Bebtelovimab for COVID-19: real-time meta-analysis of 6 studies

@CovidAnalysis, November 2026, Version 16 (V1 2022), c19early.org/btmeta.html

Abstract

Meta-analysis using the most serious outcome reported shows 34% [-24-65%] lower risk, without reaching statistical significance. Results are similar for peer-reviewed studies and worse for Randomized Controlled Trials.

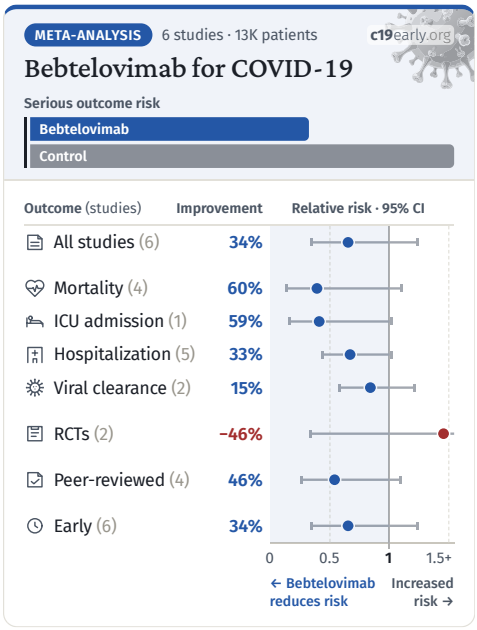
2 studies from 2 independent teams (both from the same country) show significant benefit.

Currently there is limited data, with only 34 control events for the most serious outcome in trials to date.

Efficacy is variant dependent. *In Vitro* research suggests a lack of efficacy for omicron BQ.1.1 [1], BA.5, BA.2.75, XBB, XBB.1.5, XMM.1.9.1 [2,3]. mAb use may create new variants that spread globally [4-6], and may be associated with increased risk of autoimmune disease [7], prolonged viral loads, clinical deterioration, and immune escape [5,8-12].

Prescription treatments have been preferentially used by patients at lower risk [13]. Retrospective studies may overestimate efficacy, for example patients with greater knowledge of effective treatments may be more likely to access prescription treatments but result in confounding because they are also more likely to use known beneficial non-prescription treatments.

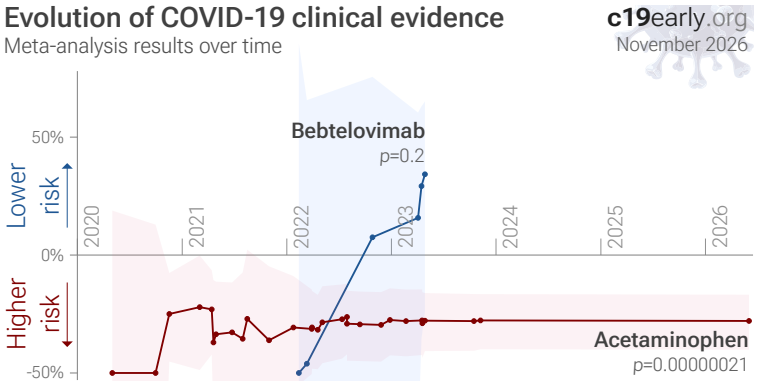
No treatment is 100% effective. Protocols combine safe and effective options with individual risk/benefit analysis and monitoring. Other treatments are more effective. All data and sources to reproduce this analysis are in the appendix.



Bebtelovimab for COVID-19

HIGHLIGHTS

- Bebtelovimab **reduced risk** with low confidence for mortality, ICU admission, and hospitalization, and very low confidence for pooled analysis.
- However **increased risk** is seen with low confidence for progression.
- **Efficacy is variant dependent.** Bebtelovimab may have **reduced or no activity** for recent variants.
- Real-time updates and corrections with a consistent protocol for 227 treatments. Outcome specific analysis and combined evidence from all studies including treatment delay, a primary confounding factor.



Introduction

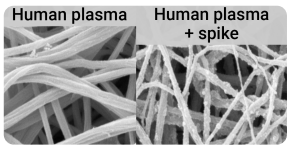


Fig. 1. SARS-CoV-2 spike protein fibrin binding leads to thromboinflammation and neuropathology, from [14].

Immediate treatment recommended

SARS-CoV-2 infection primarily begins in the upper respiratory tract and may progress to the lower respiratory tract, other tissues, and the nervous and cardiovascular systems, which may lead to cytokine storm, pneumonia, ARDS, neurological injury [15-31] and cognitive deficits [18,23], cardiovascular complications [32-38], DNA damage [39-42], organ failure, and death. Even mild untreated infections may result in persistent cognitive deficits [43]—the spike protein binds to fibrin leading to fibrinolysis-resistant blood clots, thromboinflammation, and neuropathology. Minimizing replication as early as possible is recommended.

Many treatments are expected to modulate infection

SARS-CoV-2 infection and replication involves the complex interplay of 500+ host and viral proteins and other factors [A,44-51], providing many therapeutic targets for which many existing compounds have known activity. Scientists have predicted that over 12,000 compounds may reduce COVID-19 risk [52], either by directly minimizing infection or replication, by supporting immune system function, or by minimizing secondary complications.

Monoclonal antibodies

Bebtelovimab is a monoclonal antibody (mAb). mAbs are laboratory-engineered proteins designed to mimic the immune system's ability to fight pathogens. In the context of COVID-19, mAbs typically target specific regions of the SARS-CoV-2 spike protein, inhibiting viral entry into human cells and neutralizing the virus. These antibodies are derived from the B cells of recovered patients or immunized animals and are produced in large quantities using recombinant DNA technology and cell culture methods.

Analysis

We analyze all significant controlled studies of bebtelovimab for COVID-19. Search methods, inclusion criteria, effect extraction criteria (more serious outcomes have priority), all individual study data, PRISMA answers, and statistical methods are detailed in Appendix 1. We present random-effects meta-analysis results for all studies, individual outcomes, peer-reviewed studies, and Randomized Controlled Trials (RCTs).

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Treatment timing

Fig. 2 shows stages of possible treatment for COVID-19. Prophylaxis refers to regularly taking medication before becoming sick, in order to prevent or minimize infection. Early treatment refers to treatment immediately or soon after symptoms appear, while late treatment refers to more delayed treatment. Currently all bebtelovimab studies use early treatment.

Variant Dependence

Extensive mutations in SARS-CoV-2 have resulted in variants that evade neutralizing antibodies from monoclonal antibody treatments[53,54], resulting in efficacy that is highly variant dependent. For example, *in vitro* research suggests that bebtelovimab is not effective for omicron BQ.1.1[1]. Table 1 shows efficacy by variant for several monoclonal antibodies. This table covers earlier SARS-CoV-2 variants and has not been updated for more recent variants.

Results

Table 2 summarizes the results for all studies, for Randomized Controlled Trials, for peer-reviewed studies, and for specific outcomes. Fig. 3 shows a timeline of the results in bebtelovimab studies. Fig. 4, 5, 6, 7, 8, 9, and 10 show forest plots for random-effects meta-analysis of all studies with pooled effects, mortality results, ICU admission, hospitalization, progression, viral clearance, and peer reviewed studies.

	Bamlanivimab/ etesevimab	*Casirivimab/ imdevimab*	*Sotrovimab*	*Bebtelovimab*	*Tixagevimab/ cilgavimab*
Alpha B.1.1.7					
Beta/ Gamma BA1.351/ P.1					
Delta B.1.617.2					
Omicron BA.1/ BA.1.1					
Omicron BA.2					
Omicron BA.5					
Omicron BA.4.6					
Omicron BQ.1.1					

Table 1. Predicted efficacy by variant from *Davis et al.* (not updated for more recent variants). : likely effective : likely ineffective : unknown. Submit updates.

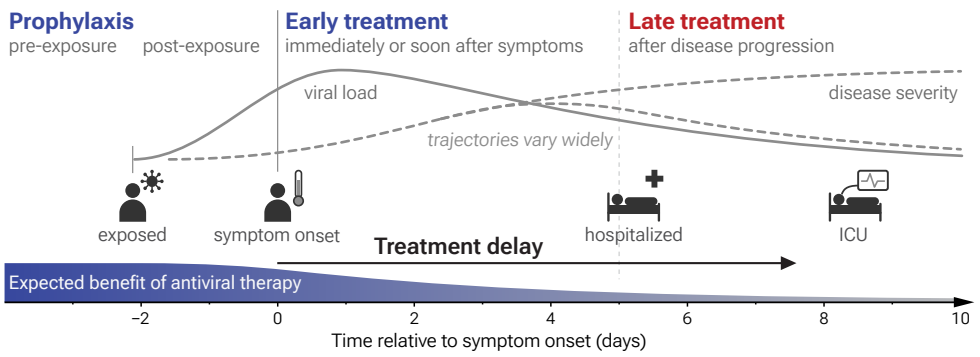

Fig. 2. Treatment stages.

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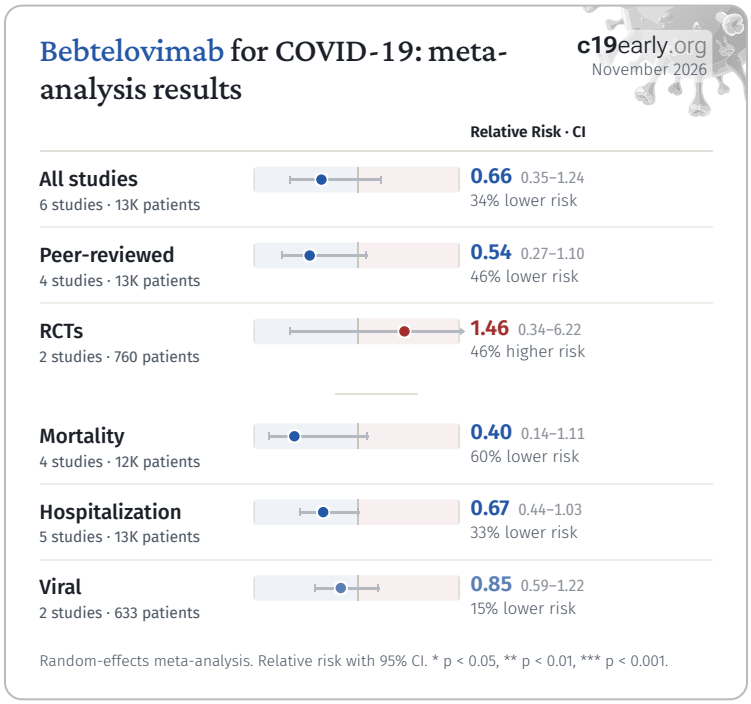

Table 2. **Random-effects meta-analysis results for bebtelovimab.** Results show the relative risk and 95% confidence interval.

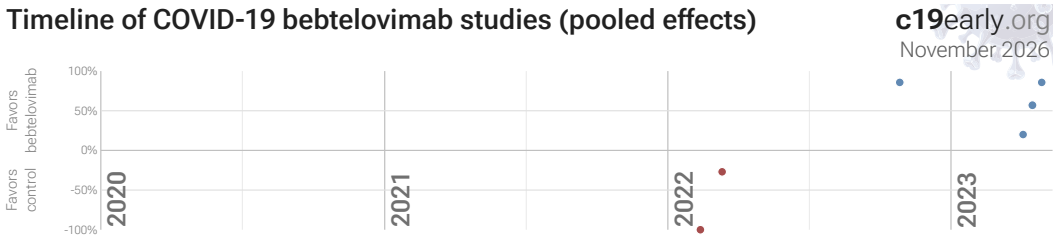

Fig. 3. Timeline of results in bebtelovimab studies.

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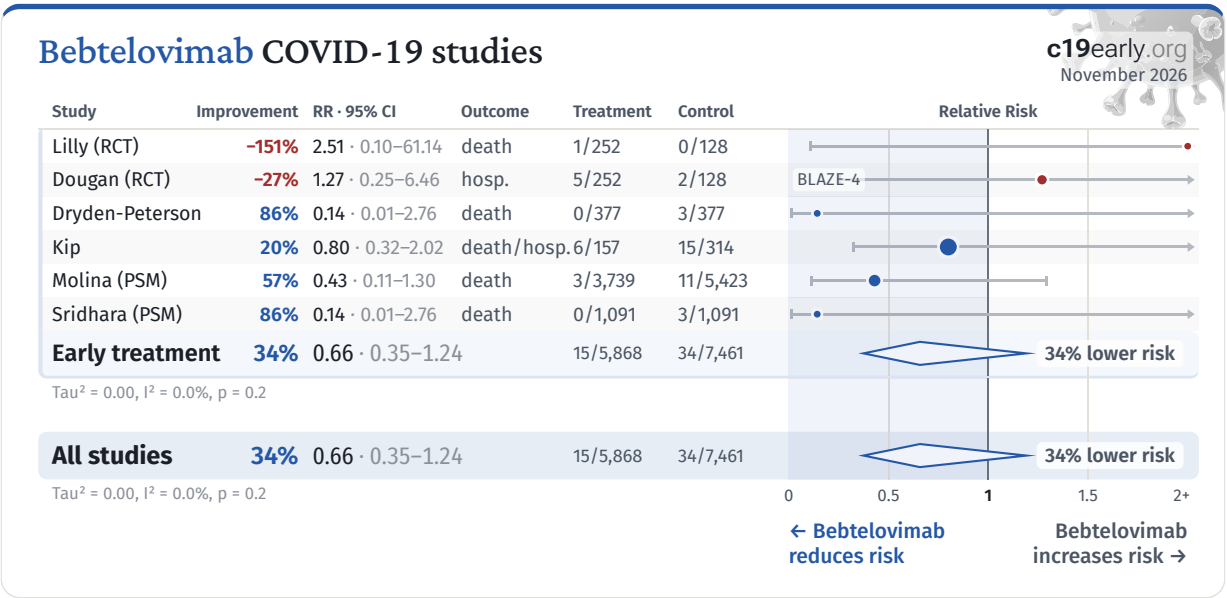

Fig. 4. **Random-effects meta-analysis for all studies.** This plot shows pooled effects, see the specific outcome analyses for individual outcomes. Analysis validating pooled outcomes for COVID-19 can be found below. Effect extraction is pre-specified, using the most serious outcome reported. For details see the appendix.

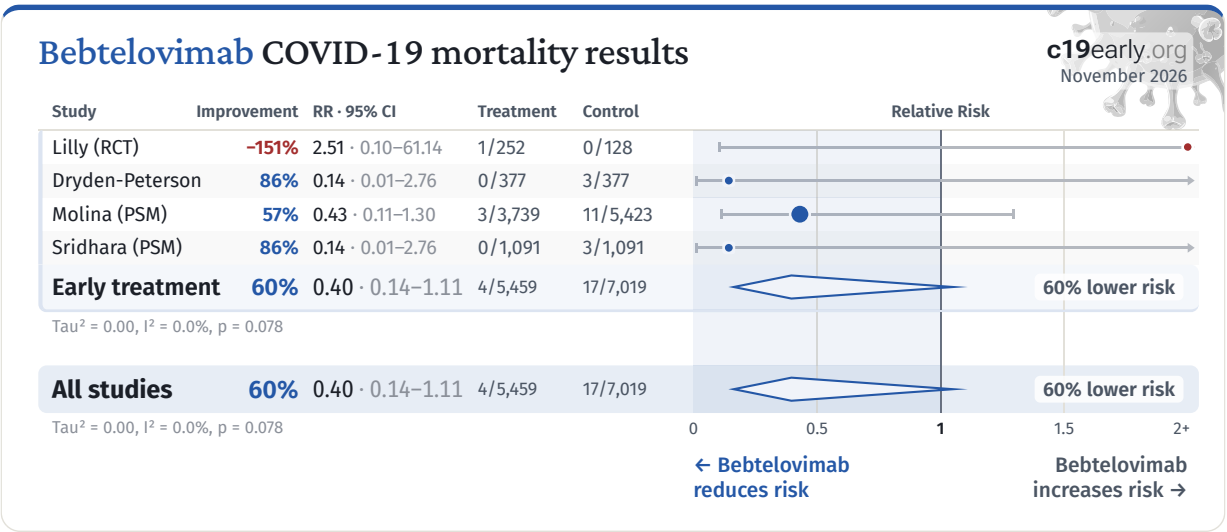

Fig. 5. Random-effects meta-analysis for mortality results.

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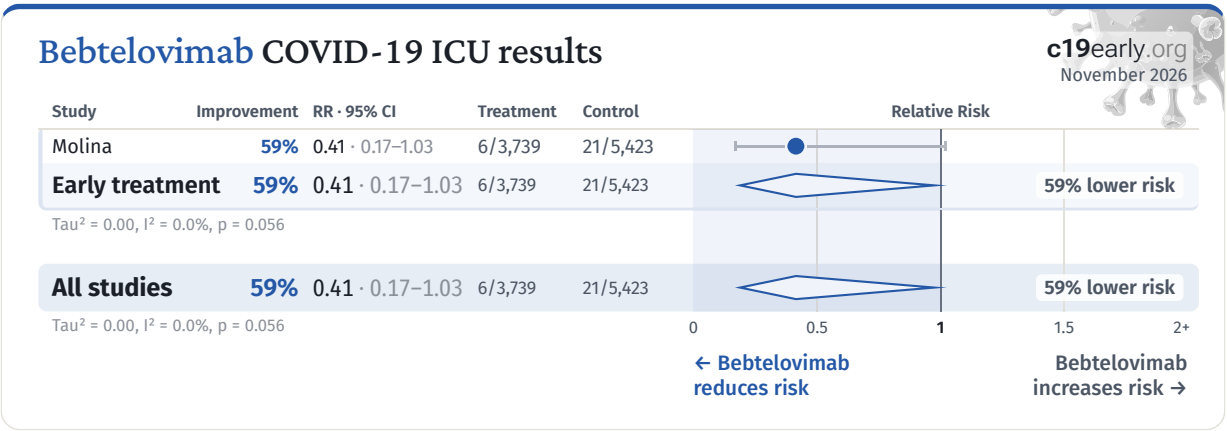

Fig. 6. Random-effects meta-analysis for ICU admission.

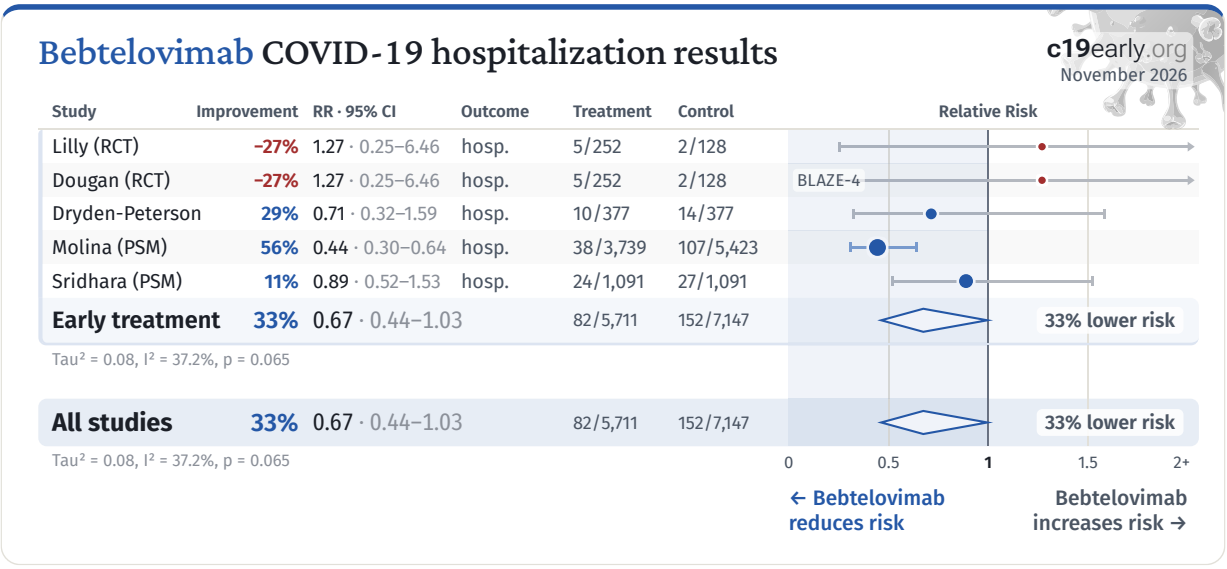

Fig. 7. Random-effects meta-analysis for hospitalization.

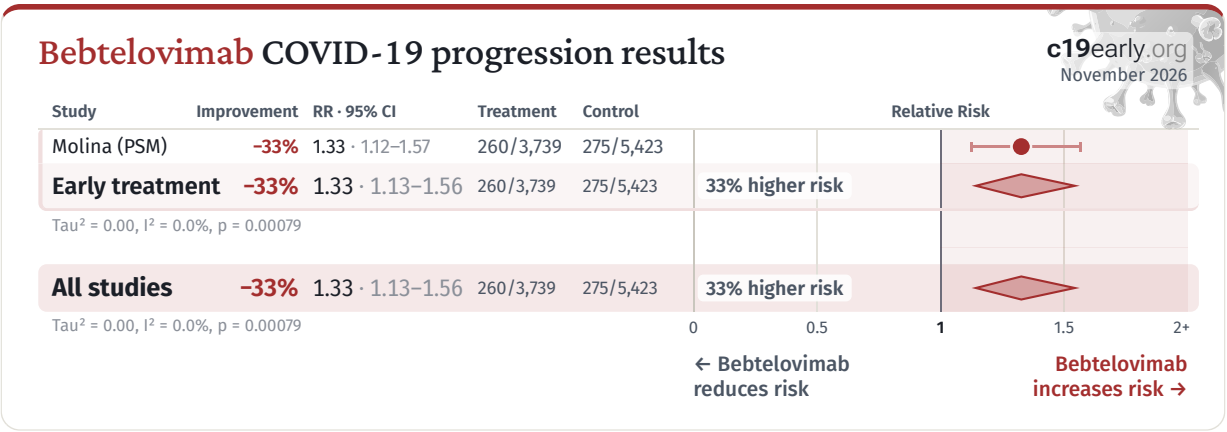

Fig. 8. Random-effects meta-analysis for progression.

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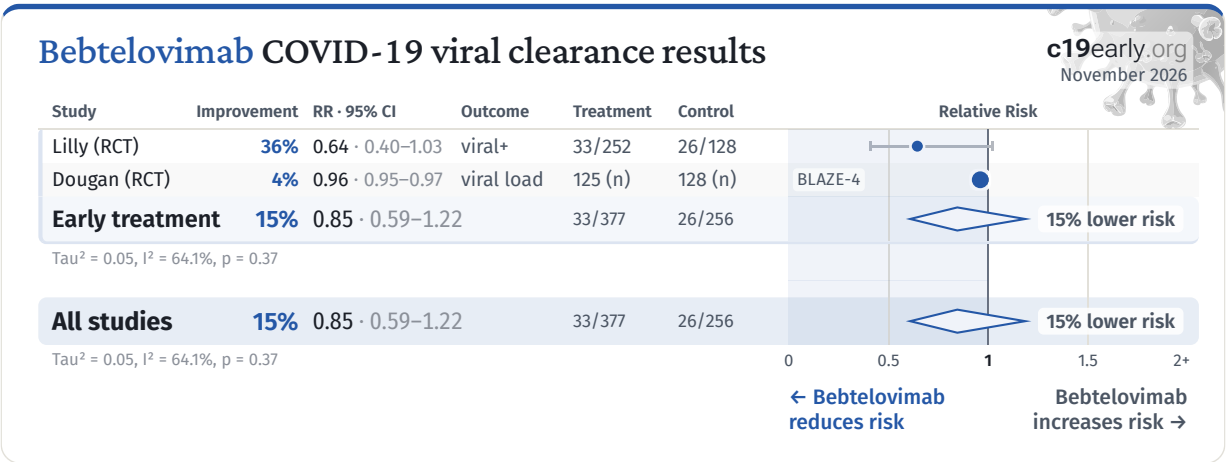

Fig. 9. Random-effects meta-analysis for viral clearance.

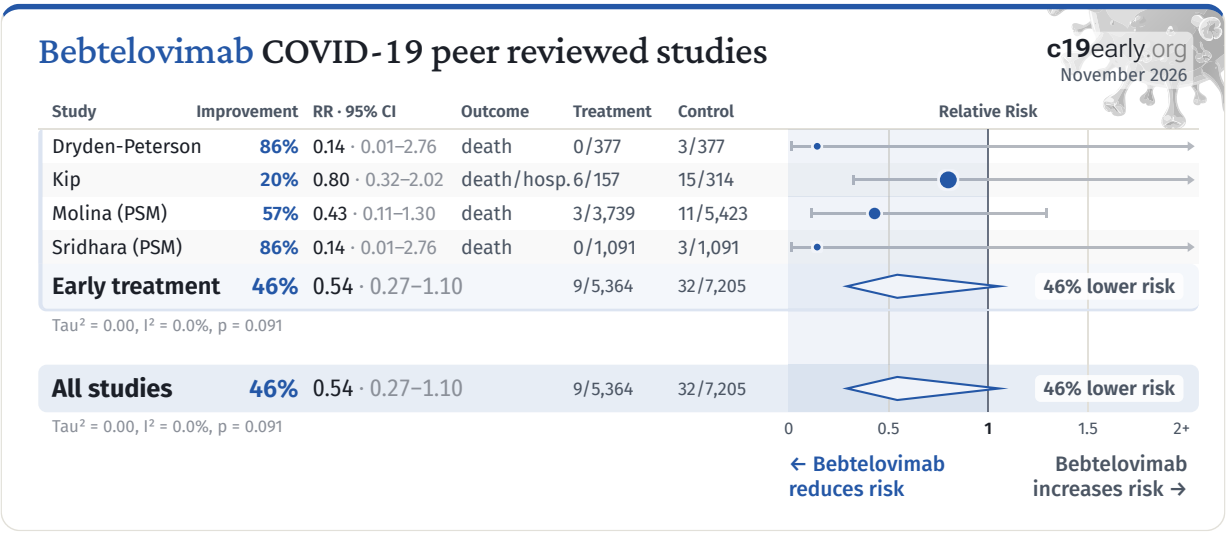

Fig. 10. **Random-effects meta-analysis for peer reviewed studies.** *Zeraatkar et al.* analyze 356 COVID-19 trials, finding no significant evidence that preprint results are inconsistent with peer-reviewed studies. They also show extremely long peer-review delays, with a median of 6 months to journal publication. A six month delay was equivalent to around 1.5 million deaths during the first two years of the pandemic. Authors recommend using preprint evidence, with appropriate checks for potential falsified data, which provides higher certainty much earlier. *Davidson et al.* also showed no important difference between meta-analysis results of preprints and peer-reviewed publications for COVID-19, based on 37 meta-analyses including 114 trials. Effect extraction is pre-specified, using the most serious outcome reported, see the appendix for details. Analysis validating pooled outcomes for COVID-19 can be found below.

Randomized Controlled Trials (RCTs)

Fig. 11 shows a comparison of results for RCTs and observational studies. Fig. 12 and 13 show forest plots for random-effects meta-analysis of all Randomized Controlled Trials and RCT mortality results. RCT results are included in Table 2.

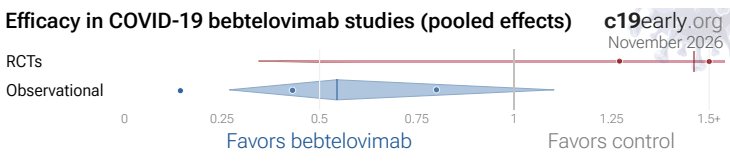

Fig. 11. Results for RCTs and observational studies.

RCTs have many potential biases

RCTs help to make study groups more similar and can provide a higher level of evidence, however they are subject to many biases[58], and analysis of double-blind RCTs has identified extreme levels of bias[59]. For COVID-19, the overhead may delay treatment, dramatically compromising efficacy; they may encourage monotherapy for simplicity at the cost of efficacy which may rely on combined or synergistic effects; the participants that sign up may not reflect real world usage or the population that benefits most in terms of age, comorbidities, severity of illness, or other factors; standard of care may be compromised and unable to evolve quickly based on emerging research for new diseases; errors may be made in randomization and medication delivery; and investigators may have hidden agendas or vested interests influencing design, operation, analysis, reporting, and the potential for fraud. All of these biases have been observed with COVID-19 RCTs. There is no guarantee that a specific RCT provides a higher level of evidence.

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Conflicts of interest for COVID-19 RCTs

RCTs are expensive and many RCTs are funded by pharmaceutical companies or other organizations with conflicts of interest, for example governments that previously denied treatment with the study drug. For COVID-19, this creates an incentive to show efficacy for patented commercial products, and an incentive to show a lack of efficacy for inexpensive treatments. The bias is expected to be significant, for example *Als-Nielsen et al.* analyzed 370 RCTs from Cochrane reviews, showing that trials funded by for-profit organizations were 5 times more likely to recommend the experimental drug compared with those funded by nonprofit organizations. *Bekelman et al.* and *Lundh et al.* show that industry-sponsored studies are more likely to be favorable. For COVID-19, some major philanthropic organizations are largely funded by investments with extreme conflicts of interest for and against specific COVID-19 interventions.

RCTs for novel acute diseases requiring rapid treatment

High quality RCTs for novel acute diseases are more challenging, with increased ethical issues due to the urgency of treatment, increased risk due to enrollment delays, and more difficult design with a rapidly evolving evidence base. For COVID-19, the most common site of initial infection is the upper respiratory tract. Immediate treatment is likely to be most successful and may prevent or slow progression to other parts of the body. For a non-prophylaxis RCT, it makes sense to provide treatment in advance and instruct patients to use it immediately on symptoms, just as some governments have done by providing medication kits in advance. Unfortunately, no RCTs have been done in this way. Every treatment RCT to date involves delayed treatment. Among the 227 treatments we have analyzed, 67% of RCTs involve very late treatment 5+ days after onset. No non-prophylaxis COVID-19 RCTs match the potential real-world use of early treatments. They may more accurately represent results for treatments that require visiting a medical facility, e.g., those requiring intravenous administration.

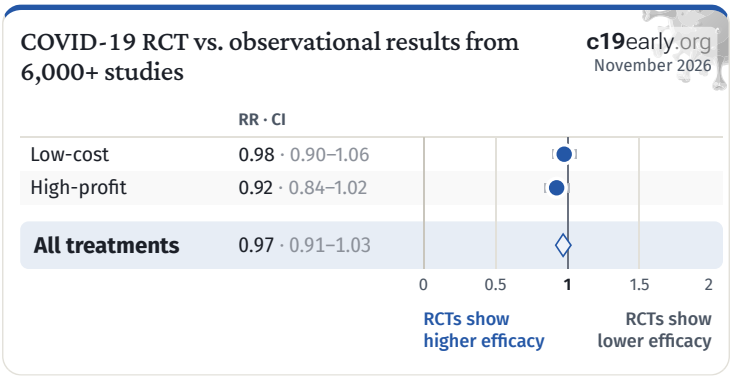

Fig. 14. For COVID-19, observational study results do not systematically differ from RCTs, RR 0.97 [0.91-1.03] across 227 treatments [63].

Observational studies have been shown to be reliable

Evidence shows that observational studies can also provide reliable results. *Concato et al.* found that well-designed observational studies do not systematically overestimate the magnitude of the effects of treatment compared to RCTs. *Anglemyer et al.* analyzed reviews comparing RCTs to observational studies and found little evidence for significant differences in effect estimates.

We performed a similar analysis across the 227 treatments we cover, showing no significant difference in the results of RCTs compared to observational studies, RR 0.97 [0.91-1.03] [66]. Similar results are found for all low-cost treatments, RR 0.98 [0.90-1.06]. High-cost treatments show a non-significant trend towards RCTs showing greater efficacy, RR 0.92 [0.84-1.02]. Details can be found in the supplementary data.

Lee et al. showed that only 14% of the guidelines of the Infectious Diseases Society of America were based on RCTs. Evaluation of studies relies on an understanding of the study and potential biases. Limitations in an RCT can outweigh the benefits, for example excessive dosages, excessive treatment delays, or remote survey bias may have a greater effect on results. Ethical issues may also prevent running RCTs for known effective treatments. For more on issues with RCTs see [68,69].

RCTs may be less reliable

Concato et al. report a paradoxical finding—RCT results had higher variability, and only RCTs were found to sometimes report significant results the opposite of the overall result. The same trend is seen for the most popular (most politicized) COVID-19 treatments—considering all statistically significant results reported in studies, RCTs are slightly more likely to report a result in the opposite direction. In other words, for these COVID-19 treatments and for the topics covered by *Concato et al.*, assuming causality from a single study is more likely to result in an incorrect conclusion for RCTs.

Increased risk of inconsistent results for RCTs suggests higher prevalence of bias, which may arise due to many issues including design bias, conflicts of interest, treatment differences by physicians aware of allocation, attrition bias, ascertainment bias, randomization failures, errors, or fraud.

Using all studies identifies efficacy 8+ months faster (9+ months for low-cost treatments)

Currently, 59 of the treatments we analyze show statistically significant efficacy or harm, defined as ≥10% decreased risk or >0% increased risk from ≥3 studies. Of these, 54% have been confirmed in RCTs, with a mean delay of 7.8 months (62% with 8.7 months delay for low-cost treatments). The remaining treatments either have no RCTs, or the point estimate is consistent.

All studies must be carefully analyzed

Neither observational studies nor RCTs prove causation—any study can be flawed or fraudulent. We need much more, for example a combination of results from many independent teams, detailed understanding of each study, knowledge of conflicts/team reliability, dose-response relationships, delay-response relationships, logical results across outcomes, or details consistent with preclinical expectations.

All studies must be evaluated individually. RCTs for a given medication and disease may be more reliable, however they may also be less reliable. For off-patent medications, very high conflict of interest trials may be more likely to be RCTs, and more likely to be large trials that dominate meta-analyses.

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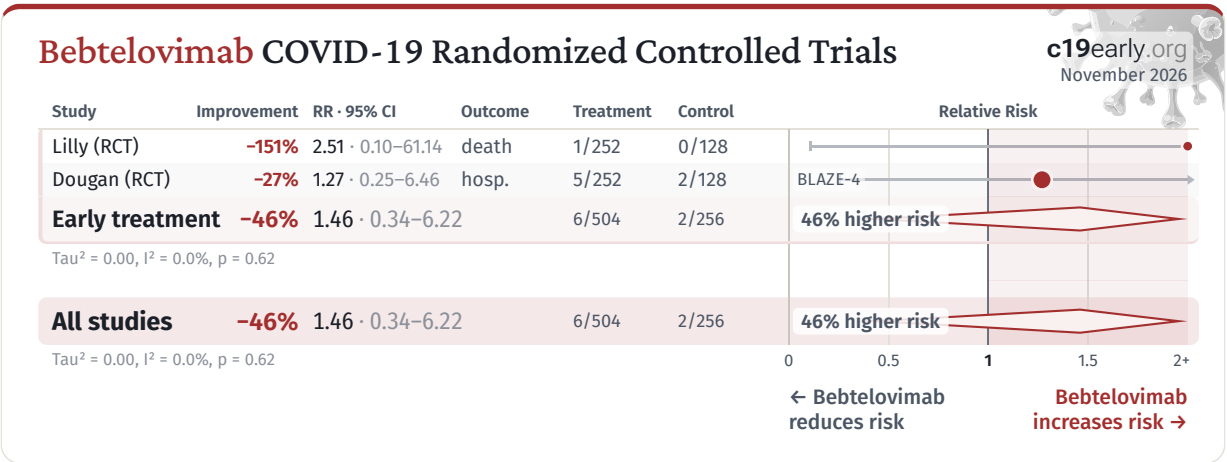

Fig. 12. **Random-effects meta-analysis for all Randomized Controlled Trials.** This plot shows pooled effects, see the specific outcome analyses for individual outcomes. Analysis validating pooled outcomes for COVID-19 can be found below. Effect extraction is pre-specified, using the most serious outcome reported. For details see the appendix.

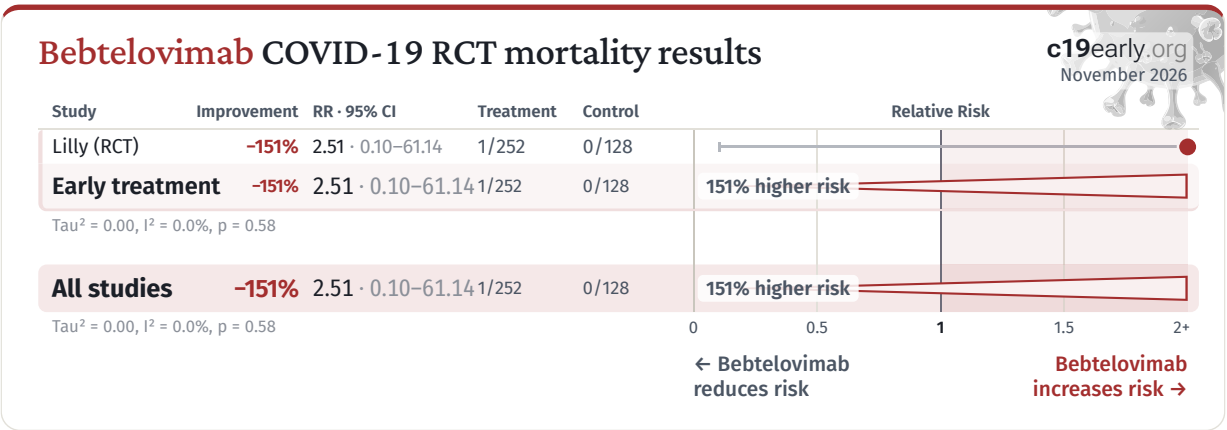

Fig. 13. Random-effects meta-analysis for RCT mortality results.

Media Bias

Low-cost treatments were subject to bias and censorship during the pandemic. Scientific bias is seen in the design, analysis, presentation, and selective reporting of studies, which favored negative results. A similar bias is seen in the media coverage for low-cost treatments. However, the opposite is seen for high-profit treatments.

We analyze media coverage for the 227 treatments we cover using Altmetric [70] and PlumX Metrics [71]. Altmetric covers ~14,000 news outlets [72] and PlumX covers ~55,000 news sources [73]. The two services are complementary with coverage of different news sources and different methods for identifying articles not containing a DOI. We consider studies to have received significant media coverage if they were covered by at least 100 articles in either Altmetric of PlumX [73,74]. The most widespread study was covered by 1,047 outlets. Studies covered by major syndicated sources shows hundreds of news articles, while studies covered by few outlets typically appear in niche medical news sources or press releases.

Fig. 15 and 16 analyze coverage for all low-cost treatments and all high-profit treatments, showing significant bias. For high-profit treatments media coverage favored positive results 3.8× more than negative results, whereas for low-cost treatments coverage favored negative results 5.4× more than positive results.

The media bias may result in widespread incorrect perceptions on the relative efficacy of high-profit and low-cost treatments. The impact is significant—increased cost limits the use of high-profit treatments and treatment equity. High-profit treatments were also more difficult to access, especially for earlier treatment which improves efficacy and minimizes community transmission.

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Media bias for high-profit COVID-19 treatments

c19early.org
November 2026

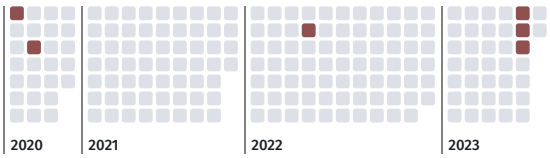
Media was 3.8× more likely to cover positive studies

19 positive studies were covered

11%

paxlovid **5**, remdesivir **3**, casirivimab/imdevimab **2**, convalescent plasma **2**, molnupiravir **2**, **one each:** antiandrogens, peginterferon lambda, sargramostim, tixagevimab/cilgavimab, tocilizumab

Only 6 negative studies were covered



3%

remdesivir **3**, **one each:** bamlanivimab/etesevimab, bebtelovimab, sotrovimab

3.8× **Positive bias for high-profit treatments**
positive studies were 3.8 times more likely to be covered

Data from Altmetric and PlumX: studies receiving significant mainstream media coverage from 6,000+ studies for 227 treatments, defined as ≥100 news outlets in either Altmetric or PlumX, which cover ~14,000 and ~55,000 outlets respectively. Media coverage was significantly higher for positive studies, p = 0.002 (Fisher's exact test). Studies during the pandemic (until May 5, 2023).

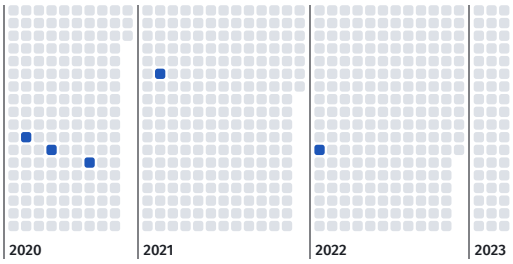
Fig. 15. For high-profit treatments, mainstream media coverage was biased towards positive results. When restricting to RCTs, the bias was even larger, with positive RCTs 12.0× more likely to be covered.

Media bias for low-cost COVID-19 treatments

c19early.org
November 2026

Media covered only 0.8% of studies showing significantly lower COVID-19 risk

Only 5 positive studies were covered



0.8%

one each: budesonide, cannabidiol, fluvoxamine, HCQ, vitamin D

34 negative studies were covered

4%

HCQ **12**, ivermectin **6**, lopinavir/ritonavir **4**, azithromycin **3**, vitamin D **3**, fluvoxamine **2**, **one each:** ibuprofen, metformin, vitamin C, zinc

5.4× **Negative bias for low-cost treatments**
negative studies were 5.4 times more likely to be covered

Data from Altmetric and PlumX: studies receiving significant mainstream media coverage from 6,000+ studies for 227 treatments, defined as ≥100 news outlets in either Altmetric or PlumX, which cover ~14,000 and ~55,000 outlets respectively. Media coverage was significantly higher for negative studies, p < 0.001 (Fisher's exact test). Studies during the pandemic (until May 5, 2023).

Fig. 16. For low-cost treatments, mainstream media coverage was biased against positive results. When restricting to RCTs, the bias was even larger, with negative RCTs 6.9× more likely to be covered.

A combination of factors may have led to the very low media coverage of low-cost treatments:

- Politicization led to a media environment where coverage was often framed to support a political narrative rather than to provide objective scientific information. As Scott Alexander said: *"if you say anything in favor of ivermectin you will be cast out of civilization and thrown into the circle of social hell reserved for Klan members and 1/6 insurrectionists. All the health officials in the world will shout 'horse dewormer!' at you and compare you to Josef Mengele."* There was strong social pressure to discredit low-cost treatments.
- Censorship of information conflicting with selected authorities. For example, individuals and organizations presenting conflicting science were often shadowbanned or completely banned on Twitter and YouTube if the science they were reporting conflicted with the opinions of specific authorities selected by those companies, including when the authorities had not even reviewed the scientific results in question.
- FDA requires *"no adequate, approved, and available alternatives"* in order to grant an EUA for novel high-profit interventions, creating an incentive for authorities to ignore or downplay existing low-cost treatments.
- Regulatory capture biases authorities towards high-profit interventions.

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- Authorities ignored most evidence for low-cost treatments, for example the NIH references only 2% of studies in delayed, rarely-updated, biased commentaries with no quantitive analysis.
- Media coverage of science is often not very accurate, e.g., misunderstanding confounding issues. For example the media widely considered the RECOVERY HCQ RCT to be conclusive on efficacy, but very late treatment of late-stage patients (mostly on oxygen already) with an excessive toxic dose (shown dangerous in a dose comparison RCT) provides no information on the recommended early/prophylactic treatment. With difficulting in understanding basic confounders like treatment delay and dose, the media may favor deferring to authorities. Many studies for low-cost treatments require greater expertise to analyze.
- Substantial funding from pharmaceutical advertising biases editorial decisions towards high-profit interventions.
- PR power - companies/teams with strong PR presence are favored in the media, which correlates with high-profit and high conflict of interest studies.
- The media was very negative in general, inflating risk, and elevating fear and anxiety. A negative bias may improve ratings and revenue, increasing motivation to continue watching coverage.

Heterogeneity

Heterogeneity in COVID-19 studies arises from many factors including:

Treatment delay

The time between infection or the onset of symptoms and treatment may critically affect how well a treatment works. For example an antiviral may be very effective when used early but may not be effective in late stage disease, and may even be harmful. Oseltamivir, for example, is generally only considered effective for influenza when used within 0-36 or 0-48 hours [75,76]. Baloxavir marboxil studies for influenza also show that treatment delay is critical — *Ikematsu et al.* report an 86% reduction in cases for post-exposure prophylaxis, *Hayden et al.* show a 33 hour reduction in the time to alleviation of symptoms for treatment within 24 hours and a reduction of 13 hours for treatment within 24-48 hours, and *Kumar et al.* report only 2.5 hours improvement for inpatient treatment.

Treatment delay	*Result*
Post-exposure prophylaxis	86% fewer cases [77]
<24 hours	-33 hours symptoms [78]
24-48 hours	-13 hours symptoms [78]
Inpatients	-2.5 hours to improvement [79]

Table 3. **Studies of baloxavir marboxil for influenza show that early treatment is more effective.**

Fig. 17 shows a mixed-effects meta-regression for efficacy as a function of treatment delay in COVID-19 studies from 227 treatments, showing that efficacy declines rapidly with treatment delay. Early treatment is critical for COVID-19.

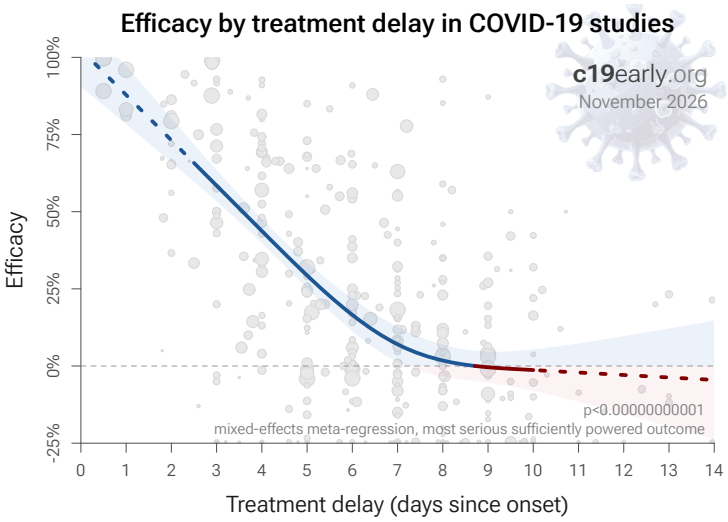

Fig. 17. Early treatment is more effective. Meta-regression showing efficacy as a function of treatment delay in COVID-19 studies from 227 treatments.

Patient demographics

Details of the patient population including age and comorbidities may critically affect how well a treatment works. For example, many COVID-19 studies with relatively young low-comorbidity patients show all patients recovering quickly with or without treatment. In such cases, there is little room for an effective treatment to improve results, for example as in *López-Medina et al.*

SARS-CoV-2 variants

Efficacy may depend critically on the distribution of SARS-CoV-2 variants encountered by patients. Risk varies significantly across variants [81], for example the Gamma variant shows significantly different characteristics [82-85]. Different mechanisms of action may be more or less effective depending on variants, for example the degree to which TMPRSS2 contributes to viral entry can differ across variants [86,87].

Treatment regimen

Effectiveness may depend strongly on the dosage and treatment regimen.

Medication quality

The quality of medications may vary significantly between manufacturers and production batches, which may significantly affect efficacy and safety. *Williams et al.* analyze ivermectin from 11 different sources, showing highly variable antiparasitic efficacy across different manufacturers. *Xu et al.* analyze a treatment from two different manufacturers, showing 9 different impurities, with significantly different concentrations for each manufacturer.

Other treatments

The use of other treatments may significantly affect outcomes, including supplements, other medications, or other interventions such as prone positioning. Treatments may be synergistic [90-114], therefore efficacy may depend strongly on combined treatments.

Effect measured

Across all studies there is a strong association between different outcomes, for example improved recovery is strongly associated with lower mortality. However, efficacy may differ depending on the effect measured, for example a treatment may be more effective against secondary complications and have minimal effect on viral clearance.

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Meta-analysis

The distribution of studies will alter the outcome of a meta-analysis. Consider a simplified example where everything is equal except for the treatment delay, and effectiveness decreases to zero or below with increasing delay. If there are many studies using very late treatment, the outcome may be negative, even though early treatment is very effective. All meta-analyses combine heterogeneous studies, varying in population, variants, and potentially all factors above, and therefore may obscure efficacy by including studies where treatment is less effective. Generally, we expect the estimated effect size from meta-analysis to be less than that for the optimal case. Looking at all studies is valuable for providing an overview of all research, important to avoid cherry-picking, and informative when a positive result is found despite combining less-optimal situations. However, the resulting estimate does not apply to specific cases such as early treatment in high-risk populations. While we present results for all studies, we also present treatment time and individual outcome analyses, which may be more informative for specific use cases.

Pooled Effects

Combining studies is required

For COVID-19, delay in clinical results translates into additional death and morbidity, as well as additional economic and societal damage. Combining the results of studies reporting different outcomes is required. There may be no mortality in a trial with low-risk patients, however a reduction in severity or improved viral clearance may translate into lower mortality in a high-risk population. Different studies may report lower severity, improved recovery, and lower mortality, and the significance may be very high when combining the results. "*The studies reported different outcomes*" is not a good reason for disregarding results. Pooling the results of studies reporting different outcomes allows us to use more of the available information. Logically we should, and do, use additional information when evaluating treatments—for example dose-response and treatment delay-response relationships provide additional evidence of efficacy that is considered when reviewing the evidence for a treatment.

Specific outcome and pooled analyses

We present both specific outcome and pooled analyses. In order to combine the results of studies reporting different outcomes we use the most serious outcome reported in each study, based on the thesis that improvement in the most serious outcome provides comparable measures of efficacy for a treatment. A critical advantage of this approach is simplicity and transparency. There are many other ways to combine evidence for different outcomes, along with additional evidence such as dose-response relationships, however these increase complexity.

Ethical and practical issues limit high-risk trials

Trials with high-risk patients may be restricted due to ethics for treatments that are known or expected to be effective, and they increase difficulty for recruiting. Using less severe outcomes as a proxy for more serious outcomes allows faster and safer collection of evidence.

Validating pooled outcome analysis for COVID-19

For many COVID-19 treatments, a reduction in mortality logically follows from a reduction in hospitalization, which follows from a reduction in symptomatic cases, which follows from a reduction in PCR positivity. We can directly test this for COVID-19.

Analysis of the the association between different outcomes across studies from all 227 treatments we cover confirms the validity of pooled outcome analysis for COVID-19. Fig. 18 shows that lower hospitalization is very strongly associated with lower mortality ($p < 0.0000000001$). Similarly, Fig. 19 shows that improved recovery is very strongly associated with lower mortality ($p < 0.0000000001$). Considering the extremes, *Singh et al.* show an association between viral clearance and hospitalization or death, with $p = 0.003$ after excluding one large outlier from a mutagenic treatment, and based on 44 RCTs including 52,384 patients. Fig. 20 shows that improved viral clearance is strongly associated with fewer serious outcomes. The association is very similar to *Singh et al.*, with higher confidence due to the larger number of studies. As with *Singh et al.*, the confidence increases when excluding the outlier treatment, from $p = 0.0000000074$ to $p = 0.00000000015$.

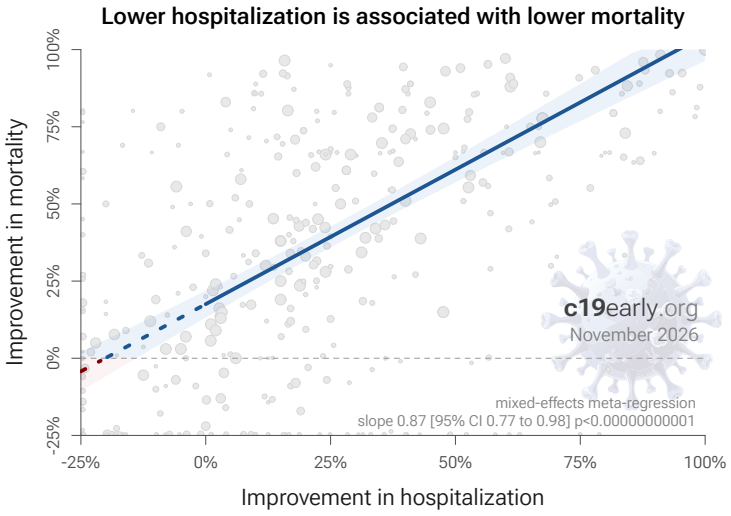

Fig. 18. Lower hospitalization is associated with lower mortality, supporting pooled outcome analysis.

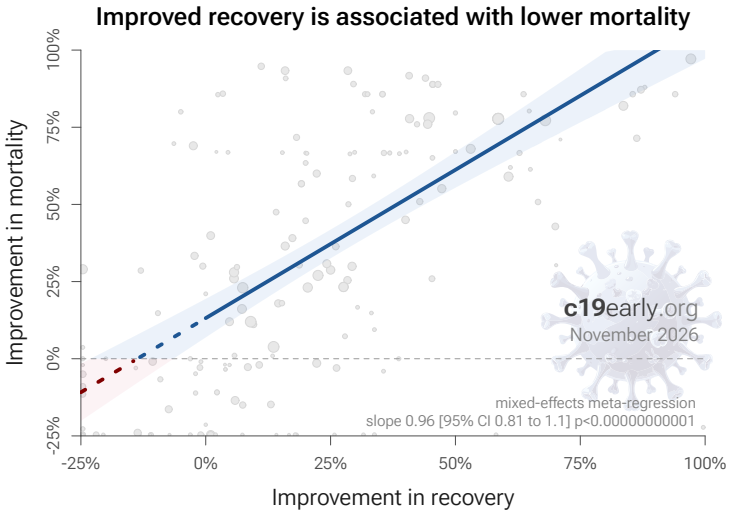

Fig. 19. Improved recovery is associated with lower mortality, supporting pooled outcome analysis.

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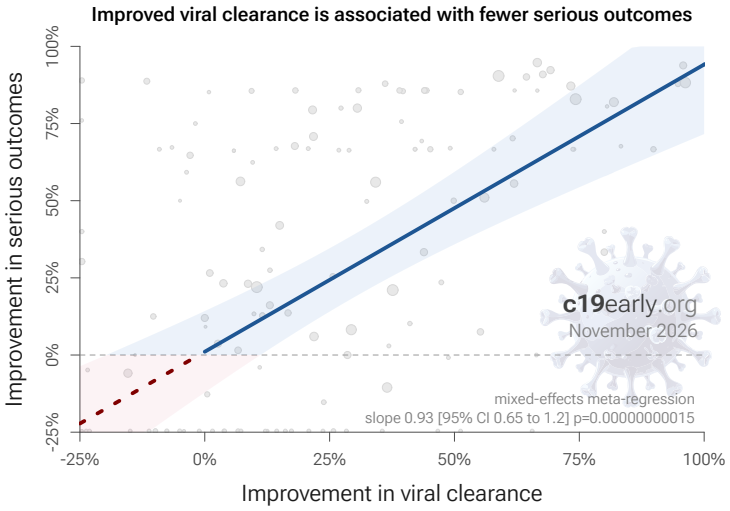

Fig. 18. Improved viral clearance is associated with fewer serious outcomes, supporting pooled outcome analysis.

Pooled outcomes identify efficacy 5 months faster (7 months for RCTs)

Currently, 59 of the treatments we analyze show statistically significant efficacy or harm, defined as ≥10% decreased risk or >0% increased risk from ≥3 studies. 85% of these have been confirmed with one or more specific outcomes, with a mean delay of 4.6 months. When restricting to RCTs only, 53% of treatments showing statistically significant efficacy/harm with pooled effects have been confirmed with one or more specific outcomes, with a mean delay of 7.5 months. Fig. 21 shows when treatments were found effective during the pandemic. Pooled outcomes often resulted in earlier detection of efficacy.

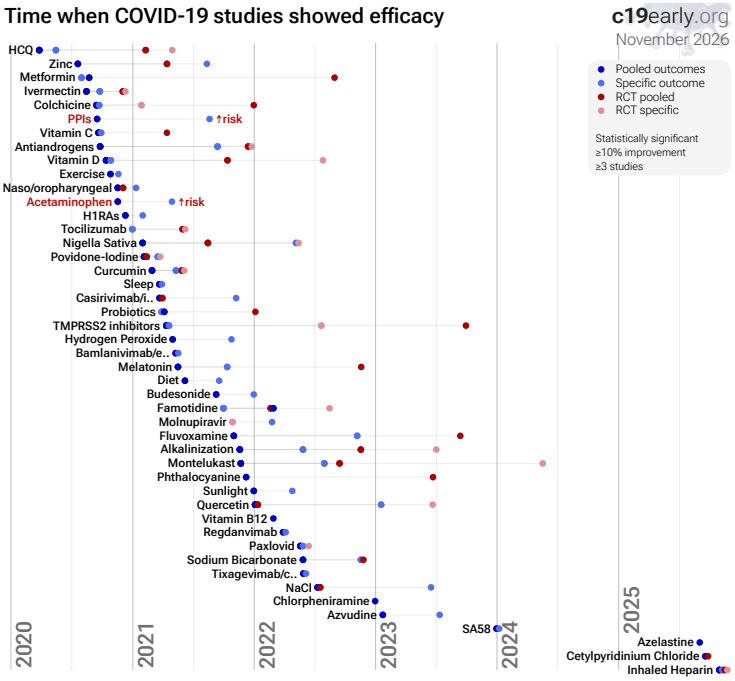

Fig. 21. **The time when studies showed that treatments were effective, defined as statistically significant improvement of ≥10% from ≥3 studies.** Pooled results typically show efficacy earlier than specific outcome results. Results from all studies often shows efficacy much earlier than when restricting to RCTs. Results reflect conditions as used in trials to date, these depend on the population treated, treatment delay, and treatment regimen.

Limitations

Pooled analysis could hide efficacy, for example a treatment that is beneficial for late stage patients but has no effect on viral clearance may show no efficacy if most studies only examine viral clearance. In practice, it is rare for a non-antiviral treatment to report viral clearance and to not report clinical outcomes; and in practice other sources of heterogeneity such as differences in treatment delay are more likely to hide efficacy.

Summary

Analysis validates the use of pooled effects and shows significantly faster detection of efficacy on average. However, as with all meta-analyses, it is important to review the different studies included. We also present individual outcome analyses, which may be more informative for specific use cases.

Discussion

Retrospective studies may overestimate efficacy

Wilcock et al. show that COVID-19 prescription treatments have been preferentially used by patients at lower risk. Retrospective studies may overestimate efficacy, and data for accurate adjustment may not be available. For example, patients with greater knowledge of effective treatments may be more likely to access prescription treatments but result in confounding because they are also more likely to use known beneficial non-prescription treatments.

Publication bias

Publishing is often biased towards positive results. Trials with patented drugs may have a financial conflict of interest that results in positive studies being more likely to be published, or bias towards more positive results. For example with molnupiravir, trials with negative results remain unpublished to date (CTRI/2021/05/033864 and CTRI/2021/08/0354242). For bebtelovimab, there is currently not enough data to evaluate publication bias with high confidence.

Funnel plot analysis

Funnel plots have traditionally been used for analyzing publication bias. This is invalid for COVID-19 acute treatment trials — the underlying assumptions are invalid, which we can demonstrate with a simple example. Consider a set of hypothetical perfect trials with no bias. Fig. 22 plot A shows a funnel plot for a simulation of 80 perfect trials, with random group sizes, and each patient's outcome randomly sampled (10% control event probability, and a 30% effect size for treatment). Analysis shows no asymmetry ($p > 0.05$). In plot B, we add a single typical variation in COVID-19 treatment trials — treatment delay. Consider that efficacy varies from 90% for treatment within 24 hours, reducing to 10% when treatment is delayed 3 days. In plot B, each trial's treatment delay is randomly selected. Analysis now shows highly significant asymmetry, $p < 0.0001$, with six variants of Egger's test all showing $p < 0.05$ [116-123]. Note that these tests fail even though treatment delay is uniformly distributed. In reality treatment delay is more complex — each trial has a different distribution of delays across patients, and the distribution across trials may be biased (e.g., late treatment trials may be more common). Similarly, many other variations in trials may produce asymmetry, including dose, administration, duration of treatment, differences in SOC, comorbidities, age, variants, and bias in design, implementation, analysis, and reporting.

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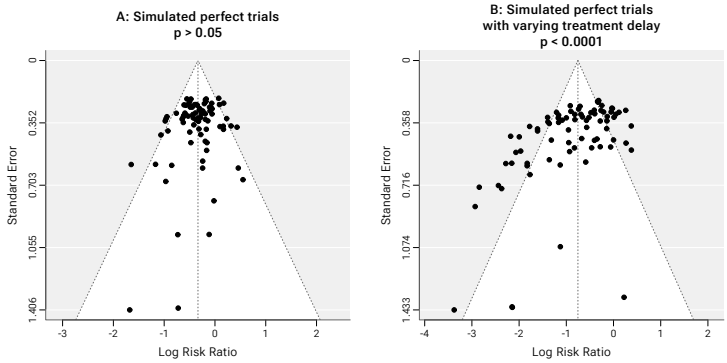

Fig. 22. Example funnel plot analysis for simulated perfect trials.

Limitations

Summary statistics from meta-analysis necessarily lose information. As with all meta-analyses, studies are heterogeneous, with differences in treatment delay, treatment regimen, patient demographics, variants, conflicts of interest, standard of care, and other factors. We provide analyses for specific outcomes and by treatment delay, and we aim to identify key characteristics in the forest plots and summaries. Results should be viewed in the context of study characteristics.

Some analyses classify treatment based on early or late administration, as done here, while others distinguish between mild, moderate, and severe cases. Viral load does not indicate degree of symptoms — for example patients may have a high viral load while being asymptomatic. With regard to treatments that have antiviral properties, timing of treatment is critical — late administration may be less helpful regardless of severity.

Details of treatment delay per patient is often not available. For example, a study may treat 90% of patients relatively early, but the events driving the outcome may come from 10% of patients treated very late. Our 5 day cutoff for early treatment may be too conservative, 5 days may be too late in many cases.

Comparison across treatments is confounded by differences in the studies performed, for example dose, variants, and conflicts of interest. Trials with conflicts of interest may use designs better suited to the preferred outcome.

In some cases, the most serious outcome has very few events, resulting in lower confidence results being used in pooled analysis, however the method is simpler and more transparent. This is less critical as the number of studies increases. Restriction to outcomes with sufficient power may be beneficial in pooled analysis and improve accuracy when there are few studies, however we maintain our pre-specified method to avoid any retrospective changes.

Studies show that combinations of treatments can be highly synergistic and may result in many times greater efficacy than individual treatments alone [90-114]. Therefore standard of care may be critical and benefits may diminish or disappear if standard of care does not include certain treatments.

This real-time analysis is constantly updated based on submissions. Accuracy benefits from widespread review and submission of updates and corrections from reviewers. Less popular treatments may receive fewer reviews.

No treatment or intervention is 100% available and effective for all current and future variants. Efficacy may vary significantly with different variants and within different populations. All treatments have potential side effects. Propensity to experience side effects may be predicted in advance by qualified physicians. We do not provide medical advice. Before taking any medication, consult a qualified physician who can compare all options, provide personalized advice, and provide details of risks and benefits based on individual medical history and situations.

Reviews

Multiple reviews cover bebtelovimab for COVID-19, presenting additional background on mechanisms and related results, including [4,11,54,124].

Other studies

Additional preclinical or review papers suggesting potential benefits of bebtelovimab for COVID-19 include [131-139]. We have not reviewed these studies in detail.

Perspective

Results compared with other treatments

SARS-CoV-2 infection and replication involves a complex interplay of 500+ host and viral proteins and other factors [44-51], providing many therapeutic targets. Over 12,000 compounds have been predicted to reduce COVID-19 risk [52], either by directly minimizing infection or replication, by supporting immune system function, or by minimizing secondary complications. Fig. 23 shows an overview of the results for bebtelovimab in the context of multiple COVID-19 treatments, and Fig. 24 shows a plot of efficacy vs. cost for COVID-19 treatments.

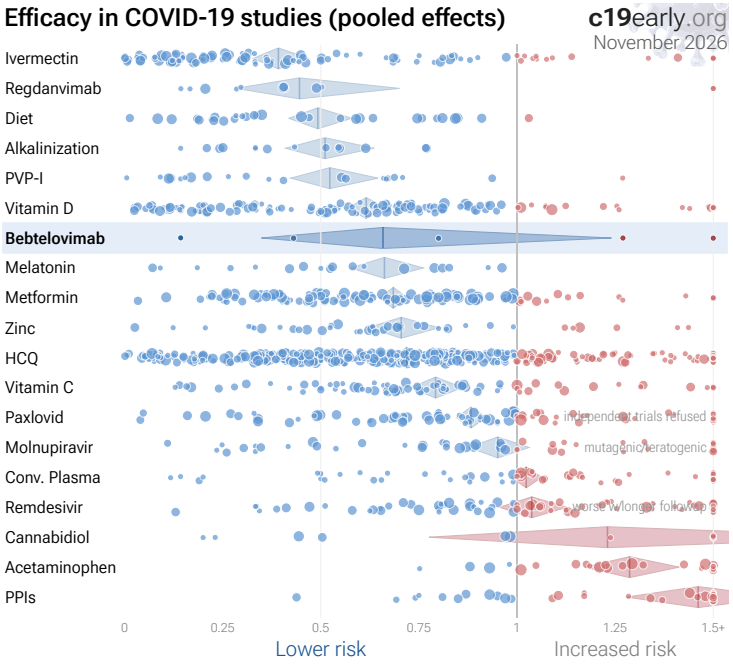

Fig. 23. **Scatter plot showing results within the context of multiple COVID-19 treatments.** Diamonds shows the results of random-effects meta-analysis. 0.5% of 12,000+ proposed treatments show efficacy [140].

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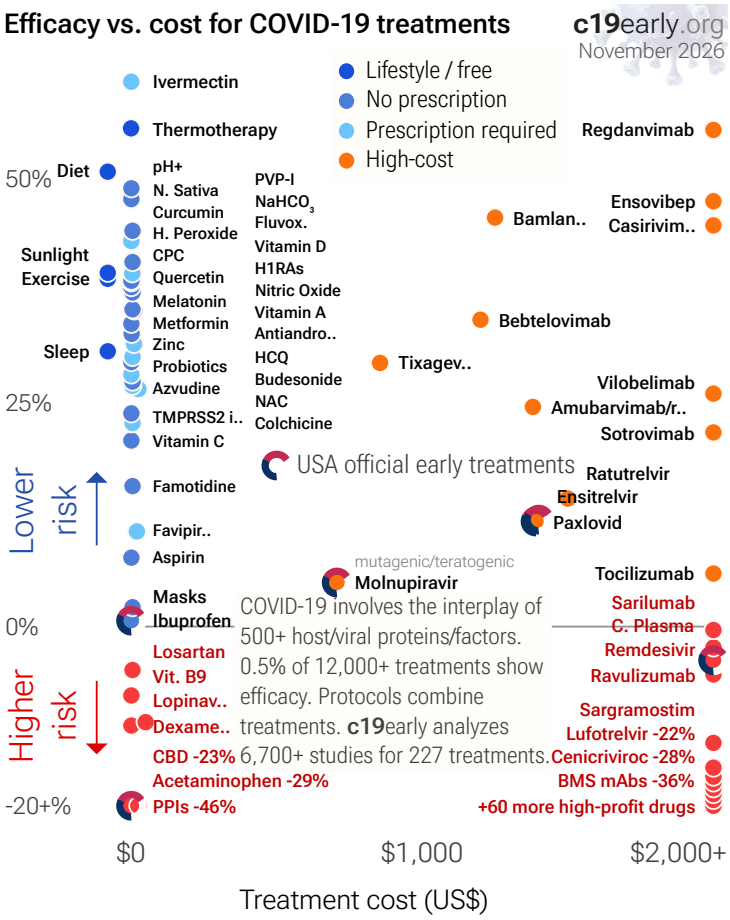

Fig. 24. Efficacy vs. cost for COVID-19 treatments.

Conclusion

Meta-analysis using the most serious outcome reported shows 34% [-24-65%] lower risk, without reaching statistical significance. Results are similar for peer-reviewed studies and worse for Randomized Controlled Trials. 2 studies from 2 independent teams (both from the same country) show significant benefit.

Currently there is limited data, with only 34 control events for the most serious outcome in trials to date.

Efficacy is variant dependent. *In Vitro* research suggests a lack of efficacy for omicron BQ.1.1 [1], BA.5, BA.2.75, XBB, XBB.1.5, XMM.1.9.1 [2,3]. mAb use may create new variants that spread globally [4-6], and may be associated with increased risk of autoimmune disease [7], prolonged viral loads, clinical deterioration, and immune escape [5,8-12].

Prescription treatments have been preferentially used by patients at lower risk [13]. Retrospective studies may overestimate efficacy, for example patients with greater knowledge of effective treatments may be more likely to access prescription treatments but result in confounding because they are also more likely to use known beneficial non-prescription treatments.

Contact. Contact us on X at @CovidAnalysis.

Funding. We have received no funding or compensation in any form, and do not accept donations. This is entirely volunteer work.

Conflicts of interest. We have no conflicts of interest. We have no affiliation with any pharmaceutical companies, supplement companies, governments, political parties, or advocacy organizations.

Disclaimer. We do not provide medical advice. No treatment is 100% effective, and all may have side effects. Protocols combine multiple treatments. Consult a qualified physician for personalized risk/benefit analysis.

AI. We use AI models (Gemini, Grok, Claude, and ChatGPT) tasked with functioning as additional peer-reviewers to check for errors, suggest improvements, and review spelling and grammar. Any corrections are manually verified. Our preference for em dashes is independent of AI.

Updates. Our COVID-19 meta-analyses involve the extraction of over 229,000 datapoints from thousands of papers for 227 treatments. We thank the thousands of scientists, physicians, and other contributors that have provided updates, suggestions, feedback, and corrections. These are all welcome and can be submitted at https://c19early.org/btmeta.html.

Dedication. This work is dedicated to top evidence-based physicians that worked tirelessly to analyze evidence and greatly reduce mortality and morbidity during the pandemic. In alphabetical order: Dr. Thomas J. Borody, Dr. Mary Talley Bowden, Dr. Flavio Cadegiani, Dr. Shankara Chetty, Dr. Ryan Cole, Dr. George Fareed, Dr. Sabine Hazan, Dr. Pierre Kory, Dr. Tess Lawrie, Dr. Robert Malone, Dr. Paul Marik, Dr. Peter McCullough, Dr. Didier Raoult, Dr. Harvey Risch, Dr. Jackie Stone, Dr. Brian Tyson, Dr. Joseph Varon, and Dr. Vladimir Zelenko.

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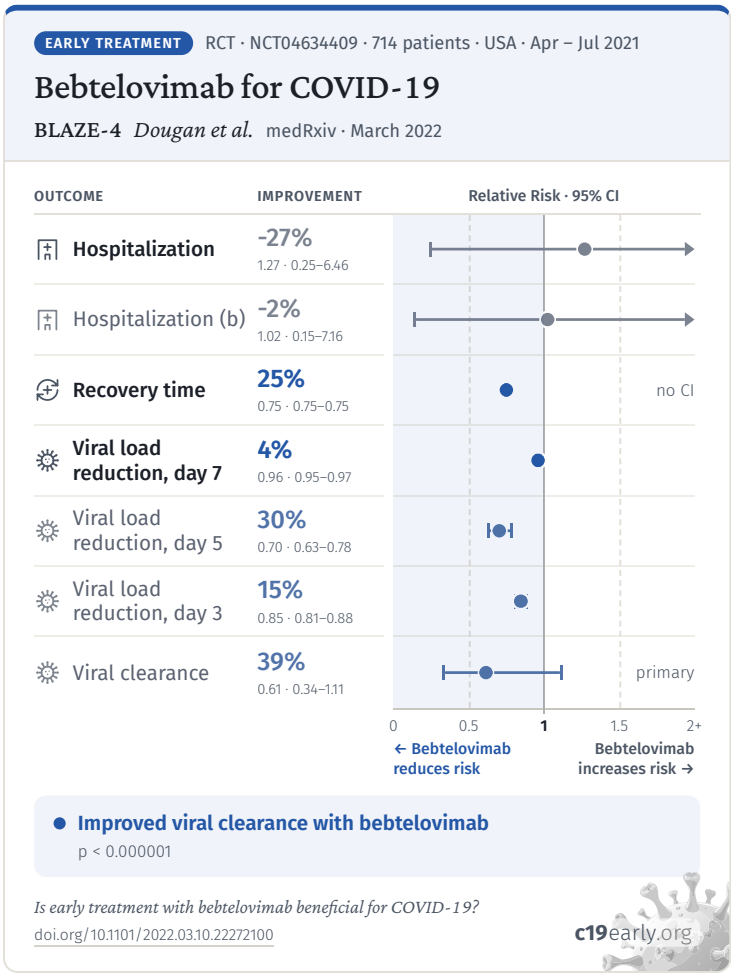
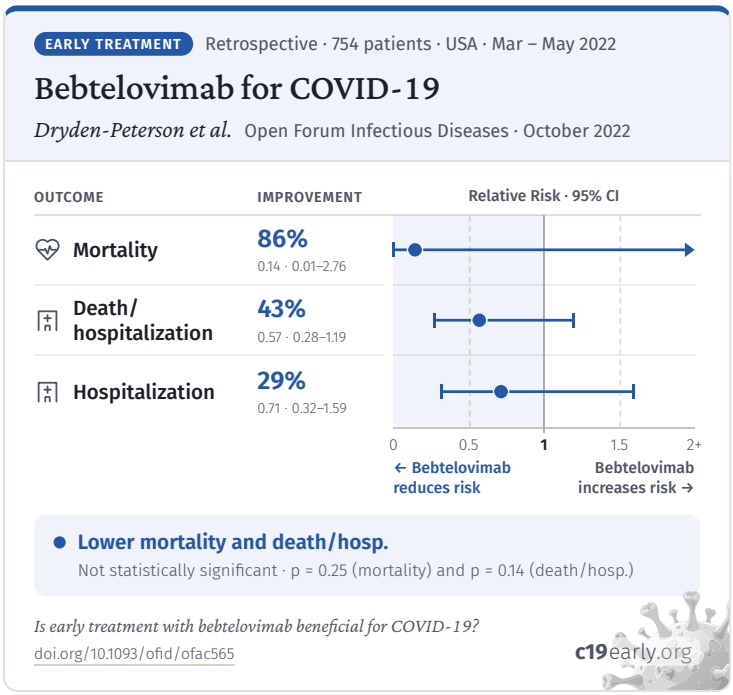
Study Notes

Dougan et al.



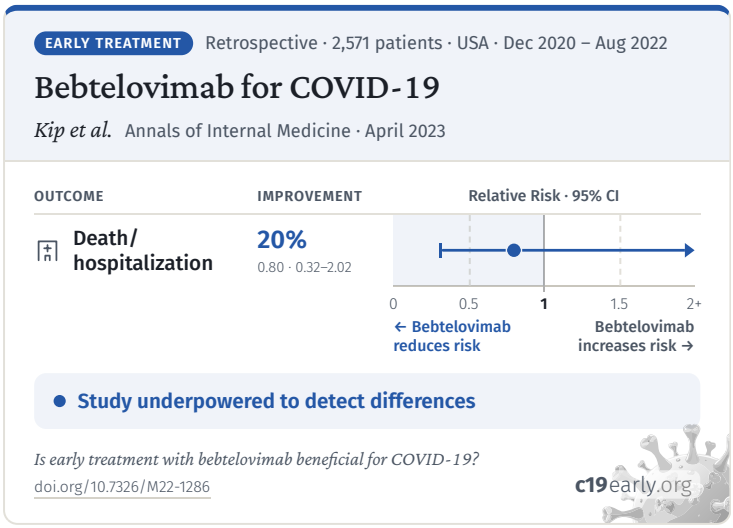
RCT showing improved viral clearance with bebtelovimab. Results refer to the placebo controlled portion of the trial.

Dryden-Peterson et al.



Retrospective 377 outpatients in the USA and matched controls, showing lower hospitalization/mortality with bebtelovimab treatment, without statistical significance. Notably, none of the patients that died in the control group were hospitalized within 14 days (later hospitalization is not reported). There may be a difference in the populations in terms of propensity to receive SOC.

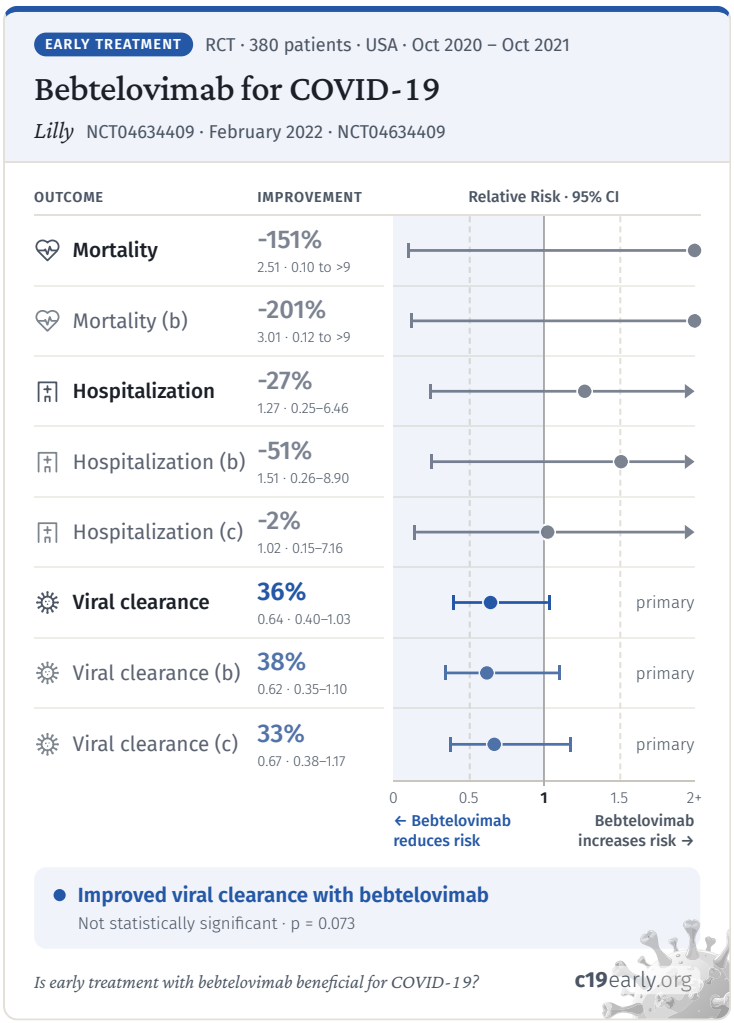
Kip et al.



Retrospective 2,571 patients treated with mAbs in the USA, and 5,135 control patients, showing lower combined mortality/hospitalization for bamlanivimab, bamlanivimab/etesevimab, casirivimab/imdevimab, sotrovimab, and bebtelovimab, with statistical significance only for casirivimab/imdevimab.

Confounding by treatment propensity. This study analyzes a population where only a fraction of eligible patients received the treatment. Patients receiving treatment may be more likely to follow other recommendations, more likely to receive additional care, and more likely to use additional treatments that are not tracked in the data (e.g., nasal/oral hygiene [141,142], vitamin D [143], etc.) — either because the physician recommending bebtelovimab also recommended them, or because the patient seeking out bebtelovimab is more likely to be familiar with the efficacy of additional treatments and more likely to take the time to use them. Therefore, these kind of studies may overestimate efficacy.

Lilly



RCT with 127 bamlanivimab, etesevimab, and bebtelovimab patients, 125 bebtelovimab patients, and 128 control patients, showing no significant differences in hospitalization and mortality. Viral clearance was improved although not statistically significant. NCT04634409.

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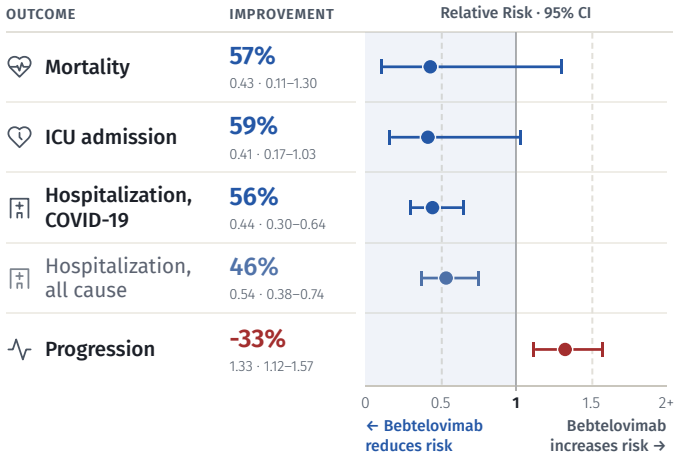
Molina et al.

EARLY TREATMENT Retrospective · 9,162 patients · USA · Apr – Oct 2022

Bebtelovimab for COVID-19

Molina et al. Int. J. Infectious Diseases · April 2023



● **Lower hospitalization and higher progression**
p < 0.0001 (hospitalization) and p = 0.001 (progression)

Is early treatment with bebtelovimab beneficial for COVID-19?
doi.org/10.1016/j.ijid.2023.04.396

c19early.org

Retrospective 3,739 patients treated with bebteloviman in the USA and matched controls, showing lower mortality and hospitalization with treatment, but higher emergency department visits.

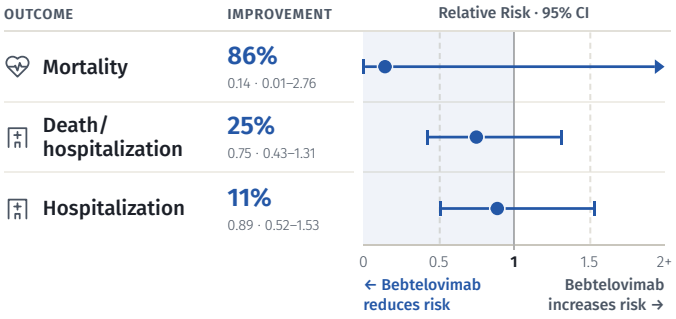
Confounding by treatment propensity. This study analyzes a population where only a fraction of eligible patients received the treatment. Patients receiving treatment may be more likely to follow other recommendations, more likely to receive additional care, and more likely to use additional treatments that are not tracked in the data (e.g., nasal/oral hygiene [141,142], vitamin D [143], etc.) — either because the physician recommending bebtelovimab also recommended them, or because the patient seeking out bebtelovimab is more likely to be familiar with the efficacy of additional treatments and more likely to take the time to use them. Therefore, these kind of studies may overestimate efficacy.

Sridhara et al.

EARLY TREATMENT PSM retrospective · 2,182 patients · USA · Apr – Aug 2022

Bebtelovimab for COVID-19

Sridhara et al.
PLOS ONE · April 2023



● **Lower mortality and death/hosp.**
Not statistically significant · p = 0.25 (mortality) and p = 0.31 (death/hosp.)

Is early treatment with bebtelovimab beneficial for COVID-19?
doi.org/10.1371/journal.pone.0279326

c19early.org

PSM retrospective 19,778 high-risk outpatients in the USA, showing no significant difference in outcomes with bebtelovimab treatment.

Confounding by treatment propensity. This study analyzes a population where only a fraction of eligible patients received the treatment. Patients receiving treatment may be more likely to follow other recommendations, more likely to receive additional care, and more likely to use additional treatments that are not tracked in the data (e.g., nasal/oral hygiene [141,142], vitamin D [143], etc.) — either because the physician recommending bebtelovimab also recommended them, or because the patient seeking out bebtelovimab is more likely to be familiar with the efficacy of additional treatments and more likely to take the time to use them. Therefore, these kind of studies may overestimate efficacy.

Appendix 1. Methods and Data

Search methods

We perform ongoing searches of PubMed, medRxiv, Europe PMC, ClinicalTrials.gov, The Cochrane Library, Google Scholar, Research Square, ScienceDirect, Oxford University Press, the reference lists of other studies and meta-analyses, and submissions to the site c19early.org, which regularly receives notification of studies upon publication. Search terms are bebtelovimab and COVID-19 or SARS-CoV-2. Automated searches are performed twice daily, with all matches reviewed for inclusion. All studies regarding the use of bebtelovimab for COVID-19 that report a comparison with a control group are included in the main analysis. Studies with major unexplained data issues, for example major outcome data that is impossible to be correct with no response from the authors, are excluded.

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Effect extraction

We extracted effect sizes and associated data from all studies. If studies report multiple kinds of effects then the most serious outcome is used in pooled analysis, while other outcomes are included in the outcome-specific analyses. For example, if effects for mortality and cases are reported then they are both used in specific outcome analyses, while mortality is used for pooled analysis. If symptomatic results are reported at multiple times, we use the latest time, for example if mortality results are provided at 14 days and 28 days, the results at 28 days have preference. Mortality alone is preferred over combined outcomes. Outcomes with zero events in both arms are not used, the next most serious outcome with one or more events is used. For example, in low-risk populations with no mortality, a reduction in mortality with treatment is not possible, however a reduction in hospitalization, for example, is still valuable. Clinical outcomes are considered more important than viral outcomes. When basically all patients recover in both treatment and control groups, preference for viral clearance and recovery is given to results mid-recovery where available. After most or all patients have recovered there is little or no room for an effective treatment to do better, however faster recovery is valuable. An IPD meta-analysis confirms that intermediate viral load reduction is more closely associated with hospitalization/death than later viral load reduction [144]. If only individual symptom data is available, the most serious symptom has priority, for example difficulty breathing or low SpO_2 is more important than cough.

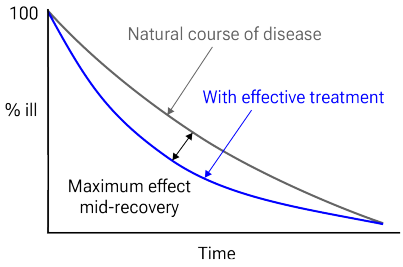

Fig. 25. Mid-recovery results can more accurately reflect efficacy when almost all patients recover. *Mateja et al.* confirm that intermediate viral load results more accurately reflect hospitalization/death.

Statistical methods

Forest plots are computed using PythonMeta [145] with the DerSimonian and Laird random-effects model (the fixed effect assumption is not plausible in this case) and inverse variance weighting. Results are presented with 95% confidence intervals. Heterogeneity among studies was assessed using the I^2 statistic. When results provide an odds ratio, we compute the relative risk when possible, or convert to a relative risk according to *Zhang et al.* Reported confidence intervals and *p*-values are used when available, and adjusted values are used when provided. If multiple types of adjustments are reported propensity score matching and multivariable regression has preference over propensity score matching or weighting, which has preference over multivariable regression. Adjusted results have preference over unadjusted results for a more serious outcome when the adjustments significantly alter results. When needed, conversion between reported *p*-values and confidence intervals followed *Altman, Altman (B)*, and Fisher's exact test was used to calculate *p*-values for event data. If continuity correction for zero values is required, we use the reciprocal of the opposite arm with the sum of the correction factors equal to 1 [149]. Results are expressed with RR < 1.0 favoring treatment, and using the risk of a negative outcome when applicable (for example, the risk of death rather than the risk of survival). If studies only report relative continuous values such as relative times, the ratio of the time for the treatment group versus the time for the control group is used. Calculations are done in Python (3.15.2) with scipy (1.18.1), pythonmeta (1.26), numpy (2.5.4), statsmodels (0.15.0), and plotly (6.9.0). Mixed-effects meta-regression results are computed with R (4.4.0) using the metafor (4.6-0) and rms (6.8-0) packages, and using the most serious sufficiently powered outcome. For all statistical tests, a *p*-value less than 0.05 was considered statistically significant. Grobid 0.8.2 is used to parse PDF documents.

When evaluating potential effect modification across groups, we use an interaction test as described by *Altman (C) et al.* We compared the log-transformed relative risks using a *z*-test, deriving the standard error of the difference from the 95% confidence intervals. A two-sided interaction *p*-value of < 0.05 was considered a statistically significant difference in treatment effect between the groups.

Quality evaluation

Cochrane RoB 2/ROBINS-I are often used to evaluate studies, and have the advantage of providing standardized rules that can be applied with minimal understanding of the domain and study. However, the rules do not account for many real-world issues, often overemphasize or underemphasize others, and studies show low inter-rater reliability [157]. Certain domains are more applicable for these tools, however the time-sensitive nature of a pandemic, with significant mortality for every day of delay in evidence assessment, and the characteristics of COVID-19 make them inappropriate for this domain. This can be demonstrated with examples where expert RoB 2/ROBINS-I ratings do not match reality for COVID-19. *Popp et al.* use RoB 2 to classify *Reis et al.* as low risk of bias, however this is the opposite of reality—the trial not only has very high risk of bias, but has very high actual known bias, refusing to release data despite pledging to, reporting multiple impossible numbers, having blinding and randomization failure, and many other issues [159]. *Axfors et al.* use RoB 2 to classify *Horby et al.* as low risk of bias, however this is the opposite of reality—the very late treatment and excessive dosage used produces results with no relevance to recommended usage. HCQ shows poor results with late treatment and excessive dosage, and the combination shows harm [B]. *Hempenius et al.* use ROBINS-I to classify 33 studies for HCQ. The two rated as having the lowest risk of bias [155,156] are far from the most informative. Both involve very late treatment, providing no information on recommended usage, and ROBINS-I does a very poor job of accounting for the impact of confounding factors [C].

Our quality evaluation focuses on known issues and bias, and the potential impact on outcomes, rather than just the risk of bias. The estimated potential impact of each confounding factor, and the direction of the impact is considered. For example, consider a study that shows significantly lower risk, the value of the study varies significantly if confounding points to an underestimate or an overestimate of efficacy. In one case, the real effect may be null, while the other case provides stronger evidence of efficacy (which may be greater than the study shows). Analysis focusing on the risk of bias, while simpler, may penalize studies for theoretical or technical issues that have no or minimal impact on outcomes. Analysis also depends on the outcome, for example certain issues are less relevant for objective outcomes such as mortality. Inaccurate penalization, and inaccurate high-quality evaluation in the face of known major issues affecting outcomes, increases in significance during a pandemic when immediate recognition of new evidence is critical, and when considering all global studies, as required during a pandemic. Investigators in other countries may have different customs for design, analysis, and reporting, and different English language skills, however they may not be less diligent or have greater bias. Investigators in lower-pharmaceutical-profit countries may have lower bias towards profitable interventions.

Treatment time

We have classified studies as early treatment if most patients are not already at a severe stage at the time of treatment (for example based on oxygen status or lung involvement), and treatment started within 5 days of the onset of symptoms. If studies contain a mix of early treatment and late treatment patients, we consider the treatment time of patients contributing most to the events (for example, consider a study where most patients are treated early but late treatment patients are included, and all mortality events were observed with late treatment patients). We note that a shorter time may be preferable. Antivirals are typically only considered effective when used within a shorter timeframe, for example 0-36 or 0-48 hours for oseltamivir, with longer delays not being effective [75,76].

Living analysis

This is a living analysis and is updated regularly. We received no funding, this research is done in our spare time. We have no affiliation with any pharmaceu-

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tical companies, supplement companies, governments, political parties, or advocacy organizations.

A summary of study results is below. Please submit updates and corrections at https://c19early.org/btmeta.html.

Early treatment

Effect extraction follows pre-specified rules as detailed above and gives priority to more serious outcomes. For pooled analyses, the first (most serious) outcome is used, which may differ from the effect a paper focuses on. Other outcomes are used in outcome specific analyses.

Dougan, 3/12/2022, Randomized Controlled Trial, USA, preprint, 22 authors, study period 19 April, 2021 - 19 July, 2021, average treatment delay 3.0 days, trial NCT04634409 (history) (BLAZE-4).	risk of hospitalization, 27.0% higher, RR 1.27, p = 1.00, treatment 5 of 252 (2.0%), control 2 of 128 (1.6%), combined bebtelovimab arms.
	risk of hospitalization, 2.4% higher, RR 1.02, p = 1.00, treatment 2 of 125 (1.6%), control 2 of 128 (1.6%), bebtelovimab only.
	relative viral load reduction, 4.0% better, RR 0.96, p < 0.001, treatment mean 3.77 (±0.21) n=125, control mean 3.62 (±0.2) n=128, day 7.
	relative viral load reduction, 29.7% better, RR 0.70, p < 0.001, treatment mean 3.03 (±0.19) n=125, control mean 2.13 (±0.19) n=128, day 5.
	relative viral load reduction, 15.4% better, RR 0.85, p < 0.001, treatment mean 1.43 (±0.2) n=125, control mean 1.21 (±0.2) n=128, day 3.
	risk of no viral clearance, 38.6% lower, RR 0.61, p = 0.12, treatment 15 of 125 (12.0%), control 25 of 128 (19.5%), NNT 13, persistently high viral load, day 7, primary outcome.
Dryden-Peterson, 10/27/2022, retrospective, USA, peer-reviewed, 7 authors, study period 16 March, 2022 - 31 May, 2022, average treatment delay 3.0 days.	risk of death, 85.7% lower, RR 0.14, p = 0.25, treatment 0 of 377 (0.0%), control 3 of 377 (0.8%), NNT 126, relative risk is not 0 because of continuity correction due to zero events (with reciprocal of the contrasting arm).
	risk of death/hospitalization, 43.0% lower, RR 0.57, p = 0.14, treatment 10 of 377 (2.7%), control 17 of 377 (4.5%), NNT 54.
	risk of hospitalization, 28.6% lower, RR 0.71, p = 0.53, treatment 10 of 377 (2.7%), control 14 of 377 (3.7%), NNT 94.
Kip, 4/4/2023, retrospective, USA, peer-reviewed, 16 authors, study period 8 December, 2020 - 31 August, 2022.	risk of death/hospitalization, 20.0% lower, RR 0.80, p = 0.65, treatment 6 of 157 (3.8%), control 15 of 314 (4.8%), NNT 105, omicron variant, day 28.
Lilly, 2/12/2022, Randomized Controlled Trial, USA, preprint, 1 author, study period 29 October, 2020 - 18 October, 2021, average treatment delay 3.6 days, trial NCT04634409 (history).	risk of death, 150.8% higher, RR 2.51, p = 1.00, treatment 1 of 252 (0.4%), control 0 of 128 (0.0%), continuity correction due to zero event (with reciprocal of the contrasting arm), combined bebtelovimab arms.
	risk of death, 200.8% higher, RR 3.01, p = 0.50, treatment 1 of 127 (0.8%), control 0 of 128 (0.0%), continuity correction due to zero event (with reciprocal of the contrasting arm), bamlanivimab, etesevimab, and bebtelovimab.
	risk of hospitalization, 27.0% higher, RR 1.27, p = 1.00, treatment 5 of 252 (2.0%), control 2 of 128 (1.6%), combined bebtelovimab arms.
	risk of hospitalization, 51.2% higher, RR 1.51, p = 0.68, treatment 3 of 127 (2.4%), control 2 of 128 (1.6%), bamlanivimab, etesevimab, and bebtelovimab.
	risk of hospitalization, 2.4% higher, RR 1.02, p = 1.00, treatment 2 of 125 (1.6%), control 2 of 128 (1.6%), bebtelovimab.
	risk of no viral clearance, 35.5% lower, RR 0.64, p = 0.07, treatment 33 of 252 (13.1%), control 26 of 128 (20.3%), NNT 14, day 7 persistently high viral load, combined bebtelovimab arms, primary outcome.
	risk of no viral clearance, 38.0% lower, RR 0.62, p = 0.13, treatment 16 of 127 (12.6%), control 26 of 128 (20.3%), NNT 13, day 7 persistently high viral load, bamlanivimab, etesevimab, and bebtelovimab, primary outcome.
	risk of no viral clearance, 33.0% lower, RR 0.67, p = 0.18, treatment 17 of 125 (13.6%), control 26 of 128 (20.3%), NNT 15, day 7 persistently high viral load, bebtelovimab, primary outcome.
Molina, 4/16/2023, retrospective, USA, peer-reviewed, 11 authors, study period 6 April, 2022 - 11 October, 2022.	risk of death, 57.0% lower, RR 0.43, p = 0.14, treatment 3 of 3,739 (0.1%), control 11 of 5,423 (0.2%), NNT 816, adjusted per study, odds ratio converted to relative risk, propensity score matching, multivariable.
	risk of ICU admission, 58.6% lower, RR 0.41, p = 0.05, treatment 6 of 3,739 (0.2%), control 21 of 5,423 (0.4%), NNT 441.
	risk of hospitalization, 55.5% lower, RR 0.44, p < 0.001, treatment 38 of 3,739 (1.0%), control 107 of 5,423 (2.0%), NNT 105, adjusted per study, odds ratio converted to relative risk, COVID-19, propensity score matching, multivariable.
	risk of hospitalization, 46.5% lower, RR 0.54, p < 0.001, treatment 48 of 3,739 (1.3%), control 116 of 5,423 (2.1%), NNT 117, adjusted per study, odds ratio converted to relative risk, all cause, propensity score matching, multivariable.
	risk of progression, 32.6% higher, RR 1.33, p = 0.001, treatment 260 of 3,739 (7.0%), control 275 of 5,423 (5.1%), adjusted per study, odds ratio converted to relative risk, ED visit, propensity score matching, multivariable.
Sridhara, 4/28/2023, retrospective, USA, peer-reviewed, 13 authors, study period 5 April, 2022 - 1 August, 2022.	risk of death, 85.7% lower, RR 0.14, p = 0.25, treatment 0 of 1,091 (0.0%), control 3 of 1,091 (0.3%), NNT 364, relative risk is not 0 because of continuity correction due to zero events (with reciprocal of the contrasting arm), propensity score matching, day 30.
	risk of death/hospitalization, 25.0% lower, HR 0.75, p = 0.31, treatment 24 of 1,091 (2.2%), control 28 of 1,091 (2.6%), adjusted per study,

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	propensity score matching, multivariable, Cox proportional hazards.
	risk of hospitalization, 11.1% lower, RR 0.89, *p* = 0.78, treatment 24 of 1,091 (2.2%), control 27 of 1,091 (2.5%), NNT 364, propensity score matching, day 30RETRO.

Supplementary Data

Supplementary Data

Footnotes

a. Viral infection and replication involves attachment, entry, uncoating and release, genome replication and transcription, translation and protein processing, assembly and budding, and release. Each step can be disrupted by therapeutics.

b. When administered late in infection, HCQ may enhance viral egress by further increasing lysosomal pH beyond the effect of ORF3a's water channel activity, thereby promoting lysosomal exocytosis, inactivating degradative enzymes, and facilitating the release of SARS-CoV-2 particles into the extracellular environment [151,152]. Research also suggests potential cardioprotective effects at lower doses, but cardiotoxicity with excessive dosage [153]. *Bobrowski et al.* also indicate negative effects if HCQ and remdesivir are combined.

c. *Peters (B) et al.* is subject to confounding by calendar-time (SOC evolved rapidly early in the pandemic, the linear covariate does not reflect non-linear SOC changes and hospital specific effects), hospital type (non-treatment hospitals were tertiary university centers), confounding by indication (4/7 hospitals initiated treatment on deterioration), immortal-time bias for as-treated (exposure assigned after baseline), significant differences for other experimental treatments, potential overadjustment from collider bias (steroid use and indication bias), limited baseline severity information, differences in hospice referral propensity across hospitals, unadjusted difference in time from onset to admission, difference in PCR positivity, and other factors. *Mahévas et al.* is subject to confounding by hospital (treatment highly dependent on the hospital, different SOC/ICU transfer practices, not included in PS), immortal time (only partly addressed in sensitivity analysis), co-treatment differences, calendar-time (SOC evolved rapidly early in the pandemic), binary coding for age (age ≥65 despite steep age-risk gradient), residual imbalance (variables dropped from PS), a composite outcome dependent on hospital triage/capacity, and other factors.

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