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Synopsis: *The current pre-planned survey science model for Roman/WFIRST*[1] *is that of sub-$1B Probe-scale or Fermi-like missions. This is just not an appropriate model for a Hubble-like $4B Flagship Observatory-Class mission. Roman/WFIRST is failing to gain the support of the science community for good reason – because the pre-planned survey science approach is just wrong for such an expensive and capable mission. Roman/WFIRST will* ***not*** *do the best science circa 2026-2030+, nor will it strengthen and grow the young diverse community of scientists who will be poised to become the new leaders in the field. What Roman/WFIRST needs is our* ***gold standard*** *approach for doing the best science, i.e.,* ***contemporaneous scientific peer-review in 2025-30+, openly-competed across the full field of astronomy, starting ~1 year before launch.***

Dear Paul:

Thank you for your consideration of the thoughts that I, and others, have had regarding Roman/WFIRST over the last few months. I certainly appreciate your comments and questions, and the willingness of the Roman/WFIRST team to discuss the questions and issues that I raised. You have all helped me clarify my thinking. I initially expressed my concerns about the lack of community interest in Roman/WFIRST to you in my 2019 November 17 letter. My subsequent letter of 2020 January 24 dealt with the issues that make the Roman/WFIRST mission of little interest and not very relevant for most astronomers. I outlined some ways to change how the community felt about Roman/WFIRST in that letter. I was expecting those two letters to cover most of my input, but I have realized from the feedback I have been getting from a number of discussions that my second letter did not manage to properly convey a key aspect – how to develop a science program commensurate with the expected $4B LCC (Life Cycle Cost) investment in Roman/WFIRST. This letter focuses on that aspect.

(A) Introductory Comments

I know, and appreciate, that the NASA Program and Project science team worked hard with the Formulation Science Working Group (FSWG) and the Science Investigation Teams (SIT) to set the mission requirements and helped set the Project on a course to be a technically-successful mission. *It is my view that the Roman/WFIRST Project at GSFC is well-structured and has the management team in place that can carry this Flagship Project forward successfully.*

[1] Occurrences of "WFIRST" have been replaced by "Roman/WFIRST" for alignment with the mid-2020 name change to Nancy Grace Roman Space Telescope, widely abbreviated to "Roman" by NASA, while retaining consistency with the original document's use of WFIRST. Slight reformatting required as a result of the additional text.

However, the science model is failing to gain the support of the science community, because it is stuck back in the JDEM 1.5-m probe era. The current approach, one that derived from probe-scale missions, or sub-$1B scale missions like Fermi, is ill-advised. *These much smaller programs and missions are not a good model for a Flagship mission.* The pre-planned, narrowly-focused survey-science model needs to change to that appropriate for a $4B Observatory-Class Flagship to ensure that the powerful imaging capability of the current 2.4-m Roman/WFIRST becomes a scientific success for the whole astronomy community, not just a small subset of the community. The small-mission pre-defined science/survey approach will *not* generate the best science from Roman/WFIRST, it will *not* be responsive to the key science questions of the late 2026+ timeframe, and will *not* enable the new younger, and much more diverse, members of the science community to establish themselves as independent scientists in that timeframe.

I am, in principle, strongly supportive of Roman/WFIRST as a Flagship mission. As you know, I have spent much of the last 35 years of my career working to develop Flagship mission concepts while also making extensive use of such missions scientifically. Roman/WFIRST *can* be a powerful facility that will complement JWST and provide Hubble "Great Observatory-like" capability to the astronomy community. I expressed my enthusiasm for Roman/WFIRST in this role in public in my talks at the 2018 August Princeton Roman/WFIRST workshop. But I am *not* at all enthusiastic about Roman/WFIRST with its current science model that excludes broad astronomical community scientific involvement.

Dark energy and exoplanet microlensing are two important topics, and each elicits much interest, but to have them be given the vast majority of the time on a $4B Flagship mission is scientifically and politically unwise. The fraction of astronomers for which these are of <u>direct</u> interest is very small (<10% from Hubble statistics, see later). In particular, offering up just 25% of the observing time for broad community science is just not defensible. The vast majority of the time should be available to the astronomy community and chosen through peer review. Key projects could be used to allocate a fraction of the time to the prior Decadal goals, if that was felt to be desirable. But given the scientific advances to come through results from our new missions and projects, like ALMA, DESI, Euclid, LSST/Rubin, E-ELT, and particularly JWST, why are we forcing Roman/WFIRST into projects that are not now, nor will be, at the forefront of *direct* community interest in 2026-2030+.

I expand on these aspects in what follows. Let me start by clarifying the history of the Roman/WFIRST mission. You are aware of the background to Roman/WFIRST, but I suspect that many are not. And the history here is important, given that Roman/WFIRST had its genesis in a number of probe-class missions in the mid-2000s that led to the sub-$1B Joint Dark Energy Mission (JDEM).

(B) The JDEM Probe History and Context for Roman/WFIRST

The nature of Roman/WFIRST has evolved dramatically from the ~1.5-m JDEM to the 1.5-m Decadal Roman/WFIRST to an interim 1.3-m Roman/WFIRST to the way more powerful 2.4-m Roman/WFIRST/AFTA[2] mission.

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[2]AFTA – Astrophysics-Focused Telescope Assets – was the name given to the donation in 2012 of a pair of National Reconnaissance Office (NRO) 2.4-m telescopes. The donated 2.4-m nearly doubled the size of the then 1.3-m Roman/WFIRST primary. It changed the nature of the Roman/WFIRST program and opened up the opportunity to make Roman/WFIRST a true Flagship mission like Hubble/Chandra/Spitzer – but the science model did not similarly evolve to take advantage of the Hubble-like AFTA 2.4-m opportunity.

1) The JDEM Probe: Roman/WFIRST had as an antecedent the 2008 JDEM mission. JDEM was conceived as a "probe-class" ~1.5-m sub-$1B mission at L2 to further our insights into dark energy. JDEM itself had a number of probe-scale antecedents (e.g., the Supernova/Acceleration Probe – SNAP, the Dark Energy Space Telescope – DESTINY, the Advanced Dark Energy Telescope – ADEPT) that grew out of the Beyond Einstein study. The Astronomy and Astrophysics Advisory Committee (AAAC) 2005-2006 Dark Energy Task Force (DETF) study recommendations gave the JDEM approach enhanced credibility. The DETF study was one of several that were set up by the AAAC during its formative years while I was Chair of the AAAC. The JDEM probe-class mission provided the framework for the Roman/WFIRST mission in the 2010 Decadal Survey.

2) The Decadal Roman/WFIRST: The Roman/WFIRST that came out of the Decadal Survey was a mission with the same size 1.5-m primary as JDEM, but now with an added program to carry out an Exoplanet microlensing survey for planets. The cost rose into the $1.6B range, likely a more realistic cost for a 1.5-m telescope. Given the boundary conditions and cost constraints placed on the Decadal, Roman/WFIRST was largely an enhanced JDEM mission. The science model remained similar to JDEM as rather narrowly-focused survey science of the probe-class, though the Decadal Roman/WFIRST was now more akin to a “medium” Strategic mission in cost.

3) The Roman/WFIRST AFTA opportunity: When the Astrophysics-Focused Telescope Assets (AFTA) NRO 2.4-m mirror opportunity arose in 2012, the then 1.3-m Roman/WFIRST telescope program expanded to include a coronagraph. The near-doubling of the mirror diameter and the addition of a coronagraph broadened the capability of Roman/WFIRST, and enhanced the interest in Roman/WFIRST in the exoplanet community – helping to offset the negative reaction in that community to the original Decadal Survey microlensing addition. At first, the cost increment of the AFTA mirror approach was not expected to be very large (over the $1.6B estimate in the Decadal), but more insight during the Phase A and Phase B formulation process led to a more realistic cost for the Hubble-size Roman/WFIRST/AFTA. With the current Phase A-D cost (cap?) of $3.2B, plus reserves (HQ UFE), plus a coronagraph that is designated to be a technical demonstration as a pathfinder for future larger Flagship missions, as well as operations costs for 5 years, Roman/WFIRST is now an ~$4B LCC mission (and possibly more, given the impact of Covid-19). This is not unexpected, nor unreasonable, for a Hubble-sized Observatory with a very powerful wide-field camera and a coronagraph capability (albeit though now with a coronagraph of limited science usage given its designation as a Class D “technology demonstration” capability to contain the total cost of Roman/WFIRST).

AFTA provided an opportunity for Roman/WFIRST to become a mission in the “Great Observatory” class of Hubble, Chandra and Spitzer, and to help Roman/WFIRST recover from some challenges for policy-makers, and for the science community, that were inherent in the Decadal version of Roman/WFIRST. *Yet NASA and the science community failed to take scientific advantage of that opportunity.*

(C) Challenges to the Decadal Version of Roman/WFIRST

The Decadal Roman/WFIRST, in reincarnating JDEM in a different form, was seen as having two key problems. First, as a "Flagship" from the Decadal it was negatively cast (quietly) by many along the lines of "why bother with a 1.5-m when we have a 2.4-m Hubble", and was considered by a number of folks, in Congress and elsewhere, as very unlikely to move forward (DOA was the comment from some people). This was an unfortunate outcome, but understandable given the constraints on the Decadal. Second, the original Decadal Survey microlensing addition was clearly intended to provide a capability for the growing exoplanet community. Unfortunately, this effort was derided as being quite insufficient by many in that community, given the growth in the exoplanet field (again mostly fairly quietly). Given these reactions, the effort in NASA and elsewhere to find a way to gain support for Roman/WFIRST in the policy-maker and political arena became crucial. The Hubble-sized AFTA provided a key step towards making Roman/WFIRST a credible mission for policy-makers.

However, failure to subsequently change the scientific model to take advantage of the AFTA opportunity further undermined science community acceptance. Retention of the probe/Fermi narrowly-focused science conceptual model in the post-AFTA era made Roman/WFIRST of little interest to the broad community. Unfortunately, the FSWG and SIT structure set up in 2015 also fostered the feeling that Roman/WFIRST was being captured by relatively small group of people (~2-3% of the community) who would have the inside track to the observing time, to the science goals, to the resources, and to the scientific visibility and honors. *This was unfortunate, but the perception is now deeply rooted that Roman/WFIRST is "for them, and not for me".*

A key reason that Roman/WFIRST still exists is that several committees and many individuals were driven by a concern that if we failed to do Roman/WFIRST, the top-ranked recommendation of the 2010 Decadal, it would tarnish future Decadal recommendations and lessen one of the strongest arguments we have with Congress and the Administration for funding the major projects of the Decadal Survey. This is a valid concern. But it does not mean that we should be rigorously beholden exclusively to long ago Decadal science recommendations, and to make unwise decisions about the science goals, when we are spending $4B of taxpayer funds (particularly given that the Administration has now cancelled Roman/WFIRST three times!). *We can be responsive to the 2010 Decadal, to the Flagship opportunity brought about by the AFTA mirror-diameter doubling to 2.4 m from 1.3-m, to contemporary science circa 2026+, and to providing equal access and timely opportunity to the new members of our community, provided that we implement an appropriate science model.*

(D) Failures of the Current Science Model

While the evolution of Roman/WFIRST, from the ~1.5-m JDEM probe to the Decadal 1.5-m Roman/WFIRST to the very powerful 2.4-m Roman/WFIRST/AFTA mission, led to a substantial increase in capability (and, of course, cost) the science program model is stuck way back in the early pre-JDEM or JDEM concepts. This model involves pre-selected teams of scientists working to define the science program well in advance of the mission with present-day science goals – or, in the case of Roman/WFIRST, much older science goals. This approach may well be fine for sub-$1B-class space missions like JDEM or Fermi, or a sub-$1B project like LSST/Rubin, or PI missions – but it is quite inappropriate for a $4B Observatory-class Flagship space-science mission since it involves just a tiny fraction of the science community with a focus on very limited science goals. *In my view this is an extraordinarily bad approach for a $4B Flagship space observatory, for many reasons:*

1) It unreasonably restricts the science that could be undertaken. While the current science cases for Roman/WFIRST are narrowly-focused, the actual capabilities of Roman/WFIRST are broad and applicable to a huge range of science goals that are interesting to the astronomy and astrophysics community across nearly all fields. Unnecessarily restricting the scientific applications is unwise, since Roman/WFIRST investigations will be very limited in their ability to build on the evolving scientific opportunities of the late 2020s from the whole suite of new or upcoming missions and experiments, including DESI, ALMA, Euclid, LSST/Rubin, the E-ELT, and particularly JWST.

2) It does not provide the right opportunities for future scientists. By restricting the science and scientists involved, it does not provide growth opportunities for the remarkable developing talents of our upcoming young, diverse community of new scientists. Our efforts to broaden our community, to greatly enhance the number of women and minorities, is starting to bear fruit, but these new young members of our community will be short-changed by the current approach and will not be able to play a front-line role in Roman/WFIRST unless its science model is changed. This is of crucial and central importance as we work to enhance opportunities for women and minorities who are now in graduate school, or who will enter graduate school soon. As they mature scientifically into the mid-late 2020s the scientific opportunities in Roman/WFIRST would be set largely in stone when they are at the point of wanting to lead their own programs and projects. Admittedly the new young diverse scientists can access the archival data, but this is like throwing crumbs – the important opportunities for their careers will have been taken by older established scientists. *This is a terrible approach, both for science and for the new entrants to our field of astronomy.*

3) It is also likely to result in the program having reduced public visibility and appreciation. The current approach for Roman/WFIRST will not result in the frequent dramatic discoveries that have given NASA great visibility world-wide. What most excites the public and policy-makers are major new, quite-unexpected, serendipitous discoveries. Such Hubble, Chandra, and Spitzer discoveries have given great PR visibility worldwide to NASA and to astronomy – including the dramatic, unexpected discoveries in areas as diverse as dark energy and exoplanets (and numerous others). Ironically, it is these two largely serendipitous discoveries that now dominate a $4B mission with narrowly-focused science! Pre-planned science programs just do not return the unexpected, attention-grabbing PR results to the same degree. They do, of course, give results that gain a lot of visibility, as the HST Hubble Constant Key Project did, but these results are just not frequent enough for a $4B investment. There is a distinct contrast between the exciting results that NASA gets from something "entirely new", as opposed to the important, but infrequent, results from early pre-planned science (which has been characterized in some discussions about Roman/WFIRST as more like dotting "i's" or crossing "t's" – an obvious oversimplification, but an indication of how Roman/WFIRST is seen *vs* Hubble or Chandra or Spitzer). *The opportunities for PR-rich "unexpected serendipitous discoveries" will be greatly enhanced by minimizing early pre-planned science and ensuring that Roman/WFIRST has a dynamic, broad-ranging, contemporary science program.*

4) It will not provide the data processing/analysis support that the community needs. The use of SITs to provide data processing and analysis software is another aspect of the current approach for Roman/WFIRST that will not provide what the community needs. Support of the community through data processing capabilities and through analysis tools has been a key aspect of making Hubble, Chandra and Spitzer so successfully scientifically. We know that development and support of such tools is expensive and demands long-term commitments to ensure access and updates for all users. Unfortunately, experience has shown that the model whereby science teams develop software for the broad science community does not ultimately produce what the community needs.

In my 30+ years of working with major missions I have seen community science teams make remarkable discoveries using software and data processing developments for their own science. Science teams do a remarkable job for their own research goals. However, what such teams do not do at all successfully is provide broadly-based software and data processing resources for the extremely wide range of science activities that are of interest and value for the whole astronomy community. Nor do science teams provide long-term support. The SIT approach will *not* save money, it will *not* provide what the community needs, and it will *not* provide the needed long-term support. *In fact, the SIT approach will end up wasting a lot of money.* Experience has shown that what is needed is what science operations centers (SOC) provide – namely long-term software development efforts that are managed according to well-honed NASA standards, and supported for the mission lifetime. Community scientists and science teams can provide valuable input on methodology and algorithms and help develop a synergistic product, but the working focus needs to be on SOC data and analysis support for the community.

5) The value for the mission of the Level 2 technical requirements: While the science model is seriously flawed, I would like to note that the 2015 SIT process has, however, been valuable for providing a useful short-term science framework that enabled the technical requirements of the mission to be defined. The dark energy and microlensing science cases provided a "design reference mission" (DRM) from which the technical requirements could be drawn. While those science cases were narrowly-focused, the actual capabilities of Roman/WFIRST are broad and applicable to a huge range of science goals that are interesting to the science community across a wide range of fields. *I see no need for a fundamental restructuring or evolution of the mission technical requirements if a much broader science program approach is adopted that will be responsive to contemporary community science interests circa 2025+.* The only concern I have regarding the mission technical requirements is that they have been driven to extremes in some cases by unrealistic science requirements. This issue can be dealt with by ensuring that the Project is supported in changing Level 2 requirements when they prove overly challenging and costly.

6) It serves just a few percent of the astronomy community. *The bottom line for science is that Roman/WFIRST is currently a $4B mission that is of little direct interest to the astronomy community.* The primary science foci, dark energy and microlensing searches for exoplanets, are niches in direct science interest across the community. Only 3% of publications from Hubble over the past few years mention dark energy. Exoplanets is a rapidly growing and dynamic field with mention in some 10% of publications from Hubble, but the science from microlensing would be but a small fraction of that broad field. Are we really going to drive a $4B Flagship mission by science that directly enhances the career path and interests of just ~5% or so of the science community? That sets a dreadful precedent for an expensive Observatory. *This needs to change, if only for our credibility as a community in trying to get funding support from the Administration – which has now terminated Roman/WFIRST three times.* These repeated cancellations add significant risk to Roman/WFIRST.

(E) Developing a Science Model Suitable for Flagship Mission.

The current narrow, scientifically-restrictive approach serves just a small fraction of the community, and will continue to do so if the model does not change. Even more troubling, it excludes from the science selection just those people who are most important to include – the new young, diverse community of scientists who will be looking for ways in the 2025+ timeframe to build their careers and bring new approaches and ideas to the table. The way to do the best science is right at hand.

Contemporaneous peer-review, openly-competed across all science, is the gold standard for doing the best science. So how do we ensure that we utilize our gold standard approach? *Very simply!* We do what we have done for decades with Hubble, with Chandra and with Spitzer. We utilize a peer-review TAC process that occurs as close as possible to the mission launch and one in which the full gamut of science interests can compete openly to do the best science as seen by the science community at that time. This approach is far superior for an observatory like Roman/WFIRST of Hubble's level of capability and science opportunity than a model where the science is limited and defined in advance. This advance-definition limited-focus survey model may well be appropriate for small sub-$1B missions and telescopes that are more narrowly-focused in their capability, but it is *not* right for a $4B Roman/WFIRST. *Not at all*.

The argument may well be made that Roman/WFIRST is unique because of its wide-field instrument and so needs to be treated "differently" and to have advance planning for all of its "surveys". *This is a specious argument.* We are far too sensible about our expensive missions to not try to optimize the use of such facilities and to optimize the science returns. Clearly we would set our processes up to do science programs that would make optimal use of a wide-field imaging capability. This may necessarily involve longer programs, but not necessarily to the exclusion of shorter ones that are scientifically-justified. Many of the longer ones will be survey-like in nature and discussions about approaches to optimize their use of the Roman/WFIRST Observatory would be good to do in advance, as per the JWST Advisory Committee (JSTAC) recommendations for JWST. But I would repeat again that the majority of the science goals should *not* be set way in advance. The right way is through a peer review process that is built around *contemporary* science goals and is one which *involves the newest and youngest members of our community in 2026+*.

Key Projects: If there are some science topics that are felt to be important to do, peer-reviewed TAC-competed Key Projects can provide the framework, as was done for Hubble. This could be a way to accommodate the prior 2010 Decadal Survey goals, as I discussed in my January 24 letter. The “Great Observatories” (Hubble, Chandra, and Spitzer) have done major or Key Projects, but they have always been for a minority of the time. For Roman/WFIRST the dark energy field has moved on since 2010 with numerous projects that will make significant contributions to dark energy research (e.g., DESI, 8-10-m projects, LSST/Rubin, Euclid, JWST, E-ELT, etc.). It is unwise, and a sub-optimal use of an expensive Flagship, to devote the majority of the time to this one science area, especially given the extensive focus on dark energy from upcoming missions and projects.

Many of these aspects regarding optimizing the science use of Flagships were discussed extensively in the over 7 years that the JWST Advisory Committee (JSTAC) was in existence while I was Chair. The various letters that JSTAC wrote with recommendations for optimizing the use of JWST can be found here: https://www.stsci.edu/jwst/about/history/jwst-advisory-committee-jstac

The JSTAC discussed the Key Project approach extensively and found that there was no need to do Key Projects for JWST. Clearly Roman/WFIRST differs from JWST, and so aspects that are unique to Roman/WFIRST should be discussed further. Committees such as the usual NASA Science Working Group (SWG) for Roman/WFIRST and the STScI Roman/WFIRST Advisory Committee (WSTAC) that advises the Director of STScI, as well as in the broader committees like NASA's Astrophysics Advisory Committee (APAC) and the Astronomy and Astrophysics Advisory Committee (AAAC), can provide feedback. While the current SIT teams are completing their activities and being disbanded, *the worst possible approach at this point would be to assemble new SIT teams to yet again carve out pre-determined science years and years before launch.*

In particular, the current surveys (or any other pre-planned surveys) should *not* be pre-assigned a *majority* of the time on Roman/WFIRST. If this is done NASA will ***never*** convince the community that it is serious about Roman/WFIRST being a telescope that is focused on doing the best science in 2026-2031+, or being truly accessible to all through a peer-reviewed TAC process. *I cannot emphasize this strongly enough.* The pre-designation of science is a major mistake of the current approach to Roman/WFIRST and really must be clearly and definitively changed through announcements to the community. This is especially so given that the science goals were defined back in the 2005-2010 timeframe and take no account of progress to date from our remarkable Great Observatory missions or from our numerous powerful ground-based observatories. These pre-determined science goals will also, and most importantly, ignore the results that will come in the next 6-11 years from 8-10-m telescopes, ALMA, DESI, JWST, Euclid, LSST/Rubin, and the E-ELT.

(F) Summary

Roman/WFIRST has the potential to be an exciting Flagship mission doing cutting-edge science by bringing the imagination of the community to bear. Roman/WFIRST can be a powerful Hubble-Class Flagship Observatory with a remarkable wide-field camera. Yet Roman/WFIRST is failing to be seen as a project worth spending $4B on by the science community. Why? Because the 2.4-m Roman/WFIRST/AFTA is being set up as a pre-planned limited-science survey mission that will serve a tiny fraction of the science interests of the astronomy community. Furthermore, it is a mission that is seen to have been captured by just a few percent of the community -- who are seen as the "haves" and "insiders". This clearly is not what we should be doing for a powerful $4B Flagship.

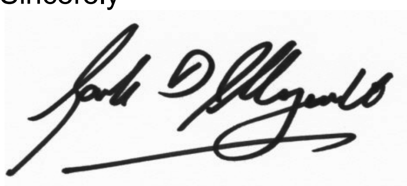
How did we get into this situation? Roman/WFIRST/AFTA had its genesis in the mid-2000 timeframe when a number of probe-class missions were rolled up into a sub-$1B JDEM. JDEM was conceived to be a 1.5-m dark energy survey instrument, responsive to the Beyond Einstein process. This grew slightly in science scope in the Decadal survey, but still with a similar size mirror. When the AFTA opportunity arose, and WFIRST grew from the then 1.3-m to 2.4-m, its path was set to become a Flagship. The science opportunities should have expanded greatly beyond those for just a small-scale survey telescope. ***Yet the science opportunities remained like that for sub-$1B Probe or Fermi-like missions.*** *The Roman/WFIRST science program is of direct interest to just a tiny fraction of the science community (~5%) and is seen to have been given to just a few percent (~2-3%) of the science community, as represented in the SITs and FSWG. While this group has done valuable service by defining detailed science approaches that have provided the technical requirements on Roman/WFIRST, the FSWG and SIT teams are seen as controlling the science program for Roman/WFIRST. Failing to take the AFTA opportunity to expand the science scope was a mistake that has led to the community seeing the now $4B Roman/WFIRST as a telescope for the "haves" and "insiders" and not as an Observatory that would provide access for the full-community by enabling cutting-edge contemporary science in the 2026-2031+ timeframe across all of astrophysics. The science model for Roman/WFIRST/AFTA should be Hubble/Chandra/Spitzer, not JDEM, or the similar probe-like capability of the WFIRST/Decadal, or of other sub-$1B scale missions like Fermi. We need to change Roman/WFIRST to a model of contemporaneous peer-review in the 2026+ timeframe, openly-competed across all astronomy, since that is the* ***gold standard*** *for doing the best science.*

Key projects, covering a small fraction of the time, could be used to deal with any science areas that might, as we get closer, be seen as needing special treatment. But the justification would need to be exceptional to do so. For JWST, as recommended by the JSTAC, it was felt that the science goals that have long been used to justify and "market" JWST did not need special treatment. If they are of overwhelming contemporary science interest they surely will be selected through a well-designed TAC process.

I would like repeat my assessment based on decades of experience leading committees like the AAAC in its formative years, and the JSTAC, as well as being deeply involved with Flagship mission concepts and development over several decades, particularly for Hubble and JWST – Roman/WFIRST *can* be a powerful facility that will complement JWST and provide Hubble "Great Observatory-like" capability to the astronomy community. However, the current pre-planned survey science model, diverging as it does from *broadly-based contemporaneous science peer review to establish the science program*, is a mistake in a $4B Flagship Observatory that has Hubble-like power for doing cutting-edge science. For enabling our future Flagship missions (cf., HabEx, Lynx, Origins, LUVOIR), as well as for justifying the cost of Roman/WFIRST, Roman/WFIRST has to be seen as one of our ensemble of Great Observatories that have opened up new scientific frontiers and also provided new opportunities for young scientists through contemporary peer review!

The current narrow pre-planned survey science approach for Roman/WFIRST will not achieve these goals, and leaves Roman/WFIRST at significant risk of cancellation.

Sincerely



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Attached prior 11/17/2019 and 01/24/2020 letters:
Roman/WFIRST Community support
Making Roman/WFIRST into a Great Observatory

cc:	Jeanne Davis	Astrophysics Division, SMD, NASA HQ
	David Jarrett	Astrophysics Division, SMD, NASA HQ
	Dominic Benford	Astrophysics Division, SMD, NASA HQ