What Comes Next for LIGO?
Planning for the post-detection era in gravitational-wave detectors and astrophysics

V.4 (Final) Report from the DAWN-II Workshop, Atlanta, GA, July 7-8, 2016
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1 Preface

On July 7 and 8, 2016, over 60 gravitational wave scientists, astronomers, and astrophysicists from around the world gathered at the Georgia Institute of Technology, in Atlanta, GA, for the workshop *"What comes next for LIGO"*, to begin planning for the post-detection era in ground-based gravitational-wave detectors and astrophysics. This was the second year that this workshop has been held: between the two meetings the field of gravitational wave astrophysics experienced the long-awaited transformation from possibility to reality.

With the 11 February 2016 announcement by LIGO of the detection of GW150914, the era of gravitational wave astronomy crossed a historic threshold[1]. This first gravitational wave event fulfills a decades-old promise to open new avenues for astrophysical exploration informed by direct observation (in both the amplitude and phase) of gravitational waves. The 2nd generation Advanced LIGO detectors that brought success were first envisioned almost a *quarter century ago*, at the time when LIGO was first proposed. The Advanced LIGO detectors and upgrades to the basic platform will serve to establish our new astronomy, but after some 15 years – in the 2030s – will reach the limits of improvements and will have probed to its limit. Thus the community believes that now is the time to start to plan how to build a more sensitive 3rd generation detector network to probe the gravitational wave Universe to higher redshift.

The one and a half day workshop consisted of three sessions devoted to different aspects of planning for the future of the GW field:

- Session A: Advanced LIGO near and longer term future;
- Session B: The international network and multi-messenger astronomy;
- Session C: Long term planning by the international community for 3rd generation detectors.

Each session included several presentations followed by brief discussion periods. The remainder of the session was devoted to a moderated, focused discussion affording participants ample opportunity to pose questions, make comments and other suggestions. The sessions were organized and led by the members of the Scientific Organizing Committee: Dave Reitze (Session A), Laura Cadonati (Session B), Albert Lazzarini and Beverly Berger (Session C). The main conclusions and recommendations from the workshop are summarized in the Executive Summary, Section 2. The remaining sections of this report present focused discussions on each session. Presentation materials from the workshop are available online [2].

This report is being shared with the broader community. This document presents the discussions and conclusions from the meeting, and represents the views of those participants at the workshop and other interested colleagues who reviewed the document and endorsed it[1]. Earlier in 2016 many of the participants had the opportunity to begin a serious dialog on the path forward for 3rd generation detector upgrades in an international setting [3, 4]. This DAWN-II workshop allowed the dialog to continue productively, leading to the conclusions and recommendations appearing below.

Albert Lazzarini, on behalf of the Scientific Organizing Committee and the workshop participants.

SOC:
Beverly Berger
Laura Cadonati
Marco Cavaglià
Gabriela González
Albert Lazzarini (Chair)
Dave Reitze

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[1] These individuals are listed in Appendix A

2 Executive Summary

The key recommendations coming from the examination of the near and longer term plans for Advanced LIGO are:

- The A+ upgrade program is a high priority near-term goal once Advanced LIGO has achieved its design sensitivity and has carried out a long duration (one to two year) observing run. The participants endorse the A+ design and urge that LIGO Laboratory and its partners embark upon the upgrade program as soon as feasible.

- There is a strong scientific rationale for implementing the A+ upgrade on LIGO-India before it begins science operations. Discussions should begin soon among the LIGO Laboratory, NSF, and the LIGO-India management team on how to plan for and fund a LIGO-India A+ upgrade.

- Continued focus on improving coating thermal noise is essential for *all* future gravitational-wave detector incarnations. To realize A+ and future detectors such as LIGO Voyager, an adequately funded, focused, and well-coordinated R&D effort on producing coatings with lower thermal noise is urgently needed.

The principal observations and recommendations coming from the session on medium/short term planning of the international network and Multi-Messenger Astronomy include the following:

- Detector performance is a multi-dimensional consideration. The community should identify a set of performance metrics for future planning, beyond the single space-time volume metric (V×T) that is now used. Examples might include metrics that emphasize the localization and discovery potential of a detector network.

- Prioritize detector improvements when planning commissioning breaks, upgrades, and observing runs, keeping the science potential as a target at all times.

- Tighter research coordination among the major projects is beneficial to future detector development. LIGO has well developed plans for the A+ upgrade program, and Virgo has written a preliminary concept document for enhancements to Advanced Virgo. The LSC INS white paper is a good example of a coordinated R&D program. Can this serve as a model for a coordinated, multi-collaboration network program?

- What are the barriers to establishing a coordinated international research effort? Allocation of sufficient funding and coordination of funding at both the agency and the project levels is needed. Achieving comparable sensitivities requires investments from multiple agencies; neither coordination nor funding will come without strong advocacy from the community with a strong, compelling science case.

- A more formal structure for regular interaction and coordination of the community is needed. This recommendation also arose in the session on long term planning and will be discussed further below.

- In order to be ultimately successful, the GW community must reach out beyond its borders, to develop advocacy among astronomers, cosmologists, geophysicists, materials scientists, etc.

Electromagnetic (EM) follow-up:

- This advocacy can be best fostered with more rapid communication of all GW information available, including sky maps, significance, distance and event ID, etc.: e.g., as a goal, issuing the first trigger with a "preliminary" flag (30 min or less) followed up with subsequent information (e.g., refined false alarm thresholds) when available. The GW community should be less conservative and allow for corrections in followups (even calling back triggers if needed).
- The GW community should continue to pursue MOUs for focused science targets/papers even in the era of open triggers.

The principal observations and recommendations coming from the session on long term planning by the international community for next-generation detectors include the following.

The international GW community should begin the process of global planning and establishing community-wide buy-in of the requirements and approach to meeting them with a 3$^{\mathrm{rd}}$ generation network of detectors. The scientific motivation must be clear, strong, and widely appreciated by the public, the broader scientific community, and by government funding agencies. It is essential to work together globally early enough to build global ownership of the design (or designs) and its implementation plan including a clear understanding of a validated cost estimate with a plan for cost risk mitigation. In the U.S., the timescale for having sufficient information available would be the next DAWN Workshop, at which time the NSF may consider beginning the process of developing a charter for an independent external study.

The Gravitational Wave Agencies Correspondents (GWAC[5]) was established informally by the NSF in 2015 after the first DAWN workshop in order to provide an inter-agency forum within which a direct channel of communication between funding agencies may be used to coordinate the use of existing funds and to explore new funding opportunities for the gravitational wave science community. The Gravitational Wave International Committee (GWIC[6]), was formed by the leaders of the various projects in 1997 to facilitate international collaboration and cooperation. These two entities, one representing the funding agencies and the other representing the major GW projects, represent the appropriate international forum within which to forge new consortia and to plan for future large-scale projects. **The GWAC and GWIC should engage to coordinate an international R&D effort in the critical technologies for next generation ground-based GW detectors.** There are different modalities for successful progress on this front. This recommendation was also made in the session on near-term planning for the international network.

Edited By David Reitze

3 Session A - Advanced LIGO: Near and Longer Term Future

3.1 Introduction

As foreseen in the first DAWN Workshop report [7], the first detections with ground-based gravitational wave detectors have been transformational. With just two confident detections of gravitational waves, a great deal has been learned about the nature of the gravitational-wave universe[1][8]. We can now say confidently that binary black hole (BBH) systems and 'heavy' stellar mass black holes exist, as well as constrain the merger rate of stellar mass BBH systems[9]. These detections were accomplished during the 'O1' Observing Run, when Advanced LIGO was operating at approximately one-third of its design sensitivity as measured by the sky and orientation averaged distance to which a $1.4 M_{\odot} + 1.4 M_{\odot}$ binary neutron star (BNS) coalescence could be detected with SNR=8. Still as discussed in the next section, there is much still to explore, especially in systems involving matter that are expected to have EM counterparts capable of providing complementary information and insights.

Through 2020, continued improvements in Advanced LIGO sensitivity will increase the astrophysical reach of the LIGO Observatories. During the same timeframe, the Virgo and KAGRA[10] detectors will finish construction and commence science operations. This session of the DAWN-II workshop focused primarily on exploring the plans for the near term evolution of the Advanced LIGO detectors. Presentations covered how the LIGO detectors might progress over the next ten years, including:

- Near term plans and activities to commission Advanced LIGO to design sensitivity (Daniel Sigg)
- An assessment of the new astrophysical knowledge derived from the detections – what was learned about the gravitational-wave sky in O1 and what science goals might be reached for O2, O3, and beyond (Salvatore Vitale)
- Detailed plans and timelines for upgrading Advanced LIGO beyond its design sensitivity (Mike Zucker)
- An overview of coating thermal noise, the primary technological bottleneck to improving interferometer sensitivity in the near term (Marty Fejer)
- A conceptual design for Voyager, an interferometer operating at the sensitivity limits of the current LIGO Observatories (Rana Adhikari).

The session concluded with a general discussion and assessment of these near term priorities.

3.2 The Immediate Future: Achieving Advanced LIGO Design Sensitivity

Once constructed and installed, the Advanced LIGO interferometers were rapidly commissioned to sensitivities well beyond those obtained with the initial LIGO interferometers, achieving ∼70-80 Mpc BNS inspiral range (at the upper end of the sensitivities anticipated for this initial observing period) before the O1 run began. These sensitivities, although capable of making the first detections, were not as good as that predicted by 'noise budget' models of the interferometers' design performance capability, specifically deviating in the 20 - 100 Hz ('low') frequency region [11]. The deficit between achieved and modeled performance at low frequencies has a significant negative impact on the BNS and BBH detection ranges and on the extraction of astrophysical parameters from the detected waveforms.

Since the end of O1, a systematic program of investigations has been underway to understand and reduce the discrepancies between the models and the actual performance. Known contributors to excess low frequency noise include the auxiliary length degrees of freedom and the angular sensing and control system. A large number of sources of scatter have been identified and mitigated. Couplings to laser noise sources have been identified. However, there remains an unexplained excess above the sum of currently understood noise sources in the 40-80 Hz frequency range. A number of noise couplings have been investigated and ruled out, among them coating thermal noise, laser noises, test mass charging, suspension electronics, and a variety of potential environmental couplings. Still not ruled out are squeezed film damping between the end test masses and reaction masses, exotic scattering mechanisms, and a few other possibilities.

Noise sources other than quantum (or shot) noise are negligible above 100 Hz. The LIGO interferometers operated with 25 W input laser powers in O1; increasing the laser power from 25 W to 50W and achieving stable operation is a major commissioning goal focus for reaching the aLIGO design sensitivity. Higher laser power comes with its own set of challenges. Parametric instabilities, alignment instabilities, and thermal effects become more prevalent and problematic, requiring significant commissioning efforts to suppress them and obtain reliable, stable operation and higher operational duty factors[12][13][14].

Sensitivity targets for O2 (2016-2017) and O3 (2017-2018), established in 2013, aim for ∼100 and 150 Mpc BNS ranges, respectively[15]. While it is conceivable that the O2 target can be met solely by increasing the laser power, reducing the low frequency noise is critical to achieving the O3 goal, and will greatly increase the O2 astrophysical reach. *Thus, commissioning to improve the low frequency sensitivity remains the highest priority for the Advanced LIGO commissioning team. The next highest priority is improving the uptime by increasing robustness against environmental effects that cause loss-of-lock.* It must be noted that the difficulty of realizing or surpassing the low frequency goals rises precipitously as the low frequency cut-off is decreased – 10 Hz is much more challenging than 20 Hz, which in turn is much more challenging than 30 Hz – making an understanding of the key astrophysics goals important.

3.3 Gravitational-wave Astrophysics Through 2020

The first two detections have already given us surprising insights into the gravitational-wave universe. Beyond confirming the existence of BBH systems, the progenitor of GW150914 contained black holes more massive than those measured in X-ray binaries (XRBs) with reliably determined masses[16]. GW151226 produced mass-weighted measurements of black hole spin components aligned with the orbital angular momentum[8]. Comparison of measured waveforms from both detections with those expected from general relativity (GR) enabled the first tests of GR in the strong gravity regime[17].

This new gravitational-wave platform for studying the universe permits a panoramic view of the astrophysical and gravitational science we can expect in the next decade. While the first detections provided clues about the formation mechanisms of BBH systems, the dominant formation channels (be it isolated binaries, triples or dynamical processes or combinations of these) can be ascertained from a population survey of black hole masses and spins obtained from mergers along with distances and inferred merger rates. Formation mechanisms will be better constrained in subsequent runs as sensitivity, and therefore science reach and range, continues to improve: we expect to further refine those constraints on individual masses[18] and hopefully identify the channel(s) involved. We expect that in the coming years *observational* black hole astrophysics will become an active field, supported by the GW measurement of potentially hundreds of black holes.

There is also the exciting potential of detecting systems even more massive than GW150914: detecting a 100 $M_\odot$ black hole, thus providing the first experimental evidence of the existence in Nature of intermediate mass black holes (IMBHs) will be a significant milestone for the field.

Detecting BNS and NSBH mergers remain a core science goal of Advanced LIGO. BNS detections will establish merger rates for BNS systems; both BNS and NSBH mergers will better determine the neutron star mass spectrum. Determining the neutron star equation of state (EOS) is a key goal of gravitational-wave astrophysics, but will likely not come before the end of the decade (in O3 or beyond), as it requires a large number of moderate SNR BNS/NSBH detections or a few 'loud' detections[19][20][21].

The detection of a galactic core collapse supernova (CCSN) by LIGO-Virgo-KAGRA[2] would significantly improve our understanding of underlying physical mechanisms which drive CCSNe explosions[22][23]. While CCNS waveforms are much more difficult to model, the accompanying neutrinos and (possibly) optical counterparts combined with a statistically signifiant detection using 'burst' algorithms would provide high confidence in a CCSN detection. The challenge here is rates given current estimates of signal strengths, signal content, and instrument sensitivity – only 1/century/galaxy with large uncertainty. Thus, a CCNS detection in the next ten years with the current detectors is likely to be dictated by random chance.

Isolated spinning neutron stars will emit continuous gravitational waves if they possess intrinsic ellipticity arising from crustal deformations, internal hydrodynamic modes, or free precession ('wobble'). Analyses from initial LIGO and Virgo have produced limits on gravitational-wave emission for the Crab and Vela pulsars that are well below the spin-down limit. Emitted gravitational-wave amplitudes will likely depend sensitively on the neutron star EOS[24][25]. It is difficult to predict when detection of this source will likely occur; however it is possible that the first detection could occur in O2.

The determination of more precise BBH merger rates from the first detections has resulted in a refinement of the magnitude of the energy density of the confusion-limited stochastic gravitational-wave background (SGWB) from prior BBH mergers[26]. Indeed, it may be possible with Advanced LIGO, when operating at design sensitivity, to detect the BBH SGWB using year-long integration times. Further investigations of the BBH SGWB could

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[2] CCSNe are weak gravitational-wave emitters. Even the most optimistic models for GW emission limit detectability to the Milky Way galaxy with current detectors

reveal key information about the metallicities and star formation rates, but this will likely require new facilities and 3$^{\text{rd}}$ generation detectors. The detection of a primordial SGWB would be truly revolutionary for cosmology; however current inflation models render that possibility very unlikely.

The impact of the discoveries and near term science goals on detector commissioning and future detector design was discussed at some length by the workshop participants. *A strong consensus emerged that gravitational-wave astrophysics would benefit most in the near term from improving the detector sensitivities at low frequencies for a variety of reasons,* among them expanding the spectrum of detectable BBH mergers to higher masses, improved parameter estimation from the increased time signals spend in-band, improved sensitivity to the SGWB, and expanding the number of detectable continuous wave sources[27].

3.4 Into the Next Decade: Advanced LIGO Plus 'A+'

It is a central tenet of the gravitational-wave community that a continuous program of sensitivity improvements is critical to the field, as improvements in sensitivity produce outsized gains in the rate of detectable sources (since the increase in rate scales as the cube of the improvement in sensitivity). In addition, the ability to extract the underlying astrophysics from detected events improves with sensitivity, due to increased SNRs for a given population of events. Once Advanced LIGO design sensitivity is achieved (anticipated by 2020), further enhancements in sensitivity must come from upgrades to the interferometers.

At last year's inaugural DAWN workshop, a number of paths to sensitivity beyond the aLIGO design were presented and explored, including quantum squeezing, improved coating thermal noise (CTN), dynamic gravitational gradient ('Newtonian') noise cancellation, and improved suspension thermal noise. At this year's workshop, a detailed program of upgrades for the LIGO interferometers, named 'A+' was presented to the community. The A+ design incorporates many of the elements identified in the first DAWN white paper [7], to produce a design with strain sensitivity corresponding to a BNS/BBH range of $\sim 350/2240$ Mpc. This would lead to an increase in range of 1.6X and 1.8X respectively for BNS and $20 M_{sun}$ BBH mergers, or alternatively a detection rate increase of 6.4X (BNS) and 4.4X (BBH) with respect to Advanced LIGO.

The A+ design improves the sensitivity in all frequency bands. At the heart of A+ are frequency-dependent squeezing[28] and improved test mass coatings. Squeezed light will be used to reduce the quantum noise at low frequencies through the reduction of radiation pressure on the test masses and at high frequencies ($>\sim 500$ Hz) through the reduction of shot noise. Improvements at low and mid frequencies also rely on a reduction of coating thermal noise by a factor of two over current Advanced LIGO mirror coatings. Balanced homodyne detection is planned, to provide a lower loss, higher fidelity readout of the gravitational wave channel over DC readout.

The concepts behind many of the planned A+ upgrades have already been experimentally demonstrated, either on large scale interferometers or at laboratory scale. At this point, many of the primary challenges are on developing engineered solutions which can be deployed in A+. The one notable exception is the production of optical coatings with reduced thermal noise; this remains a vigorous area of R&D in the LIGO Scientific Collaboration Coatings Working Group. (The challenge of producing large aperture low thermal noise mirror coatings is discussed in more detail below.)

A preliminary cost and schedule has been developed by the LIGO Laboratory Systems Engineering Group. Assuming funding begins in fiscal year 2019, a five year upgrade program is envisioned, with a one year installation period beginning in 2022 followed by a one year commissioning period starting toward the end 2022, and the first A+ observing run occurring in late 2023. The preliminary budget for the A+ upgrade is approximately $29M, and funding will be sought from the NSF Physics Division ‘mid-scale’ program. At the current cost, the A+ budget exceeds the foreseen funding for individual Physics Division ‘mid-scale’ projects. LIGO Laboratory may also engage its international partners who contributed to the Advanced LIGO construction project to explore whether similar in-kind contributions are possible with the A+ upgrade; these would defray US-borne costs.

Workshop participants noted that plans to initiate an A+ upgrade program for LIGO-India have not yet been established. Once the A+ upgrade program is completed, a significant disparity in sensitivities would exist between the US and India interferometers if a similar upgrade is not undertaken for the Indian instrument. There is a strong scientific rationale for making LIGO-India identical in configuration and performance to the US LIGO interferometers. LIGO-India project planning is still in the formative stages; thus it would beneficial and cost effective to build the A+ upgrade into the LIGO-India interferometer plans *now* rather than upon completion of LIGO- India (which would be both disruptive to operations and more costly). *Discussions should begin soon with LIGO-India on how to plan for (and fund) a LIGO-India upgrade.*

LIGO, Virgo, and KAGRA will be in observing mode and making frequent detections during the period when LIGO-India is planned to become operational in 2024, at about the time A+ will come online. The Virgo commissioning program has planned for achieving design sensitivity in 2021 or 2022, although it is still early in the Virgo commissioning phase so these estimates may change. Virgo is also looking at a ‘V+’ upgrade, implementing squeezing and other technologies sometime in the next five years, creating an opportunity for near-term common efforts in technology and adoption of common designs. KAGRA is still in the very early stages of planning its commissioning program, however achieving design sensitivity by 2022 is a possibility. Three operating instruments are a minimum to continue network observation in this epoch. Thus, there could be tension between observing and taking the detectors offline to upgrade them. In the future, careful consideration of how to interleave the A+ upgrade with network observational time periods will be required. Moreover, if developing coatings with lower thermal noise proves more difficult than anticipated, it is possible that the A+ upgrades may have to be staged, adding further complexity to the planning.

3.5 The LIGO Voyager Concept

While A+ will dramatically increase the rates of certain classes of gravitational-wave sources and enable the first detections of others, the fundamental facilities limits of the LIGO Observatories can accommodate interferometers with even greater sensitivities. A factor of two to three can be gained in sensitivity over A+ in a broad frequency range with the current LIGO observatory infrastructure based on designs using our current level of understanding of other limits to sensitivity.[3]

The Voyager design aims to reach the extant LIGO facilities limits through a major upgrade that incorporates several new ideas and technologies. Key to the design is the use of 200 kg crystalline silicon test masses using amorphous Si (α-Si) and SiO_2 coatings and operated at 123 K (where the thermal expansion coefficient of Si crosses zero). A change from the Nd:Yag 1 μm laser wavelength to 1.55-2.1 μm is needed to adapt to the transmission band of Si test masses. Stable high power lasers operating in this longer wavelength range will be needed, and frequency-dependent squeezing at these longer wavelengths will be required to achieve the desired quantum noise performance at low and high frequencies. This mandates the need for replacing the laser and optical components as well as high quantum efficiency photodetectors operating at the design wavelength. Newtonian noise cancellation will be needed over that needed for A+.

While LIGO Voyager will utilize the aLIGO seismic isolation systems and other components, the light sources and the optical components are completely new, using many technologies not used in today's interferometers. However the R&D needed to ascertain the feasibility of the design is well underway. Unlike KAGRA, which requires a substantial cryogenic plant to operate at 23 K, the Voyager design requires a far simpler cryogenic system for quietly cooling and maintaining the temperature of the test masses. Preliminary designs for cooling of silicon test masses are now being tested. Benefitting from the semiconductor manufacturing industry, large diameter silicon substrates are becoming available, although the purity may not yet be at the level needed for Voyager. Investigations of the opto-mechanical properties of silicon and fabrication of silicon fibers and bonding methods are progressing to enable low thermal silicon fiber suspensions. Research into high power stabilized long-wavelength lasers is underway in several laboratories around the world.

Plans are being developed to convert the Caltech 40 m interferometer into a Voyager testbed, including long wavelength lasers, silicon optics, and cryogenic suspension.

3.6 Meeting the Challenges of Building Improved Detectors

Realization of A+ and future interferometers such as Voyager will require not only engineering squeezed light, but also sustained R&D programs on optical coatings to better understand and reduce coating thermal noise, to enable 'cold' interferometer operation, to manufacture and coat test mass substrates made of new materials (silicon), and to cool and maintain low temperature test masses without introducing displacement noise. Among all these advances, *the one domain which has the most immediate need and presents the most complex challenges is reducing coating thermal noise.*

Experimentally synthesizing coatings with lower mechanical loss has proven to be a particularly vexing challenge for the community. In the past year, progress has been made on a number of fronts on understanding the underlying mechanisms producing mechanical loss in amorphous coatings. The microscopic mechanism underlying coating thermal noise is very likely associated with low energy excitations associated with the thermal hopping between states of two level systems distributed in energy in amorphous materials (loosely akin to bond-flipping). Large scale molecular dynamics modeling of SiO_2 have advanced to

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[3] This assumes that the LIGO vacuum refurbishment program restores the requisite vacuum pressure at the Livingston Observatory.

the level where computations of mechanical loss and other relevant macroscopic parameters qualitatively agree with experimental data, and can now be used to perform ‘computational synthesis’ of potential candidate coating materials[29][30][31].

Insights into coating thermal noise are also provided by recent work on the ‘energy landscape’ of amorphous materials as they undergo a phase transition to the glass state. A slower phase transition (cooling) allows for greater exploration of the energy potential configuration space and the possibility of achieving a lower energy state (and correspondingly less internal dissipation due to thermal excitations). This idea can be experimentally tested using slow deposition rates onto heated substrates. This process increases the mobility time for the molecule, allowing it to sample a greater number of states, and increasing the probability of finding the lowest energy state, and has already shown reduced mechanical losses in α-Si thin films. These ‘stable glass’ concepts along with improved computational methods and atomic structural characterization are a very promising avenue of research. The use of nano-layered coatings, either to prevent crystallization during high temperature deposition, or to directly produce coatings with improved “Q” is another promising technique that already showed initial positive results. However, a large-scale coordinated R&D program is needed to fully explore these ideas.

Crystalline coatings are an alternative to amorphous Ion Beam Sputtering (IBS) coatings. They have already demonstrated lower thermal noise, below that needed for use in A+, on small diameter mirrors. Key challenges for crystalline coatings are maintaining low loss as the production processes scales to large diameters. Current Molecular-Beam Epitaxy (MBE) production facilities are limited to 20 cm coatings.

3.7 Conclusion and Recommendations

The first detections have transformed gravitational-wave science, providing information and insights needed to guide the commissioning of Advanced LIGO and Virgo as well as to point the path for near and longer term detector upgrades. The key recommendations distilled from the presentations and discussion in Session A are:

Recommendation: The A+ upgrade program is a high priority near-term goal and should be ready for implementation once Advanced LIGO has achieved its design sensitivity and has carried out a long duration (one to two year) observing run. The participants endorse the A+ design and urge that LIGO Laboratory and its partners embark upon the upgrade program as soon as feasible.

Recommendation: There is a strong scientific rationale for implementing the A+ upgrade on LIGO-India before it begins science operations. Discussions should begin soon among the LIGO Laboratory, NSF, and the LIGO-India management team on how to plan for and fund a LIGO-India A+ upgrade.

Recommendation: Continued focus on improving coating thermal noise is essential for *all* future gravitational-wave detector incarnations. To realize A+ and future detectors such as LIGO Voyager, an adequately funded, focused, and well-coordinated R&D effort on producing coatings with lower thermal noise is urgently needed.

Edited By Laura Cadonati

4 Session B - The international network and multi-messenger astronomy

4.1 Introduction

The detection of GW150914 represents "first light" for gravitational wave astrophysics. It came with surprising properties: the black holes were much heavier than any black hole known in X-ray binaries, their existance is consistent with low-metallicity formation scenarios, their spins were weakly constrained but nowhere near maximal, and their distance was z$\sim$0.09[16][32]. GW150914 offered a stringent test of general relativity, with the best ever *dynamical* bound on the graviton mass, ($m_g < 10^{-22}$eV)[17] [4]. Still there is much science still to be explored: finding EM counterparts of LIGO sources, discriminating between central engine, external fireball and ejecta, pinpointing host galaxies, determine formation environments. Also: finding gravitational wave standard sirens, explaining the cosmic abundance of heavy elements, explaining the nature of short GRB, and challenging whether stellar BBHs are truly barren of matter. *Understanding the full astrophysical richness of compact binaries will take not just the GW network, but the broader astronomy community across many wavelengths, and localization with multiple detectors will play a key role.*

This session focused on two aspects of the short/medium timescale planning: (1) the international network of gravitational wave detectors in the next decade and (2) the path towards multi-messenger astronomy. In the first half of the session, Leo Singer presented state-of-the-art predictions for sources, rates and localization, while Lisa Barsotti discussed post-O3 observing scenarios: how upgrades and data taking can be managed for the best science, including Virgo and KAGRA timeline, expectations on evolution of their sensitivities, and prospects for LIGO-India. The two presentations were followed by a panel on the potential of a coordinated R&D program and the role of GWAC (Gabriela González, Giovanni Losurdo, Pedro Marronetti, Sanjit Mitra, Shinji Miyoki, David Reitze). In the second half of the session, Peter Shawhan summarized what did and what did not work in the GW-EM partnership during and following O1, and we held a panel discussion on how agreements should be modified or the model for partnership changed (James Annis, Neil Gehrels, Jonah Kanner, Antonia Rowlinson).

4.2 The International Network

4.2.1 Sources, rates and localization in the post O1 era

In view of the O1 discovery and the planned operations for the GW interferometer network, we expect the detection rate to be set by the range of the second most sensitive detector: adding a third detector with equal sensitivity will not significantly improve the detection rate. On the other hand, simulation studies show that adding a differently located detector significantly increases the completeness of surveys: we expect to detect tens of

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[4] Better *static* bounds have been derived from model-dependent studies of the large-scale dynamics of galactic clusters [33] and weak lensing observations [34].

BBH signals by O2, hundreds by O3, and detections or meaningful limits for BNS and NSBH binaries in O2/O3.

The sky localization accuracy for these events will begin to improve once a third detector is online at any sensitivity, and it will be optimized by maximizing the range of the third most sensitive detector. In particular, the localization will dramatically improve if the third detector is at least 1/4 as sensitive as LLO/LHO, with 60% reduction of the median 90% area. Thus, even with "early" sensitivity, Advanced Virgo can fundamentally transform the character of GW observations.

4.2.2 Observing Scenarios beyond O3

LIGO and Virgo have jointly drafted a coordinated run schedule [15]:

- **2015-2016 (O1)** A four-month run with the two-detector H1L1 network at early aLIGO sensitivity (40–80 Mpc BNS range).
- **2016-2017 (O2)** A six-month run with H1L1 at 80–120 Mpc and V1 at 20–60 Mpc.
- **2017-2018 (O3)** A nine-month run with H1L1 at 120-170 Mpc and V1 at 60–85 Mpc.
- **2019+** Three detector network with H1L1 at full sensitivity of 200 Mpc and V1 at 65–115 Mpc.

At the time of this writing, Advanced LIGO is on track to follow this schedule. Advanced Virgo achieved first lock of one of the 3 km long arms in May 2016; installation and integration are nearly completed, but there are still challenges in operating monolithic suspensions under vacuum, and various suspension failures had to be addressed since November 2015 (the root causes are still being investigated in a dedicated test facility, while commissioning and first observations will progress with steel wires for 4 core optics). Virgo is expected to join O2 in 2017, and meet the minimal goal of 20 Mpc BNS sensitivity range.

KAGRA will implement a 3 km underground detector, designed for cryogenic operation (20K, sapphire test masses); a room temperature, simple Michelson interferometer was first locked in March 2016. Sensitivity progression and observing scenarios are under discussion within the KAGRA collaboration, with three phases of increasing complexity in the interferometer configuration. The recent schedule foresees cryogenic operations in late 2018 or early 2019, and KAGRA is expected to join observations at its design sensitivity in early 2020.

LIGO-India "in principle" approval was issued on February 17, 2016. The site selection has converged on a site. Vacuum infrastructure drawings are completed and there is a project schedule that is consistent with beginning observations in 2024, barring technical or other delays.

As a general guideline for run planning, given the O1 results, we use the aLIGO-O1 sensitivity as the benchmark for detection, the predictions for rate and localizations mentioned in §4.2.1, and the fundamental philosophy to improve the detector if possible, keeping the science potential as target. Figure 1 is a plausible world-wide observing scenario for the next decade, and figure 2 is a projection of the V×T (Volume × Time) figure of merit in different scenarios. The expectation is to maximize coincidence across the network in the next few years, with improved sky localization. Due to the outsized impact of sensitivity on the detection rate, **we assume A+ upgrades done in parallel at the LHO and**

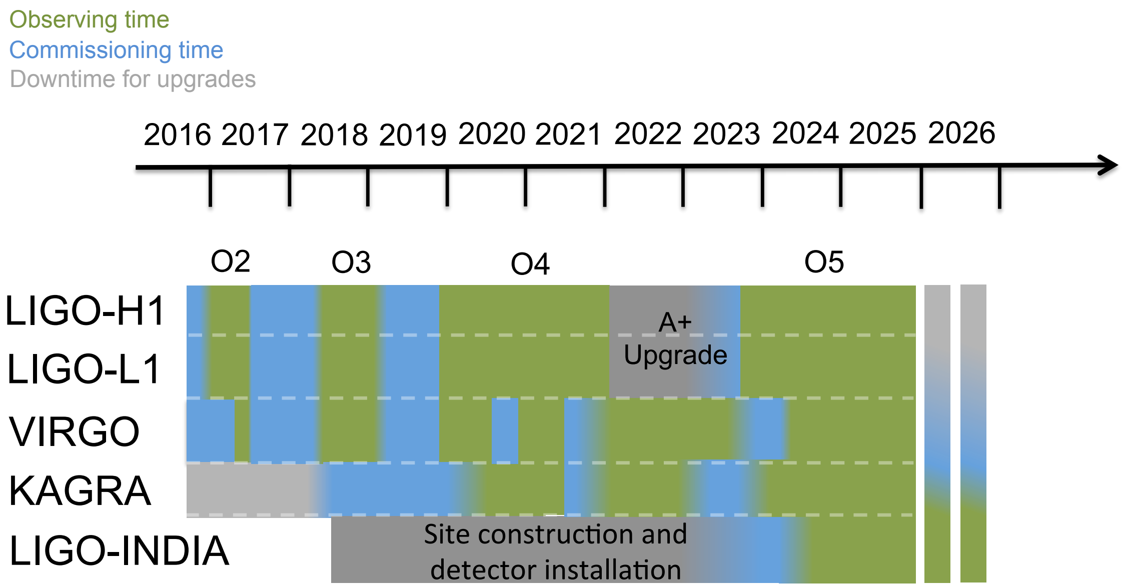

Figure 1: Plausible world-wide observing scenario for the next decade.

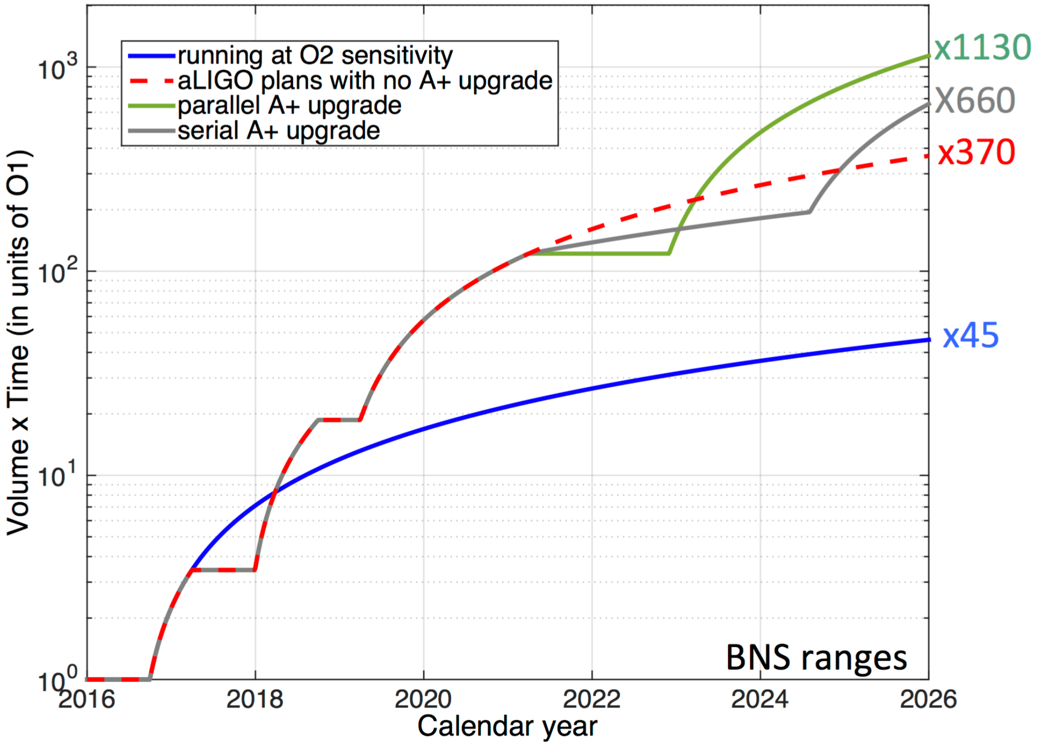

Figure 2: projected growth for the V×T figure of merit in different scenarios: O2 sensitivity, Advanced LIGO with no A+ upgrades and parallel vs serial A+ upgrades at LLO and LHO.

LLO sites; this is shown graphically in Figure 2. Serial A+ upgrades of first one then the other LIGO detector may be an option to consider once Virgo and KAGRA achieve aLIGO-O1 sensitivity, thereby guaranteeing coverage with at least three detectors during the serial aLIGO upgrades. The A+ technologies will offer enhancement opportunities for the whole network.

V×T is the metric used in these projections - the concern was raised that it is not the only metric to be used, as it does not factor in, for instance, that the field will be driven by what is not understood and may be driven by SNR increases, not just running longer at a given sensitivity. So, in some sense V×T is a very "conservative" metric. Can we factor in the potential for new discoveries? How can that be quantified? For future planning, the GW community should formulate a set of metrics beyond V×T for BNS, spread over frequency bands, maybe factoring in the potential for parameter estimation. However, it is important to point out that even with the V×T metric, the upgrade of both aLIGO detectors to A+ is very advantageous. Indeed, with respect to keep running aLIGO at design sensitivity, an upgrade to A+ in parallel at both LIGO sites offers a factor of 3 improvement in the observable time-volume.

The current timeline suggests that LIGO-India should plan now for a rapid progression toward an A+ detector, with early plans for a filter cavity infrastructure and leveraging on progress in coating research for A+ (additional test mass substrates to be coated when improving coating options become available). LIGO-India will then add flexibility to the network for major Voyager-like upgrades, by allowing one of the other detectors to be taken off-line.

4.2.3 Potential of a coordinated R&D program and the role of GWAC

The panel discussion included experts from LIGO, Virgo, KAGRA and LIGO-INDIA. We reviewed the plans for the short/medium term.

- LIGO has solid plans for the A+ upgrades, described in the section of this document discussing workshop session A.
- Virgo's immediate focus is joining O2, and has produced a vision document that foresees three phases in next decade, the first two to achieve Advanced Virgo design sensitivity by 2018 and further upgrades $\sim$ 2022, but as yet without an implementation plan.
- KAGRA has three phases: (1) operation of 3km simple IFO in 2018 (2) evolved version cryogenic in 2019 (3) commissioning start in 2019 2nd quarter to reach target sensitivity $\sim$ 2022. The project is manpower limited and there are no future concrete plans beyond reaching current KAGRA design or resources for participation in future 3G detectors. However, KAGRA will be able to contribute experience in cryogenic technology and quiet underground operations.
- LIGO-India is just now ramping up, with rapid progress since the in-principle approval last spring. Although construction of LIGO-India is a major challenge and the principal focus, plans have begun for third generation R&D beyond LIGO-India, and experimentalists have been contacted, and lectures and meetings are being planned. There will also be collaboration opportunities for LIGO-India installation. Ideally, funding for R&D can be included in LIGO-India construction.

The panel emphasized that the success of R&D for one detector is useful for all detectors because advances can be shared among all projects. Coordinating R&D and funding at the international level is very important and takes strong advocacy among the various project leaderships. Opportunities for coordinated R&D exist for squeezing and Newtonian noise cancelation. But we need an assessment of the available person-power to undertake these programs and a coordination model. We need to foster more formal structures of communication, coordination and planning, which could be in the form of a steering committee or a working group. It was suggested to use the LSC Instrumentation (INS) white paper as a model for a coordinated R&D program, and to use the existing mechanism of the LIGO visitor program.

The panel also acknowledged we cannot only talk with each other and that ultimately, for a successful R&D program the GW community needs to reach out beyond its borders, to other fields (e.g. material scientists, geologists...). This could be accomplished by refocusing the existing Gravitational Wave Advanced Detector Workshop (GWADW) series or inviting experts outside the LIGO-Virgo community to our collaboration meetings.

A major issue to consider is funding: upgrades are costly. Achieving comparable sensitivities requires investments from multiple agencies and countries. However, neither coordination nor funding come without pushing. For this discussion to take place at the level of funding agencies, Gravitational Wave Agencies Correspondents (GWAC) [5], a committee of international funding agencies from a number of countries, was created informally and had a first meeting in 2015. GWAC is operative and eager to receive inputs. The purpose of GWAC is to coordinate funding for GW activities from large to small scale. Large scale will not be possible without coordination between funding agencies. Small scale funding may include training of postdocs & students. Binary interactions are also possible, e.g., between Germany and the US: GWAC can be used by pairs of PIs. We recommend that the Gravitational Wave International Committee (GWIC) [6] interact closely with the GWAC in this coordination effort.

4.3 Multi-Messenger Astronomy

The EM-GW partnership has matured over the past decade and proven to be successful, with common interest in doing science.

Prior to O1, LIGO and Virgo performed several types of externally triggered and joint searches for both transient and continuous GW signals. They also carried out an EM follow-up program during S6/VSR2+3, with about a dozen partner groups, and a variety of communication protocols. The collaborations sent alerts for 14 GW triggers, images were obtained for 8, including the “Big Dog” blind injection event. Image analysis results were published in a single paper after a few years of work [35]. Relationships tended to persist, even after reaching the formal end of the MOU scope, based on common interest and trust: expert guidance is helpful even in cases where the data are publicly available.

In preparation for O1, LIGO and Virgo have worked out a new framework for EM follow-up partnership, and enrolled 74 partner groups under a standardized MOU. During the O1 run, three alerts were sent to 65 groups of astronomers with observational capabilities, and about 40 groups followed-up at least one alert, giving a broadband coverage of the sky maps and the rapid characterization of the candidate counterparts.

1. **GW150914**: 2 days to alert. 19 days to update on BBH nature. 4 months to final FAR and sky map.
2. **G194575**: 6.5 hours to alert. 29 days to retraction.
3. **GW151226**: 1.6 days to alert. 15/17 days to update on redshift/final FAR. 23 days to final sky map.

It should be noted that GW150914 was detected before all the trigger exchange infrastructure was in place, and GW151226 was first identified by a new pipeline. Additionally, we had not yet decided to share the BBH nature of the event and the distance.

4.3.1 Lessons learned from the O1 experience

Establishing partners

The process of establishing partnerships with the EM community was facilitated by setting clear criteria for eligibility and MOU conditions that did not scare away many potential partners. There were many applications, and the vast majority were eligible. However, the LV-EM Forum did not function as expected: the registration system required lots of monitoring, and the forum itself failed to maintain regular communications (for instance via email or teleconferences); the occasional messages were fairly impersonal. The subscription to private GCN Notices and Circulars required additional steps which were overlooked by some partners, and often required iterations with a single human contact point.

Outlook: Most active partners are set up now, but the registration system and GCN distribution configuration still need manual intervention regularly. LIGO and Virgo are working to keep communication threads going in this Forum.

Identifying candidate events

During O1, low-latency analyses ran reliably and reported triggers which were vetted quickly, and junk triggers were rejected. There were however some points of confusion that caused delays, such as the fact that BBHs were deliberately excluded from the low-latency CBC search at first (causing delays for GW151226). Also, the GCN Retraction Notice mechanism was not fully implemented and tested, so it wasn't used during O1, which made us extra cautious about sending alerts.

Outlook: It is necessary to re-implement software to be more robust, to deal better with multiple triggers from the same transient in the GW data, and to do a better job of monitoring. We would like to minimize the alert latency by changing the role of human vetting to occur after sending the initial alert, and then possibly issuing a retraction if needed.

Communicating about candidates

The mechanics of sharing information with our EM partners worked quite well, with a standardized communication route with private GCN, GraceDB access for partners and the EM Bulletin Board. However, alerts were delayed by 1-2 days due to needing to seek approvals and there were initial technical issues with private GCN. Concerns about secrecy and protocol led to not sharing important information from the GW analysis until much later, which led to inefficient observing and frustration; our partners did not feel as if they were being treated as trusted partners. There was also some confusion due to our providing multiple sky maps.

Outlook: O1 was all special cases and the collaborations did not have a chance to settle into a routine. We expect the process will be much smoother next time, with experience and reduced pressure. We should however agree on guidelines and then be able to act without bureaucratic overhead. We also advocate providing more information in future, such as binary classification or the source distance.

Follow-up Observations

EM partners displayed much enthusiasm in the follow-up endeavor, even for the first event candidate GW150914, which was announced with many caveats as an *unvetted event of interest.* Many astronomers willingly shared information about their observations. However, the observations had shortcomings in terms of depth and/or sky coverage [36], and there was no real coordination of follow up observations.

Outlook: In the future, we expect that astronomers may become more selective about following up events. We will continue not to provide recommendations on where to observe, besides the skymap, just encourage communication between observers.

Findings from Observations

There were several positive outcomes from the EM follow-up observations, as Astronomers analyzed their own data and have written many papers, potential counterparts were considered rationally and published (or not), and many optical transients were classified spectroscopically and dismissed. An intriguing, controversial Fermi GBM weak transient was published and is being critiqued by the community, and there was good give-and-take on optical transient PS15dpn with position-constrained parameter estimation from the GW data, firmly establishing that the redshift was inconsistent. We established a streamlined "partner paper check" mechanism and formerly private GCN Circulars were added to the public GCN archive. There was some stress around the announcement of the discovery of GW150914, as the partner paper check turn-around was inconsistent under the pressure of the February 11 deadline.

Outlook: We need to critically analyze the question of how best to undertake joint analyses and papers in the future.

About Open Alerts

LIGO+Virgo declared: "...the LSC and Virgo will begin releasing especially significant triggers promptly to the entire scientific community... after the Collaborations have published papers (or a paper) about 4 GW events, at which time a detection rate can be reasonably estimated. The releases will be done as promptly as possible, within an hour of the detected transient if feasible. Initially, the released triggers will be those which have an estimated false alarm rate smaller than 1 per 100 years.... Partners who have signed an MoU with the LSC and Virgo will have access to GW triggers with a lower significance threshold and/or lower latency...".

This will allow MOU partners to publish immediately, before any final word from the GW analysis.

While the 4th GW detection is likely to occur during the O2 run, its public announcement and publication will probably occur after the end of the run, at which time the transition to prompt releases will become the *modus operandi.*

4.3.2 How should agreements be modified or the model for partnership changed?

The panel and participants expressed unanimous consensus on the importance of accurate and prompt communication of all available information, for better scientific outcome. While during preparations for O1 it was difficult to decide how to share information, because we had never detected anything, we don't need to be as conservative moving forward.

The suggestion is to set up an automated system that can issue triggers with a "preliminary" flag within 30 minutes or less, with the possibility of later retraction if triggers turn out to be due to artifacts. Efforts should continue to automate data validation and detector characterization.

For this, we need to consider critically what false alarm threshold is to be used for sending a trigger, allowing ourselves to be less conservative and allowing for correction in followup, including the possibility to revoke the alert if needed.

Information that would be useful include sky maps, event significance, distance, and information on the nature of the event (for instance whether it is a BBH or it is likely to involve a NS).

We expect there will be interest in MOUs for focused science targets/papers even in the era of open triggers, and some collaborations will be interested in detailed MOUs for deeper data exchange (as is done for IceCube for instance) in addition to trigger release. This is a model that has worked well in the past and it is expected that some collaborations would be interested in continuing with MOUs.

4.4 Conclusion and Recommendations

- **Recommendation:** produce a set of 3-4 metrics beyond Volume×Time, covering the discovery potential of the detector network over the full band, and use these for run planning in the next decade.
- **Recommendation:** Coordinated R&D for short/medium timescales, with coordination between funding agencies (through GWAC) and the science teams (through GWIC).
- **Recommendation:** enable rapid communication of all information available, including sky maps, significance, distance and event ID: first trigger with a 'preliminary' flag (30 min or less) and revised false alarm threshold: to be less conservative and allow for corrections in followup (even call back if needed). Prioritize automation of data quality validation for candidate events.
- **Recommendation:** continue to pursue MOUs for focused science targets/papers even in the era of open triggers.

Edited By Beverly Berger, Albert Lazzarini

5 Session C - Long term planning by the international community for 3^{rd} generation detectors

5.1 Introduction

The transition to gravitational-wave astronomy, ushered in by the first detections of gravitational waves from binary-black-hole mergers, has led to a long-deferred desire to move toward realistic planning for next generation facilities and instruments. While a European-funded design study for the Einstein Telescope (ET) was completed in 2011 [37], US funding-agency interest in an analogous facility has only recently awakened with the successful operation of Advanced LIGO during its inaugural observing run. Here we report on a session of the workshop motivated by the prospect of planning a network of next-generation facilities that will likely cost one billion dollars or more; their realization could be fostered by organizing the international community similar to the way the high energy physics community addresses large-scale projects. To this end, the session was organized around three presentations on lessons learned from the SSC and ILC (J. Brau), how the European GW community organized itself for the ET design study (M. Punturo), and the need for a blue-ribbon external study on the future of (ground-based) GW detectors as the first step in a US-plus-international community coordinated effort (R. Weiss).

5.2 NRC study

Studies by the NRC can provide an authoritative, objective assessment of scientific and technological issues related to large projects in order to advise the US funding agencies. In addition, the committee compiling the report, especially the chair, can advocate for its recommendations with the funding agencies and Congress. It should be noted that such studies and reports of advisory panels played a significant role in NSF's decision to fund LIGO [38]. However, the advisory-panel structure used then no longer exists. In addition, analogs of NRC studies do not exist in Europe – the proposal to fund the ET design study was essentially self-contained and included the science case. The Gravitational Wave International Committee (GWIC [6]), a working group of the International Union of Pure and Applied Physics (IUPAP, [39]), developed and published a roadmap for GW detection in 2010 [40], with contributions from all current and envisioned GW projects and input from many of the relevant funding agencies. However, at least in the US, the GWIC report does not carry the imprimatur nor does it have the governmental influence of an NRC study.

An NRC study could play an important role in promoting an international effort for next generation GW detector facilities in several ways. This past year, P. Marronetti, the NSF Gravitational Physics Director, formed an international communication network, the Gravitational Wave Agencies Correspondents (GWAC [5]), among his counterparts who oversee GW funding around the world. GWAC could participate in the NRC study in several ways including providing advocacy, financial support for the study, and informing the NRC committee of the activities and interests in their countries. For maximum impact, the study committee would include international members and the charge to the committee would request that the report's assessment to be made in the context of international projects: the study committee's recommendations must take cognizance of the complete international landscape and must address international cooperation. GWAC can play an important role with respect to the study, ensuring that the report's recommendations could be directed to all the participating agencies. Without specific guidance in this regard, the report would normally be addressed only to US funding agencies.

Several organizational issues for the NRC study are currently unresolved:

- *Timing:* One important consideration is the coupling between an NRC study and other surveys. If the study could be completed by late 2018 it would serve to inform the 2020 Astronomy Decadal Survey. However, given the timescale suggested by the NSF, it is not likely that an NRC study can be completed by late 2018. The timescale for GW detector research also requires an early start to the analysis. The initial LIGO instruments required 30 years of development and it was 15 years from the first Advanced LIGO conceptual paper to its realization. The LIGO binary black hole discoveries in 2015 provide an impetus to starting now even though the full landscape of discovery by the current generation of instruments and their upgrades will not be known for a number of years. It is not completely clear how essential it is to align a LIGO study with the upcoming decadal survey; the main advantage would be the opportunity to provide an informed endorsement of a LISA-like mission. **In any case this dictates that the study be initiated as soon as possible**.
- *Scope:* While the immediate focus may be ground-based detectors beyond the current generation, there is the larger question whether the study can or should cover the entire GW spectrum — ground-based, space-based, pulsar timing, and CMB polarization. Certainly the synergy among them for the science case needs to be addressed. However, there have been several recent studies of LISA [41, 42], eLISA [43], and other space-based options [44] so the argument can be made that the relevant funding agencies are already well informed about technical details and costs. Evaluation of space-based detectors should thus be excluded from the study's scope with one exception: if (e.g.) LISA and one or more 3G detectors are operating at the same time, what new science does this synergy make possible, especially beyond a network of upgraded 2G detectors? The enhancements and upgrades of the 2G detectors themselves have fewer unanswered questions requiring an NRC study since the 2G+ parameters are highly constrained by the existing facilities – and the costs for such upgrades will not rise to the level of those needed for new green-field 3G facilities.

Many fundamental questions about 3G instruments are currently unresolved. The following list could form the basis for the charge to the committee conducting the study, for example:

- What science could be best addressed by 3G detectors in new facilities?
- What is the potential impact of that science on broader issues in astronomy, gravitational physics, nuclear physics, and other research areas?

- What is the optimal number of 3G detectors?
    - Where should they be located on Earth?
    - How is the science affected if the number and location of detectors is not optimal?
- Should they be above ground or underground?
    - what is a realistic useful lower frequency bound in a terrestrial environment?
    - what additional science goals are enabled by moving underground?
- What is the role of enhanced / upgraded 2G detectors in the 3G era?
- What is (are) the best conceptual design(s) to achieve 3G science?
    - Is the best design different if the number and placement of 3G detectors is not optimal?
    - What are the advantages and disadvantages of a heterogeneous vs. homogenous detector network?
- What are optimistic and realistic timescales for 3G development and construction?
- What are reasonable cost estimates for a 3G facility and the necessary R&D program?
- How can the international community best manage advanced detector R&D from now to the 3G era to optimize synergy and minimize unnecessary duplication?
    - How should the existing ET design study and R&D program be incorporated in the international vision and effort extending beyond the EU?
- What crucial technology R&D (especially R) should be emphasized now with applications both for 2G enhancements and upgrades, and for 3G?

Recommendation: At this point, it is premature to ask the NSF to begin the process of commissioning an NRC study on the future of ground-based GW detection with an international focus and addressing the questions raised in this report. First, a strong commitment by the international GW community is needed to begin the process of global planning and establishing community-wide buy-in of the requirements and approach to meeting them with a 3$^{\text{rd}}$ generation network of detectors. The scientific motivation must be clear, compelling, and widely appreciated by the public, the broader scientific community, and by government funding agencies. It is essential to work together globally early enough to build global ownership of the design and its implementation plan including a clear understanding of a validated cost estimate with a plan for cost risk mitigation. **In the U.S., the timescale for developing the science case and conceptualizing a 3$^{\text{rd}}$ generation network would be the next DAWN Workshop, at which time there should be sufficient information available to allow the NSF to begin the process of developing a charter for an independent external study**.

5.3 Applicable lessons learned

5.3.1 The Einstein Telescope Study, 2007 - 2011

The ET design effort grew out of the European Commission's Integrated Large Infrastructures for Astroparticle Science (ILIAS [45]) 6th Framework Programme, 2004-2008. The program's goal was to foster infrastructure development among the astroparticle and gravitational-wave communities. The astroparticle focus was primarily on underground laboratory experiments. Discussions among the ILIAS member groups led to the focus by the EU GW community, primarily members of the GEO (an LSC group) and the Virgo collaborations, on an underground 3G facility. It should be noted that all the GW groups in Europe, as well as some scientists from the USA, India, and Japan participated in the study. The process leading to the successful proposal for the study required a networking structure to be in place for all the interested parties to discuss ideas and mediate interests. This structure was open to all stakeholders but sufficiently organized to make progress. The ET project had an organizational structure wherein team members had well-defined tasks. In addition, there is an explicit connection to the institutions involved both to strengthen the mandate and to provide feedback.

The successful proposal yielded a 3-year grant of €3M. The study was conducted under several constraints. Most important, the call for proposals arose in a program to support infrastructure. This led the study to focus on infrastructure while assuming conservative technologies to realize detectors with minimal R&D(§1.3.1 in [37]). Furthermore, the lack of corresponding initiatives outside the EU led to a design having three nested $\angle$-shaped interferometers arranged in an equilateral triangle with 10-km arms, that could make discoveries as a single detector.

The ET design study incorporated the contributions of $\sim$200 scientists, leading to overall agreement by the participants on the strategic goals as outlined in the original proposal. The final report on the design study is publicly available [37] and the science case has been presented at a number of conferences [46]. The ET study is generally well known among the entire GW community and much of the relevant astrophysics community. The ET design study ended in 2011. The effort is currently supported for technology development by the European Commission with a focus on the development of cryogenic technologies for KAGRA and ET.

The lessons learned from the ET design study and R&D program can be used to inform a broader international program for 3G facilities. In the near term, coordinated R&D, especially in the highest priority areas, could foster tight collaboration among groups and ensure that no critical areas are neglected. GWIC, as the international body representing all GW efforts, has been suggested as the organization to provide the coordination, possibly forming a subcommittee for that purpose. Ideally, the corresponding funding agencies (i.e., those in GWAC) would engage GWIC in a dialog and partnership to promote and support this activity. However, unlike the EC's formally constituted AstroParticle European Research Area (ASPERA [47]), GWAC is still unofficial in that it has no real mandate to oversee and guide major efforts. Nonetheless, even informal coordination could provide a sufficient mandate to GWIC to enable it to play the role described here.

5.3.2 International Linear Collider, 2002 - 2013

A number of elements of a possible path forward to 3G facilities are also features of the worldwide effort to promote the International Linear Collider (ILC). First was the recognition that multi-billion-dollar projects must be international from the beginning. Second, that the science case must be compelling before detailed concepts for the technology can be developed, and that programs of this scale require support and advocacy from the broader international scientific community: this requires outreach to colleagues outside the field. Third, that a realistically costed, robust design for the project must be developed. Fourth, that the effort must proceed under the aegis of an international body. Fifth, that potential funding agencies must be involved as advocates from the beginning.

The International Committee on Future Accelerators (ICFA [48]), established in 1975, is a working group of IUPAP, just as is the more recently formed GWIC. However, unlike GWIC, whose members represent leadership of individual GW projects, the 15 ICFA members represent regions of the world involved in high-energy physics. ICFA has taken on the responsibility for guiding the field toward realization of the ILC.

In addition, since 2003, Funding Agencies for Large Colliders (FALC [49]) with membership from the US, France, Germany, Italy, Spain, UK, CERN, China, India, Japan, and Korea has met regularly and has ILC as its primary focus. Of particular interest is a modest common fund to support central infrastructure to allow ICFA to organize the ILC effort.

In 2002, ICFA concluded that the independent worldwide efforts in linear collider development were competing with each other rather than advancing the field. ICFA then established a technical review committee to assess the competing efforts. The review concluded that only two of the approaches, SuperRF and X-band, were mature enough to be considered as the basis for an internationally unified development effort. ICFA then established a recommendation panel to choose between them, announcing SuperRF as the "winner" in 2004. The community accepted ICFA's recommendations and allowed it to oversee the Global Design Study [50] (led by B. Barish). In addition, the global unity of the scientific community led to inclusion of national ILC R&D programs in the funding agencies' budgets. The design study was completed in 2013. In addition to the technical design, the design effort provided a realistic and transparent review of the cost of the project (including taking into account the idiosyncrasies of various funding agencies), adding to its credibility. An implementation plan was produced in 2015 including management and governance, host country responsibilities, cost sharing and in-kind contributions, and non-cash contingency.

The collaboration models, governance models, coordination among funding agencies at the international level, and approaches to achieving consensus driven by common science goals can all serve as templates that might be applicable going forward in our own field.

5.4 Conclusions and recommendations

In many ways, the global structures in the GW community are not yet mature enough to promote an international effort analogous to the ILC. However, the basics are in place. The ground-based GW detection projects already collaborate at some level on instrument research while the LSC and Virgo Collaboration have shared data since 2006. In 2010 GWIC released a roadmap for the field [40]; as an IUPAP Working Group, it could become the forum for coordinating near-term R&D, and potentially a global design effort for a 3G network. An NRC study could assess within this global context various existing proposals for 3G instruments. Such a study could provide the framework for a global effort, whether the effort constitutes an array of identical instruments or a synergy of different ones. An NRC study could also provide the vehicle necessary to communicate the compelling science case to the broader communities beyond the GW field. Ideally, the study could catalyze the funding agencies around the world to formalize GWAC in the same manner that FALC has been formalized, so it could lend its weight to a coordinated, worldwide 3G effort.

Recommendation: GWAC (either formally or informally, together or individually) should encourage or request GWIC to coordinate an international R&D effort in the critical technologies for next generation ground-based GW detectors.

Recommendation: The international GW community should begin the process of global planning and establishing community-wide buy-in of the requirements and approach to meeting them with a 3G detector (network). The scientific motivation must be clear, strong, and widely appreciated by the public, the broader scientific community, and by government funding agencies. It is essential to work together globally early enough to build global ownership of the design and its implementation plan including a clear understanding of a validated cost estimate with a plan for cost risk mitigation.

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◇ Amaldi Meeting, July 2011, Class. Quantum Grav. 29, 124013, 2012, `https://arxiv.org/abs/1206.0331`
◇ 12th Marcel Grossmann Meeting Proceedings, `http://arxiv.org/abs/1003.1386`
◇ Cosmography with the Einstein Telescope, `http://inspirehep.net/record/823859?ln=en`
◇ 2^{nd} ET meeting, Erice Oct, 2009, `https://agenda.infn.it/conferenceProgram.py?confId=1564`.

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[49] Funding Agencies for Large Colliders: `http://www.falchep.org/pages/default.aspx`.

[50] ILC Global Design Study:
Volume 1 - Executive Summary: `https://arxiv.org/pdf/0712.1950v1`
Volume 2 - ILC Physics: `https://arxiv.org/pdf/0709.1893`
Volume 3 - Detectors: `https://arxiv.org/pdf/0712.2361`
Volume 4 - Accelerators: `https://arxiv.org/pdf/0712.2356`
.

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Ando, Masaki	University of Tokyo
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Evans, Matt	MIT
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Kanner, Jonah	Caltech
Lantz, Brian	Stanford University
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