Learn to Walk Before You Run: A Case for Fundamental CME Research Utilizing Idealized MHD Models

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Synopsis

Coronal mass ejections (CMEs) are the largest energy release events in the solar system (together with flares), and an integral part of the dynamics of the Sun's corona and the inner heliosphere. Despite a plethora of unsolved science questions regarding these enigmatic events, it appears that focus has been gradually shifting in recent years from *fundamental* CME research (and its encouragement via solicitations) towards research that is predominantly driven by understanding and predicting space weather. For the CME numerical modeling community in particular, it has become increasingly important to evolve towards developing *practical* applications, such as community models or operational space-weather forecasting tools ("Research to Operations"). While we support the latter trend, we strongly caution against simultaneously neglecting fundamental CME research (and the utilization of simple, idealized MHD simulations), driven by the disputable perception that our knowledge of CMEs has sufficiently evolved to finally put it to 'practical use'. In this paper, we discuss the current situation and illustrate the enduring importance of fundamental CME research, and the usefulness of idealized models, based on selected unsolved science questions. To counteract the looming further decrease of fundamental CME research in our community, we recommend to (1) generally encourage and support fundamental research and 'simple' modeling approaches; (2) establish a new, focused funding opportunity for such research in the field of heliophysics; and (3) foster synergy on fundamental CME research between the solar and the plasma physics and stellar communities.

1 Introduction & Motivation

Coronal mass ejections (CMEs) are huge expulsions of plasma and magnetic field from the Sun's corona into interplanetary space [e.g., Webb & Howard 2012]. Together with the often associated flares and prominence (or filament) eruptions, CMEs constitute the largest energy-release events in the solar system. Furthermore, they have been recognized as the main drivers of space-weather disturbances around Earth and other planets [e.g., Baker & Lanzerotti 2016; Temmer 2021]. CME properties, especially those relevant to space weather (such as their speed, trajectory, and magnetic-field strength/orientation), can vary within large ranges. In many cases, these properties are largely set already during the *early evolution* of CMEs in the low and middle corona. Despite widespread research conducted since the discovery of these enigmatic events about 50 years ago, we still have only a relatively rough, qualitative (rather than quantitative) understanding of the processes and conditions that determine these properties [e.g., Manchester et al. 2017]. The same holds true for other fundamental processes such as, for instance, the initiation and driving mechanisms of CMEs [e.g., Green et al. 2018].

In recent years, the constant advance of computational capabilities, paired with the availability of high-resolution/high-cadence observations and a more complete coverage of CME evolution between Sun and Earth, have sparked an increased interest in space-weather research and prediction. Computational advances have also led to the development of semi-realistic magnetohydrodynamic (MHD) numerical simulations of CMEs, which incorporate real data and are capable of reproducing many features of observed events all the way from the low solar corona to Earth with increasing accuracy [e.g., Tóth et al. 2007; Manchester et al. 2014; Török et al. 2018; Downs et al. 2021; Jin et al. 2022]. We note, however, that these models still require substantial work time for setup, trial-and-error runs, and analysis. Therefore, they are still far from being run in real time for prediction purposes, albeit simplified versions (designed primarily for community use) are becoming increasingly less demanding [Jin et al. 2017]. Simultaneously, many empirical, semi-empirical, and other physics-based (MHD) models for the propagation of CMEs in the corona and heliosphere have been developed, and a considerable fraction of them are available at NASA's Coordinated Community Modeling Center (CCMC; `https://ccmc.gsfc.nasa.gov/index.php`). For a comprehensive review of such models, see, e.g., Zhao & Dryer [2014], Riley et al. [2018], and Vourlidas et al. [2019].

These developments have, in turn, led to a growing interest in (and to new funding opportunities for) "practical applications", i.e., to the desire to transform scientific models (including CME models) into tools that can be utilized for community use and, in the long run, for operational space-weather forecast (often referred to as "Research to Operations" or R2O). At the same time, however, it appears that *fundamental* CME research, and its encouragement via proposal solicitations, has been more and more neglected. There seems to be a perception that our understanding of CMEs and of their impact on the inner heliosphere has sufficiently evolved to justify transitions from "pure science" (including scientific modeling) to practical applications (see, e.g., NASA's recent LWS Strategic Capability solicitation). A decline of interest in (and less encouragement of) fundamental CME research is also apparent from the previous Decadal Survey, where none of the four "Associated Actions" recommended by the panel for SHP3 ("Determine how magnetic energy is stored and explosively released") addresses CMEs (three address energetic particles, one addresses forecasting/nowcasting of eruptions and space weather). Also, the decreasing number of CME sessions at scientific meetings over the past years (e.g., at the SHINE workshop, which was originally much more dedicated to CME research) illustrate this trend. Additionally, there seems to be a perception, at least in parts of the solar/heliospheric community, that relatively simple, idealized models ("toy models", as one unstated colleague once put it) are not appropriate for modeling CMEs and studying their properties, because they are "not realistic".

We would like to strongly caution against continuing this trend, for several reasons. First, research should be driven primarily by our desire to understand nature, rather than by practical utilization (after all, we are scientists, not engineers). Researchers, especially young ones, should not be discouraged to pursue their interest in understanding the physics of CMEs just because the science they have in mind may not appear "relevant enough" for space weather or may utilize "unrealistic" modeling approaches. Otherwise, we may risk not having researchers with a clear physical understanding of CMEs in a couple of decades. Second, shifting the focus too early or too much from scientific research to practical applications bears the danger of slowing down our progress in understanding the physics and, as a consequence, also the progress in developing applications. To quote David Deutsch: [1] «Any application, any forecast is based on insight. But if we abandon the search for fundamental explanations, and instead believe that it is sufficient to generate useful applications, then we merely progress from one decimal point to the next, and only in areas that are already well explored.» Third, as we will illustrate in §2, the conditions and processes that determine those CME properties that are most relevant to space weather are only poorly understood, especially in a quantitative manner. As a consequence, even the most sophisticated CME models presently rely on (often many) trial-and-error attempts to reproduce large-scale characteristics of observed CMEs (speed, trajectory, rotation, etc.), yet alone in-situ measurements. Fundamental CME research, especially when targeted at developing *quantitative* knowledge of these processes and conditions, can therefore greatly help to improve existing CME modeling approaches and, in the long run, our capabilities of predicting thier properties and impact. Fourth, state-of-the-art models of CMEs (and of the background corona and inner heliosphere) have become indispensable for many scientific investigations. However, because of their complexity and the rich physics involved, they are often not well suited for developing an understanding of the fundamental processes and conditions that determine essential CME properties, as that typically requires to isolate physical mechanisms and to perform systematic, parametric studies. For such tasks, "simple" idealized (i.e., data-independent) models, which allow one to isolate physical mechanisms, and which are computationally significantly cheaper, are much better suited. Nevertheless, state-of-the-art models, when available, should always be used to scrutinize the results and implications of idealized modeling studies under more realistic conditions.

To avoid potential misunderstandings: we appreciate that (some) funding opportunities by NASA and NSF exist that provide the possibility for proposing fundamental CME research, and we are not arguing against ramping up our efforts in transitioning existing models to community use or operational space-weather forecasting, as has been successfully done for WSA-ENLIL [e.g., Odstrcil et al. 2005], which is now used at the NOAA Space Weather Prediction Center and abroad. Nor do we argue against further improving state-of-the-art MHD models of CMEs, as many science questions can be addressed *only* with such models, and since, ultimately, they will play an important (if not the main) role for operational space-weather forecast in the future. All these developments are fully justified and should be further pursued. However, we believe that they need to be *complemented* by fundamental CME research that utilizes simpler approaches, to a larger extent than is presently done. Such research will be essential in the coming decade, not just for the enhancement of our overall scientific understanding of CMEs, but also for the development and continuous improvement of the practical applications mentioned above. In the next section, we present several examples that underline the importance of supporting and encouraging fundamental CME research in the years to come. In the final section, we present some recommendations that could alleviate the issues outlined above.

[1] Unprofessionally translated from an interview published in the German news journal "Der Spiegel" (4/2/22).

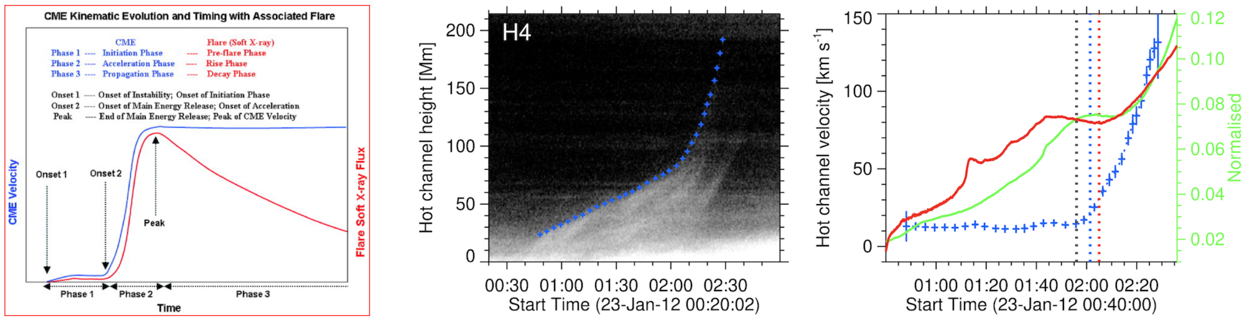

Figure 1: *Left:* Schematic plot of CME kinematics (blue) and associated soft X-ray flare (red), outlining three different phases [from Zhang & Dere 2006]. *Right:* Stack plot (SDO/AIA 131 Å) and rise profile of the eruption of a hot-plasma channel that produced a CME. Blue signs mark measured heights and speeds; the red/green line shows integrated soft X-ray/EUV flux [from Cheng et al. 2020]. Dotted lines mark flare onset (red) and transition to rapid acceleration from two different methods (blue, black).

2 Examples of Much-Needed Fundamental CME Research

To illustrate the need for a larger support of fundamental CME research, we present in this section a number of unsolved science questions, focusing on the initiation and early evolution of CMEs. As discussed above, we like to emphasize that such questions should be tackled predominantly with idealized numerical modeling approaches.

We note that a strong demand for extending fundamental research on CME initiation also exists in the growing field of stellar CMEs (see White Paper “Connecting Solar and Stellar Flares/CMEs: Expanding Heliophysics to Encompass Exoplanetary Space Weather" by Lynch *et al*). Furthermore, understanding the evolution and properties of CMEs in interplanetary space requires fundamental research as well (see White Papers “The Importance of Fundamental Research on the Upper Coronal and Heliospheric Evolution of Coronal Mass Ejections” by Lugaz *et al.* and “On the importance of investigating ICME complexity evolution during propagation” by Winslow *et al*). We also note that in this paper we focus on numerical modeling—observational requirements for CME research are discussed in the White Paper “New Observations Needed to Advance Our Understanding of Coronal Mass Ejections” by Palmerio *et al*.

While we strongly believe that CME research should be pursued in its own right, driven by our desire to understand nature, and independently of its potential space-weather-relevance, we purposely selected in § 2.2–2.4 science questions that are directly related to properties that determine the space-weather impact of CMEs. This was done to emphasize the fact that systematically tackling such questions will not just enhance our scientific understanding of CME physics, but also successively improve the quality of scientific and community CME modeling and, ultimately, our space-weather forecasting capabilities.

2.1 CME Initiation and Driving & Slow Rise Phase

CMEs stem from pre-eruptive configurations that consist of sheared and twisted magnetic fields (so-called filament channels) that are always located above polarity inversion lines. Typically, but not always, an eruption culminating in a CME is preceded by a slow-rise phase that is characterized by an approximately constant rise velocity of a few km s^{-1} (for larger prominence eruptions; typically lasting hours) to several ten km s^{-1} (for active-region eruptions; typically lasting minutes). The slow-rise phase then transitions into a rapid-acceleration phase, during which the eruptive flux gains speeds of a few hundred, in some cases more than thousand km s^{-1}, which is then followed by a propagation phase of approximately constant (typically slowly decreasing) speed [see Figure 1 (left) and, e.g., Zhang et al. 2001; Cheng et al. 2020]. There exits a broad consensus that CMEs (and the often associated flares and prominence eruptions) are magnetically driven [Forbes 2000]. However, the underlying physical mechanisms are still not well understood, which has led to a plethora of competing ideas and models, with some being more favored than others [see, e.g., Green et al. 2018]. Unfortunately, one cannot avoid the impression that progress in this field has significantly slowed down over the past years. It almost seems as if we have largely accepted the status quo and essentially gave up on the challenge of predicting the *onset* of solar eruptions (not just their space-weather consequences), at least as far as direct physical understanding (as opposed to pure statistics or AI) are concerned. In order to revive progress on this important topic, various unsolved questions regarding the initiation and driving of CMEs have to be tackled more vigorously. For instance:

(1) What is causing the slow-rise phase? For some time it was believed that this phase is merely a signature of a beginning instability that causes the subsequent eruption. However, recent observations often clearly show a prolonged slow-rise phase (Figure 1) with a substantial and constant velocity (about 13 $\mathrm{km\,s^{-1}}$ in this case). This strongly suggests that the physical mechanism at work in this phase is fundamentally different from the one acting during the rapid-acceleration phase, where the speed of the rising flux grows exponentially in the majority of cases [e.g., Vršnak 2001]. Hence, it seems that the mechanism (or mechanisms) acting during the slow-rise phase "prepare" (initiate) the system for eruption, by slowly evolving it towards the threshold of some other mechanism that drives the actual eruption (see below). Presently, we do not know which mechanism (or mechanisms) act during the slow-rise phase. Comprehensive and systematic efforts tackling this question are urgently needed.

(2) What are the thresholds of suggested eruption mechanisms? As mentioned above, a large number of mechanisms have been suggested over the past decades, out of which the most prominent are breakout reconnection [Antiochos et al. 1999; Lynch et al. 2008], tether-cutting (or flare) reconnection [Moore et al. 2001; Karpen et al. 2012], and the torus instability [Kliem & Török 2006; Kliem et al. 2014]. Unfortunately, our understanding of the onset conditions of these (and other) mechanisms is still largely of a *qualitative* nature. With the exception of the torus (and helical kink) instability, we have no *quantitative* knowledge of their specific thresholds. And even in those cases, the specific threshold can vary within a relatively large range, depending on the properties of the pre-eruptive configuration. Without such quantitative knowledge, it will remain very challenging to pin down which mechanism is at work in a specific eruption. Systematic numerical (and perhaps analytical) studies are needed to tackle this problem. Furthermore, studies that approach the question of eruption onset by considering volume quantities such as the free magnetic energy or magnetic helicity [e.g., DeVore & Antiochos 2005; Amari et al. 2011; Pariat et al. 2017] need to be further pursued.

(3) What are the respective contributions of these mechanisms? In contrast to the initiation or slow-rise phase, it seems that for the rapid-acceleration phase the relevant mechanisms have been boiled down to flare reconnection and torus instability [e.g., Green et al. 2018], supported perhaps by breakout reconnection for CMEs stemming from non-bipolar source regions [e.g., Aulanier 2014]. Moreover, it is very likely that these two mechanisms work together in most, if not all, eruptions, since they are closely coupled (see footnote on p.6). However, it is presently fully unclear which one dominates under which circumstances (e.g., quiet Sun vs. active regions), and what that means for the resulting acceleration and CME speed.

All these questions are very difficult to address with present-day observational capabilities. However, they are perfectly suited for well-designed, systematic parametric studies based on idealized MHD simulations. Moreover, since the relevant physics happens in the low corona, where the plasma beta is small, simplifying and computationally efficient approximations (zero-beta MHD) can be used. Consequently, corresponding efforts should be strongly encouraged and pursued in the coming decade. After all, almost all of our current knowledge on CME mechanisms stems from idealized analytical or numerical calculations.

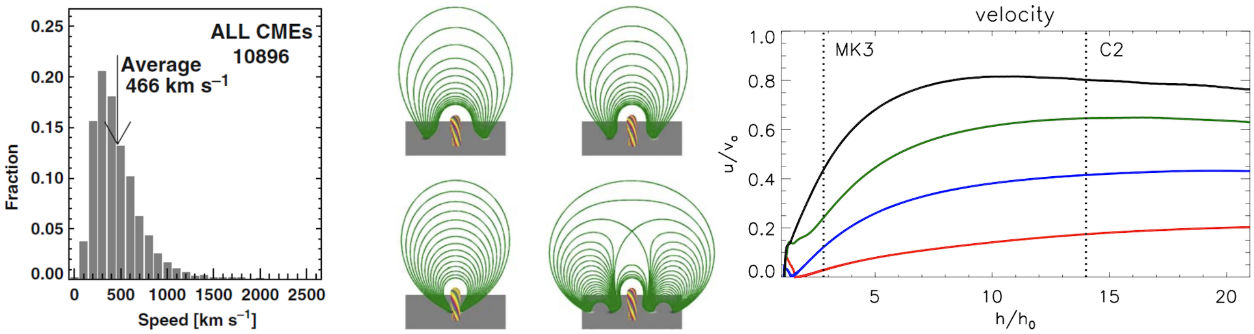

Figure 2: *Left:* Speed distribution of SOHO/LASCO CMEs 1996–2007 [from Gopalswamy 2010]. *Right:* Initial configurations and corresponding (normalized) CME speed as a function of (normalized) height in a series of idealized CME simulations [from Török & Kliem 2007]. The red, blue, green, and black curves correspond to the top left, top right, bottom left, and bottom right configurations, respectively.

2.2 CME Acceleration & Speed

Apart from the orientation and strength of its magnetic field, the speed of a CME is the most important parameter determining its space-weather impact, as it controls the CME's arrival time, the strength of its interaction with Earth's magnetosphere, and whether or not the CME will drive a shock. However, since CME speeds vary by more than one order of magnitude (Figure 2; left), it is crucial to understand the conditions that determine this quantity, especially since the fastest CMEs, which produce strong particle events and the strongest impact on the magnetosphere [e.g., Richardson & Cane 2011], are relatively rare (Figure 2; left).

Presently, we have only a very poor understanding of the factors that determine the CME speed (where we mean the speed obtain after the rapid-acceleration phase, i.e., the initial propagation speed). Many attempts have been undertaken to find strong correlations between the CME speed (as estimated from coronagraph observations) and observational properties of the associated source region (or combinations of these properties), so far with limited success.[2]

It is clear that the CME speed is determined predominantly by magnetic forces that act on the erupting flux during the rapid-acceleration phase, especially for fast events, which obtain most of their acceleration in the magnetically dominated low corona [e.g., MacQueen & Fisher 1983]. However, at the present time, we have very little quantitative knowledge of these forces, and how they (and the resulting CME speed) depend on the properties of the pre-eruptive magnetic configuration, the ambient (background) field and, perhaps, on the initiation mechanism. Acquiring such knowledge could provide, for instance, valuable input for models that aim to predict CME arrival times. Again, this is an area where well-designed, systematic numerical studies can be of great value, but only very few attempts have been undertaken so far (see right panel of Figure 2 for an example).

2.3 CME Trajectory (Rise Direction)

CMEs typically propagate away from the Sun in a more or less radial direction. However, their trajectories sometimes deviate from a radial propagation by several tens of degrees [e.g., Manchester et al. 2017], so that even events launched at disk center can miss Earth, leading to false space-weather alerts [e.g., Mays et al. 2015]. Such deviations predominantly take place in the corona, often below 5–10 $R_\odot$ [e.g., Isavnin et al. 2014], and have been associated with the "channeling" of the erupting flux by asymmetric source region fields [e.g., Möstl et al.

[2]We note that strong correlations found between CME speed and characteristics of the associated flare (e.g., reconnected flux) merely reflect the very efficient mutual feedback-coupling between the ideal flux-rope expansion (instability) and the flare reconnection, often referred to as "CME–flare relationship" [e.g., Vršnak 2016].

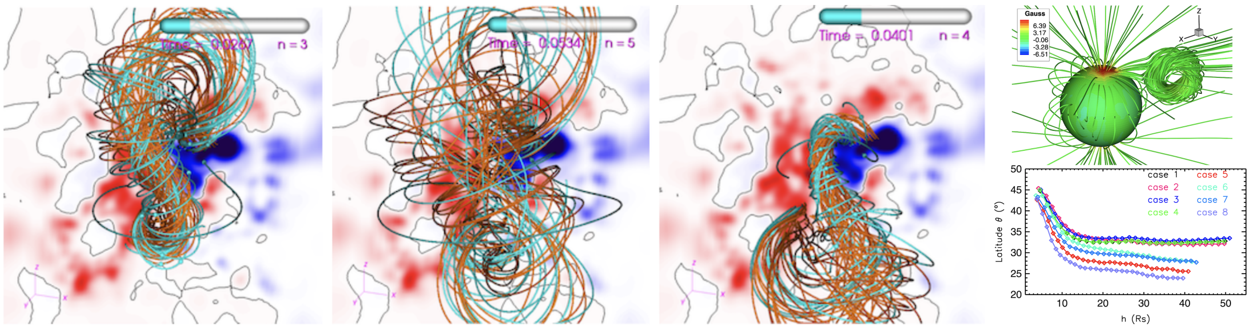

Figure 3: *Left:* Three different data-constrained MHD test simulations of a CME that took place on 29 November 2020 CME in NOAA AR 12790, exhibiting very different rise directions. Yet, the initial flux-rope configurations (not shown) differ only by very small modifications of the rope's axis shape, illustrating that the CME trajectory can be very sensitive to the choice of the initial flux-rope parameters. *Right:* Initial configuration (top) and trajectories for 8 different cases (bottom) in an MHD simulation study of CME deflection [from Zhou & Feng 2013].

2015; Liewer et al. 2015] or its deflection at coronal holes [e.g., Gopalswamy et al. 2009]. Back-of-the-envelope calculations suggest that CMEs are deflected towards regions of minimum magnetic energy density of the background field [Shen et al. 2011] and, based on a simplified description of the forces acting at the CME surface, a semi-analytical tool for calculating the deflection (and rotation) of CMEs has been developed [ForeCAT; Kay et al. 2015].

Despite these efforts, our quantitative understanding of this problem is still very limited. More sophisticated approaches are needed, especially since, as we illustrate for three test simulations in Figure 3 (left panels), CME trajectories can be very sensitive to the properties of the pre-eruptive configuration (for the same background field). Systematic MHD simulation studies that go beyond investigating single cases [e.g., Lugaz et al. 2011; Zuccarello et al. 2012] are needed to better understand and quantify the forces and conditions that determine CME trajectories, but such studies are still very rare (see right panel in Figure 3 for an example).

2.4 CME Rotation

The southward component of the magnetic field, $-B_z$, of a CME that hits Earth is the main parameter determining its geo-effectiveness, and predicting this quantity is the "holy grail" of space-weather research. In principle, a CME's magnetic orientation can be derived from observations of the pre-eruptive configuration [e.g., Palmerio et al. 2017]. However, CMEs occasionally undergo significant rotations of 120 degrees or more [see Manchester et al. 2017], a process that is best witnessed if the erupting flux carries a filament [Figure 4 (left); see also Green et al. 2007]. Such pronounced rotations significantly change the orientation of the CME's magnetic field, which is one important factor hampering our abilities to predict B_z.

CME (or flux rope) rotations have been associated, for example, with the helical kink instability [e.g., Török & Kliem 2003], the straightening of S-shaped field lines during eruption [Lynch et al. 2009, see also Figure 4; right], and the presence of an external shear field [Isenberg & Forbes 2007]. However, just as for the CME speed and trajectory (see above), we have a conception of how the underlying physics work, but a very limited *quantitative* knowledge of the detailed processes and forces. Consequently, just as for speed and trajectory, it is currently very difficult, if not impossible, to predict how much a potential CME may rotate, based on observations or modeling of the pre-eruptive state alone. Again, more comprehensive, systematic numerical studies are needed to alleviate this knowledge gap. CME rotation has been studied with MHD simulations for a few specific cases [e.g., Fan 2005; Lynch et al. 2009; Shiota et al. 2010; Zhou et al. 2022], but parametric studies still remain very rare [e.g., Kliem et al. 2012].

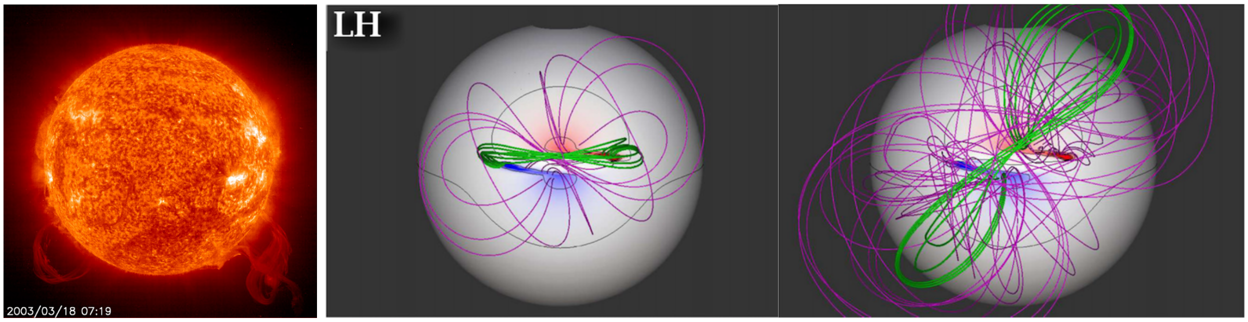

Figure 4: *Left:* Simultaneous eruptions of a loop-shaped and a helical prominence (SOHO/EIT). *Right:* MHD simulation of a rotating CME, shown at two consecutive times [from Lynch et al. 2009].

3 Concluding Remarks

In this paper we illustrated, based on a number of selected unsolved science questions, why we believe that strengthening fundamental CME research and the utilization of idealized MHD models for such research are urgently needed. In order to address this need, we recommend the following to the Heliophyiscs 2024 Decadal Survey Committee:

(1) From a broader perspective, we believe that fundamental research on CMEs should be encouraged more strongly, and become more 'decoupled' from space weather, in the sense that scientists (especially young ones), should not ignore or abandon a specific research topic they actually like, just because it is (or may be) not 'relevant enough' for space weather, or to come up in proposals with some potentially far-stretched justifications on why their research is important for it. Fundamental research should always be pursued without such pressure, as science should be driven not just by practical benefits for society and technology, but also by human curiosity and the desire to understand nature. Voicing such an encouragement can be easily implemented in surveys, road maps, or solicitations.

(2) More specifically, we recommend to establish a new funding opportunity that is specifically tailored for the development and utilization of idealized numerical (MHD) models to pursue fundamental research in the field of heliophysics (not just CME research, as that would be too narrow) that focuses on well-defined questions. Proposing such research to existing calls is somewhat challenging, as some programs tend to focus on rather applied science, while others are too large in scope or too general, or require also the utilization of observational data. We envision a smaller solicitation (∼100 k$ per year), but preferably with a longer than usual duration (five years) to accommodate work time needed for code and model development. Since the focus should be on well-defined science questions such as those outlined in § 2, a page limit of 10 pages for the technical section of proposals appears appropriate.

(3) In recent years, fundamental CME research has become important also in other science communities, most notably in the plasma physics and stellar physics communities. Plasma laboratory experiments increasingly address CME-relevant topics such as flux-rope instabilities or reconnection [e.g., Myers et al. 2015], and more recent setups [3] promise to provide the opportunity to mimic CME propagation (see White Papers by S. Dorfman *et al.* and E. Lichko *et al.*). On the stellar side, the numerical modeling of CMEs on active stars has seen significant progress, mostly based on techniques that were originally developed to model solar CMEs (see White Paper by B. Lynch *et al.*). This growing synergy should be fostered by establishing targeted funding opportunities for joined workshops and collaborative research programs.

[3] Such as the "Big Red Ball"; https://wippl.wisc.edu/big-red-ball-brb/.

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