasing productivity and minimising the health-care burden.

We declare no competing interests.

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Data discrepancies and substandard reporting of interim data of Sputnik V phase 3 trial

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Restricted access to data hampers trust in research. Access to data underpinning study findings is imperative to check and confirm the findings claimed. It is even more serious if there are apparent errors and numerical inconsistencies in the statistics and results presented. Regrettably, this seems to be what is happening in the case of the Sputnik V phase 3 trial.[1]

Several experts[3,4] found problematic data in the published phase 1/2 results.[2] We have made multiple independent requests for access to the raw dataset, but these were never answered. Despite publicly denying some problems, formal corrections were made to the Article,[2] thus addressing some concerns.[5] Notwithstanding the previous issues and lack of transparency, the interim results from the phase 3 trial of the Sputnik V vaccine[1] again raise serious concerns.

We have a serious concern regarding the availability of the data from which the investigators draw their conclusions. The investigators state that data will not be shared before the trial is completed, and then only by approval of stakeholders, including a so-called security department. Data sharing is one of the cornerstones of research integrity; it should not be conditional and should follow the FAIR principles.

The second concern pertains to the trial protocol, as already described in an open letter by the Russian Society for Evidence-Based Medicine.[3] The Sputnik V investigators mention that three interim analyses were added to the study on Nov 5, 2020,[1] but this change was not recorded on ClinicalTrials.gov (NCT04530396). Unfortunately, the full study protocol has not been made publicly available, so the rationale behind this change or the type I error rate adjustment, if


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For the **FAIR principles** see https://www.go-fair.org/fair-principles/

any, is not known. According to the ClinicalTrials.gov record NCT04530396, the primary outcome was changed on Sept 17, 2020. Initially, the primary outcome was to be assessed after the first dose, but the evaluation was postponed to after the second dose. The presented primary result (efficacy of 91·6%) is dependent on this change, but the reasons for the change have not been made public. Moreover, the latest ClinicalTrials.gov record (Jan 22, 2021) defines the primary outcome inconsistently: "Primary Outcome Measures: percentage of trial subjects…after the first dose…based on the percentage…after the second dose".

Besides these protocol amendments, the definition of the primary outcome is unclear in the Article,[1] where it says that when COVID-19 was suspected, participants were assessed with "COVID-19 diagnostic protocols, including PCR testing". Here, we lack some crucial information, such as the clinical parameters determining suspected COVID-19, what diagnostic protocols were used, when the PCR testing was done, what specific method was used, or how many amplification cycles were used. The way cases of suspected COVID-19 were defined could have led to bias in PCR testing used to assess the number of confirmed COVID-19 cases, which is crucial for the efficacy determination.

A final point of concern about the study protocol relates to the enrolment and randomisation of patients. According to the trial profile in figure 1 of the Article,[1] 35 963 individuals were screened and 21 977 individuals were randomised. The ClinicalTrials.gov record for NCT04530396 (Jan 20, 2021) mentions that 33 758 patients were enrolled. We would expect that this last figure should be equal to either the number of participants screened or randomised. Moreover, there is no information about what caused the exclusion of 13 986 participants, as per the trial profile.

The third concern relates to the data reported and numerical results. We found the following data inconsistencies: (1) in figure 2 of the Article,[1] data for the vaccinated group on day 20 refer to more individuals than at day 10, as if there was either information missing for 100 participants at day 10, or participants were enrolled after day 10 (figure 2 was formally corrected on Feb 20, 2021, but the correction statement did not state the reasons leading to such correction); and (2) in table S1 of the appendix,[1] the number of participants reported for the different vaccinated age cohorts do not add up to the reported total (n=338 *vs* n=342). With such inconsistencies, we question the accuracy of the reported data.

A very peculiar result of the major subgroup analysis of the primary outcome caught our attention. The vaccine efficacy was said to be high for all age groups. The reported percentages were 91·9% in the 18–30-year age group, 90·0% in the 31–40-year age group, 91·3% in the 41–50-year age group, 92·7% in the 51–60-year age group, and 91·8% in participants older than 60 years. We checked the homogeneity of vaccine efficacy across age groups (interaction tests): the p value of the Tarone-adjusted Breslow-Day test was 0·9963, and the p value of a non-asymptotic test was 0·9956,[6] indicating a very low probability of observing a homogeneity this good if the actual homogeneity is perfect. By applying 18 other homogeneity tests (six in table 1, seven in table S6, six in table 2 of the Article[1]), we could not find other major abnormality in the overall distribution of p values (appendix).

We also found some highly coincidental results reported in table S3 of the appendix. In particular, two upper confidence limit values for two different distributions (placebo group at baseline for unstimulated and antigen-stimulated measures) both equal 0·708. Of course, this is possible, but we call once more for access to the data from which the statistics originate for close scrutiny.

In line with our earlier concerns with the phase 1/2 results[4] and the substandard reporting of the phase 3 interim results,[1] we invite the investigators once more to make publicly available the data on which their analyses rely. Access to the protocol, its amendments, and the individual patient records is paramount, as much for clarification as for open discussion of all the issues.

We also invite the Editors of *The Lancet* to clarify the consequences of further denying access to the data needed for assessing the results presented, should the authors still deny it.

EMB is the owner of Resis Srl. All other authors declare no competing interests. Code to test the homogeneity of vaccine efficacy across age groups is available on the Open Science Framework, https://osf.io/sudxe/

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1 Logunov DY, Dolzhikova IV, Shcheblyakov DV, et al. Safety and efficacy of an rAd26 and rAd5 vector-based heterologous prime-boost COVID-19 vaccine: an interim analysis of a randomised controlled phase 3 trial in Russia. *Lancet* 2021; **397:** 671–81.

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See Online for appendix

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Authors' reply

Clear and transparent regulatory standards exist for provision of clinical trial data, including data reported in clinical study reports that are considered sufficient for regulatory review and approvals. The reporting of the interim analysis[1] in the phase 3 Sputnik V clinical trial fully complies with those standards. It is on this basis that Sputnik V has received registration in 51 countries, which confirms our full transparency and compliance with regulatory requirements.

The amendment was made to the protocol on Nov 5, 2020. The complete protocol, as amended (Section 10.4 Interim Analysis and Statistical Significance Level Applied), was submitted to *The Lancet* along with the rest of the documents for review.

Efficacy is assessed within 6 months after first dose (time of observation of study participants); however, the calculation of the primary outcome is based on the number of cases of COVID-19 in participants who received both doses (after second dose), as indicated in the protocol. This is consistent with the primary outcome of other studies.

The registration scheme for COVID-19 cases is described in the Article (p 674).[1] When COVID-19 was suspected, participants were assessed according to COVID-19 diagnostic protocols, including PCR testing at a central laboratory in Moscow, Russia. Severity of disease was established upon confirmation of the COVID-19 diagnosis by site investigators. A description of the assessment criteria for severity of COVID-19 is available in the appendix of the Article.[1] Thus, we have described the clinical parameters to determine COVID-19. PCR testing was done in hospitals using test systems registered in Russia.[1] It seems strange to ask about the number of amplification cycles when performing PCR on a registered test system.

Enrico Bucci and colleagues correctly note that 21 977 individuals were included in the study, as of Nov 24, 2020, as shown in figure 1 of the Article.[1] The ClinicalTrials.gov record states that as of Jan 20, 2021, the number of participants increased to 33 758. 13 986 individuals were indeed excluded; some of the volunteers were screened and had not yet been randomised at the time of the snapshot, and others were excluded according to the exclusion criteria or did not meet the inclusion criteria.

Numerical inconsistencies were simple typing errors that were formally corrected.

We provide data on the number of cases, sample sizes, efficiency values, confidence intervals, and significance level for each age group in the Article.[1] According to the above formula for calculating the efficiency and the method for calculating the confidence interval, readers can calculate and confirm that the efficiency values are the same as shown in table 2.[1] The homogeneity of the values only confirms the fact that, as described in the Article, the effectiveness of the vaccine does not differ between age groups. In this case, the main parameter by which one can judge the difference in effectiveness is the confidence interval, the differences in which are quite significant due to the different sample sizes and the number of COVID-19 cases at the time of analysis.

With regard to the data on the upper limit of the confidence interval in the placebo group in table S3 of the appendix, we confirm the data shown are correct.

It is important to note that the safety and immunogenicity of the Sputnik V vaccine has been confirmed by researchers in Argentina, where the vaccination with Sputnik V began. Preliminary data[2] show the vaccine has an appropriate safety profile, and the most common adverse events were pain at the injection site, fever, and muscle pain. A study of immunogenicity showed 16 titres of neutralising antibodies to SARS-CoV-2 after the first dose and 64 titres after the second dose, which correlates with the published results from vaccine clinical trial phase 1/2 and phase 3 in Russia.[1,3] Unfortunately, due to the use of different ELISA kits, it is not possible to compare the specific IgG concentrations in these studies. However, it should be noted that a specific humoral response was detected in all vaccinated participants.

An important detail in the report from Argentina[4] is the observation that vaccination of people with a history of COVID-19 leads to a quick and significant increase in antibodies after a single dose of vaccine.

Thus, to date, the safety and immunogenicity of the Sputnik V vaccine has been confirmed in multiple studies.

We declare patents for an immunobiological expression vector, pharmaceutical agent, and its method of use to prevent COVID-19.

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