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

Workshop, Silver Spring, MD, May 7-8, 2015
Conclusions and Recommendations

Contents

1 Preface

On May 7 and 8, 2015, about 70 gravitational wave scientists, astronomers, and astrophysicists gathered in Silver Spring, Maryland, in a workshop to discuss "What comes next for LIGO", specifically to begin planning for the post-detection era in gravitational-wave detectors and astrophysics.

The detection of LIGO's first gravitational wave will be a transformational event, opening new avenues for astrophysical exploration, opportunities to build more powerful detectors directed at known source populations and data analysis enhancements informed by direct detection. Participants in this workshop discussed how the first few detections might influence which paths offer the best opportunities, and how the community can be prepared with appropriate plans to maximize the scientific results for gravitational wave (GW) science and also for the broader scientific community.

The workshop consisted of five sessions on different topics, each with brief talks by different speakers setting the stage, followed by active discussion of questions posed by the session leaders and speakers. On the second day, the leader of each session organized a focused discussion of conclusions to be shared with the broader community. The sessions were organized and led by the members of the Scientific Organizing Committee: Peter Shawhan (Multi-messenger Astronomy), Laura Cadonati (Data Analysis), Matthew Evans (Advanced Detectors), Stan Whitcomb (International Network), and Beverly Berger (Broader US Context).

The main conclusions and recommendations that came out of the workshop are summarized in Section 2, followed by sections focusing on each session. Drawing from background information presented in the sessions, we also summarize in Appendix A the current status of Advanced LIGO and the other GW detectors in the international network. In Appendix B we review sources of gravitational waves, their expected rates and waveform frequency and character, as well as possible counterparts in the electromagnetic spectrum that will start "multi-messenger astronomy" associated with GW astrophysics. All slides from the workshop are available at `https://wiki.ligo.org/LSC/LIGOworkshop2015/`.

This document summarizes the discussions and conclusions from the meeting, and represents only the opinions of the participants, written down by Organizing Committee members and other volunteers. Those who reviewed the document and indicated a desire to endorse it are listed in Appendix C. We plan to meet again in the future in similar workshops as we learn more about the gravitational-wave sky with results from the Advanced LIGO detectors and their sister observatories around the world, preparing for "what comes next".

Gabriela González, on behalf of the Scientific Organizing Committee and the workshop participants.

2 Summary and Recommendations

The recent progress establishing the early operating sensitivity of the Advanced LIGO detectors follows the observing plan laid out in 2013 [1]. With conventional assumptions about astrophysical source rates [2] and the expected progression to design sensitivity, detection of gravitational wave (GW) signals by the LIGO and Virgo detectors is expected to begin in the next few years.

The first detections with ground-based gravitational wave detectors will be transformational. With an emerging understanding of the GW sky, we will be able to make informed decisions about where it is best to invest effort and resources to better target the GW signals and extract maximum scientific value from them. This workshop was the first community-wide meeting to focus on that transition and to discuss the wide range of discovery opportunities and preparations which should be made starting **now**.

One clear outcome of the workshop was to highlight the importance of continuing dialog and communication, both within the international GW community and engaging the broader community of astronomers. These connections should be nurtured and supported. We recommend, therefore, that workshops be held periodically in the future to bring together scientists from all areas to discuss opportunities and plans.

The workshop focussed on how the first detections will influence the next steps for near term upgrades to the existing advanced detectors, for optimal technical requirements of major upgrades and/or new facilities, and for urgent areas of emphasis in data analysis and theoretical studies. When appropriate, we mention approximate funding needed, although these estimates are rough, and will be refined in further discussions as warranted. It is important to note that when estimated costs are for instrumentation and hardware, development and installation will probably require an additional equivalent cost in engineering.

Work for future innovations described here must not jeopardize mission-critical tasks to establish GW detections in the first place, including less glamorous activities such as data quality investigations, calibration, and computing support. These are core activities shared by the LIGO Laboratory and LIGO Scientific Collaboration (LSC), and supporting these priorities will require focused attention from the LSC as well as from funding agencies.

2.1 Detector improvements

Advanced LIGO, as well as Advanced Virgo and KAGRA, were designed with the best understanding at the time of likely GW signals and a balanced overall frequency response, limited by fundamental physical effects in different frequency ranges. The detection of the first GW signals, whatever they turn out to be, will lead to very high priority on ways to increase the sample of events and to get as much astrophysics as possible from each detection, by identifying to the detectors' frequency band or bands which should be more urgently improved. The potential gains must be balanced against the cost and operational downtime needed to deploy them. Research on instrument technology to push the sensitivity envelope in the future needs to be done in advance, if it is to be implemented soon after the first observations.

Detector thermal noise from the mirror coatings will limit mid-band detector sensitivity, reducing benefits of other possible upgrades: R&D in this area should be a high priority **now**.

A robust program to identify new coating materials or techniques must be undertaken and funded at a level sufficient to achieve success; such a program may require \$1-10M in R&D in the next 10 years, and more than \$5M per detector for larger test masses. Improved coating thermal noise should yield a significantly higher detection rate for compact binary systems, and greatly improved range for systems with black holes, possibly with a cosmological reach.

A few relatively minor upgrades to Advanced LIGO which are already under development may significantly increase its scientific output. At the lowest frequencies, Newtonian (gravity gradient) noise from the environment will be a limitation. Adding Newtonian noise cancellation to Advanced LIGO is non-invasive (i.e., will incur almost no downtime) and has a very modest cost (likely less than \$200k). Using squeezed light can improve the advanced detectors' sensitivity broadly, especially above 100 Hz, and reduce the risk of high power operation. The cost of squeezing is small relative to the overall detector, about \$1.5M per detector in instrumentation. High-frequency sensitivity would provide information on the equation of state of neutron stars and tests of general relativity, as well as help the detection of other transient events that have low event rates.

After a few gravitational-wave observations, the community should undertake design studies to increase the sensitivity significantly in the same observatories and possibly consider new facilities, considering trade-offs on on such choices as the length of the instruments, or construction on the earth's surface vs. underground. Since technology research takes many years to reach a state when it can be considered in trade-off studies, a broad R&D program to test new ideas (cryogenics, laser wavelengths, mirror material and coatings, topologies) needs to be supported at this time.

2.2 Data analysis

The Initial LIGO and Virgo detectors provided ever-improving real data which allowed the LSC and the Virgo Collaboration to develop and test data analysis methods for a wide range of likely and less-likely signals and to set astrophysically relevant upper limits. Still, there are areas which should be improved motivated following the first detections of gravitational waves, especially if those signals were unexpected. In this section, we discuss three scenarios and the different priorities which may arise from them.

Case 1: Discoveries are from expected sources

We expect the first detections to originate in coalescences of binary systems with neutron stars and/or stellar mass ($< 15 M_\odot$) black holes. These sources will be exciting discoveries: a merger of neutron stars might show for the first time the birth of a black hole, and detections of black hole-neutron star or stellar mass binary black hole systems will provide first confirmation of long-standing astrophysical predictions. Other gravitational wave detections that are less likely but consistent with current astrophysical models are gravitational waves from a nearby rotating neutron star, or a galactic supernova; detection of either of these would be revolutionary.

In this scenario, we recommend investing in detector characterization, in both human resources and in greater automation of the process; continued focused development for computationally fast waveforms across parameter space including spins and merger signatures; combining the results of the stochastic background searches and of the transient searches to develop and constrain the population models of the astrophysical sources; and developing of a statistical framework for using detections (source rates and properties) to constrain the vast parameter space of astrophysical models of binary formation. The latter in particular has a long lead time, so it is urgent to begin now, before detections come.

If continuous periodic GWs are found without a known counterpart and deviating from the model for a spinning down isolated star, we will need additional computing resources for a fully coherent analysis, in order to characterize the source. We should also advocate for electromagnetic astronomers to follow up to try to identify a counterpart.

The envisioned data analysis work in this detection scenario will need an investment in human resources of approximately \$2M in 1-3 years.

Case 2: Detections are from unexpected sources

The first few detections may be from sources we consider unlikely according to current astrophysical knowledge, like binary neutron star systems with low mass (less than $1.25\mathrm{M}_\odot$) or high spin (larger than 0.4) or eccentric orbits (>0.1); and systems with massive black holes (larger than $15\mathrm{M}_\odot$). Detections of supernovae in distant galaxies, coincident with electromagnetic and neutrino counterparts, would provide a challenge for current supernova models. Detection of the stochastic gravitational wave background of cosmological origin (e.g. inflationary, post-inflationary, or cosmic strings) is also plausible while not likely. These breakthrough observations would open many areas of new research in astrophysics.

In this scenario, there should be a shift in the focus of data analysis to extend the parameter space to searches for gravitational waves in the new region (e.g., high mass or spinning or eccentric). The parameter priors for searches and parameter estimation studies should be updated following the new observations. It will be important to explore implications of new observations for formation of binary systems in general. These efforts will require support of many scientists, with an investment of approximately \$2-4M in 1-3 yrs.

Case 3: A gravitational wave mystery

If there is a high significance gravitational wave detection with neither a robust binary-system signature nor electromagnetic or low-energy neutrino counterparts, it will be a puzzle. We may find a signal predicted by an exotic theory, but have difficulty unambiguously associating the signal with the theory in the absence of electromagnetic (EM) counterparts. The mystery may be resolved with more detections of the same kind or new astrophysical models for possible sources.

We will need state-of-the-art simulations of sources incorporating matter to associate with or rule out burst candidates; as well as astronomy and astrophysics expertise to exclude some sources, based on the duration of the signal, frequency content and frequency evolution with time (if any). The total gravitational wave energy may rule out models but, in the absence of a counterpart, conclusions will be difficult since there will not be good estimates of distance.

We estimate that the follow-up work needed in this unexpected situation will require an investment of at least \$2M, but it is hard to anticipate given the uncertainties involved.

2.3 International network, and multi-messenger astronomy

In all detection scenarios, an international network of gravitational wave observatories is required for source localization. Network coordination of downtimes and upgrades may be very important: improving the weakest detector in the network can benefit sky localization; improving the strongest detector in the network can benefit parameter estimation. The relative value of these will become clearer when GW signals have been detected, potentially allowing us to set better priorities for operations and upgrade schedules.

Multi-messenger astronomy has become an important element in the GW science program and should be continued, both through externally triggered GW searches and through follow-up of GW event candidates. In support of more effective multi-messenger follow-up, we advocate building a validated, high-completeness public galaxy catalog to ~ 200 Mpc; developing optimal follow-up observing strategies; and characterizing the transient sky to understand the nature of potential false positives and algorithms to differentiate them. These development efforts may need > \$500k.

We also urge support for utilization of existing astronomical observing capabilities, as well as development of new facilities, that will be effective for searching for counterparts of GW events. This should include rapid and numerous Targets of Opportunity at major facilities; support for global follow-up telescope networks on the ground and highly capable space missions; and support for data mining and candidate follow-up with large optical/near-infrared transient surveys.

To improve GW detector network coordination, we recommend multi-month exchange visitor programs; international meetings regarding near- and long-term planning; and joint instrument science and engineering working groups developing common technical solutions. To establish a model for such collaboration, we advocate forming a joint working group involving experts from each of the gravitational wave detectors, to target initially just one near-term instrument refinement, and then beginning a joint engineering effort among the major GW detectors. These programs may require moderate investments at the level of collaborative multiple investigator funding.

We propose assembling a joint design program for GW detector improvements, distributing costs for development and production. This will be very important, since implementing detector upgrades may need significant resources, on the level of funding for mid-scale innovation projects. The current collaborations and other interested research groups should establish milestones for planning future detectors, and subsequently oversee activities to ensure that the milestones are achieved. We recommend a closer link among the global funding agencies to coordinate medium- and long-term planning, looking for synergy between the agency capabilities to most effectively stimulate the international field of gravitational wave physics and astrophysics.

2.4 Broader US context

Considering the broader context for the gravitational wave field in the US, we recommend that NSF (possibly with other agencies), with advice from the gravitational wave community broadly interpreted, explore the options for creating an advisory panel to include ground-based gravitational wave science. This advisory panel should, at the appropriate time, consider the opportunities and issues related to both fostering the emerging new field of gravitational wave astronomy, and advance planning for major new facilities to enable ground-based gravitational wave science in the early 2030s if warranted.

3 Advanced LIGO improvements

Edited by Stefan Ballmer, Matthew Evans, Peter Fritschel, and Sheila Rowan

While the primary objective of detector upgrades is to take Advanced LIGO *beyond* its designed sensitivity, several of the technologies discussed in this section also provide *risk mitigation* for Advanced LIGO. For example, significant research and development of quantum squeezing discussed in this section has been undertaken in recent years as a means of mitigating potential problems with high-power interferometer operation. However, this technology can also be used to improve the sensitivity of Advanced LIGO if full power operation is achieved. Similarly, improvements in other areas, such as coating thermal noise, provide options which both reduce any residual technical risk in Advanced LIGO and give a means of improving the sensitivity of a perfectly functioning detector.

Furthermore, the technologies discussed below are not only a means of incrementally improving Advanced LIGO; they are also necessary steps on the path to future detectors.

The following potential upgrades to Advanced LIGO are aimed at the various science targets discussed in Appendix B. When appropriate, we mention approximate funding needed, although these estimates are rough, and will be refined in further discussions as warranted. It is important to note that when estimated costs are for instrumentation and hardware, development and installation will probably require an additional equivalent cost in engineering.

3.1 Quantum Squeezing

Injection of “squeezed light” into LIGO is a proven means of improving the detector sensitivity at high frequency [3, 4]. As a near-term upgrade to Advanced LIGO, squeezed light has the potential to increase the detector sensitivity above 100 Hz. While this alone will result in only a modest increase in binary inspiral range, it aids in essentially all other science targets.

The use of “frequency dependent squeezed light” [5, 6, 7] will be required to realize the full potential of squeezing when Advanced LIGO is operating at high-power. The technology required to produced frequency dependence is also mature and the engineering design could begin at any time. It may be added to an existing squeezed light source in-situ, or installed together with the squeezed light source should that upgrade happen after the detector has started high-power operation.

Technology is mature for frequency independent squeezing, or frequency dependent squeezing with short cavities - implementation may cost \$1.5M per detector. Using long filter cavities for low frequency squeezing requires investment in technology development, with approximate costs being \$1M per detector for instrumentation.

Given the mature state of this technology, its relatively low cost with respect to the total cost of a detector, and the short downtime that such an upgrade is likely to incur, squeezed light is a clear choice for improving Advanced LIGO. Squeezing also mitigates the risks involved in high-power interferometer operation, and as such may be installed before Advanced LIGO reaches design sensitivity should technical issues make this the most expedient means of improving detector sensitivity.

3.2 Coating Thermal Noise

Thermal noise couples into a gravitational wave detector at various locations, but by far the toughest constraint on detector sensitivity is from Brownian thermal noise associated with the optical coatings. This is because (i) optical coatings interact directly with the laser light hitting the front faces of the test-mass mirrors, making it difficult to mitigate the noise coupling to the gravitational wave readout, and (ii) optical coatings already have a set of very stringent optical constraints, limiting the possible choice of materials.

The current Advanced LIGO detectors are, at design sensitivity, limited by coating Brownian thermal noise across a wide frequency band of about 30 Hz to 300 Hz. In this band, this noise level is equal to or not far below the quantum noise, as the quantum noise was optimized to be broader and shallower exactly because of the limiting coating thermal noise. Even at higher frequencies ($\sim$ 3kHz) coating thermal noise will eventually limit the sensitivity benefit achievable from reducing the quantum noise.

The Advanced LIGO optical coatings are silica-tantala (SiO_2-Ta_2O_5 dielectric stacks with a titania-doping (TiO_2) in the Ta_2O_5 layers. They were selected for low mechanical loss, while respecting the additional optical specifications. The dominant mechanical loss is due to the high-index Ta_2O_5. The titania-doping of the Ta_2O_5 layers is the main improvement over the initial LIGO coatings [8].

There are several approaches for reducing the coating Brownian thermal noise, all of which are being investigated in the LIGO Scientific Collaboration.

(i) The first approach is the search for a slightly modified version of the coating currently used in Advanced LIGO. In particular, the currently used titania-doped silica-tantala coatings need to be annealed at high temperature to reduce the mechanical losses. The annealing temperature is limited by the crystallization threshold of the coating material. The use of an alternate dopant such as Zirconia can stabilize the coating, allowing an increased annealing temperature, and thus potentially reduced mechanical losses. Preliminary investigation of this approach is underway in the LIGO Scientific Collaboration. A variant of this approach is a multi-material coating proposed in [9]. By using a high mechanical-loss/low optical-loss material for the first few dielectric layers, and a low mechanical-loss/high optical-loss material for the remaining dielectric layers, a composite coating with lower overall mechanical loss and acceptable optical loss could be achieved.

(ii) The second approach involves switching to crystalline coatings such as $\mathrm{Al_xGa_{1-x}As}$, which have extremely low mechanical loss. This approach was pioneered in [10]. Layers with varying index of refraction are created by varying the amount of aluminum and gallium. While the approach is very promising, it requires growing the coating structure separately from the mirror substrate, and then transferring and bonding the thin coating. Doing this on the 34 cm diameter Advanced LIGO optics without compromising the mechanical and optical properties of the coating is challenging.

(iii) Coating Brownian noise could also be reduced by increasing the optical spot size, effectively sampling a larger mirror area. While this is promising for future gravitational-wave interferometers with increased arm length [11], increasing the spot size for the current Advanced LIGO facilities will drive the arm extremely close to optical instability, making alignment control problematic. It is difficult to gain more than approximately 10% in amplitude thermal noise over the Advanced LIGO geometry. However, a side benefit would be a slight increase in optical clipping losses, which would reduce the onset of parametric instabilities in the arm cavities [12], allowing the operation at higher arm powers.

(iv) Finally, one can imagine switching to cryogenic operation of an interferometer to reduce thermal noise. However, because the mechanical loss of fused silica increases as temperature is lowered below room temperature (peaking in loss around 40K), this approach would require a change of substrate and coating materials, which in turn would also require a change of optical wave length. Due to the significant change in required hardware we currently do not consider a cryogenic upgrade for the immediate future of Advanced LIGO.

Once an appropriate coating material is available, upgrading the Advanced LIGO interferometers only requires replacing the four main mirrors, a relatively simple operation. Options (i) and (ii) are potentially ground-breaking for gravitational-wave observatories, but need significant research, which is currently limited by the available funding. Very rough estimates of the cost for R&D for Zirconia coatings is $1M over 1 year, and for scaling up AlGaAs $10M over 5 yrs. The costs of new sets of 80kg masses for 2 interferometers may be more than $10M.

Reducing the coating thermal noise through either options (i) or (ii), and at the same time implementing squeezing discussed in the previous section, would increase the Advanced LIGO design inspiral range from about 210 Mpc to about 350 Mpc, increasing the observable merger event rate by almost a factor of 5 [13].

3.3 Newtonian Noise Cancellation

Ground motion, known as seismic noise, can disturb a gravitational wave interferometer by moving the optics. Interferometers are isolated from this by many layers of active and passive mechanical filters which prevent seismic noise from reaching the optics at frequencies in the detection band. These filters cannot, however, shield the interferometer from the direct gravitational interaction between the seismic motion of the ground and the optics.

The motion induced by this gravitational interaction is referred to as “Newtonian Noise” (NN) and may limit Advanced LIGO sensitivity below 20 Hz. Unlike other fundamental noises (e.g., quantum and thermal noise), NN is non-stationary and may change by an order of magnitude above or below the median level depending on seismic conditions. Also unlike other fundamental noises, the source of NN is directly measurable with an auxiliary sensor, in this case a seismometer or array of seismometers. As such, there is the potential to measure seismic activity and use those measurement to predict and cancel NN.

Newtonian Noise cancellation has been an active area of research for several years and it is expected that a factor of 5 - 10 reduction in NN can be achieved with a modest seismometer array. Adding NN cancellation to Advanced LIGO is non-invasive (i.e., will incur almost no downtime) and low cost (less than $200k). The scientific payoff will come mostly in the form of increased inspiral range and better sensitivity to IMBH systems in times of high seismic activity.

3.4 Suspension Upgrades

Installation of test masses with new coatings may represent an opportunity to make minor suspensions improvements (e.g., thinner fibers, giving an improvement in suspension thermal noise of a factor of approx. 1.5 at 10Hz) without incurring significant additional downtime - a cost estimate is approximately \$300K, not including new blanks or coatings or engineering support.

Upgrading the suspensions to reduce suspension thermal noise by factors of more than 3 at 10Hz can in principle be done with heavier (80kg) masses but would require a replacement of components, necessitating significant investment and downtime. The cost for hardware is roughly \$10M and 1 year per interferometer, not including new seismic isolation parts, the ongoing R&D, or engineering costs.

3.5 Conclusions and recommendations

A few small upgrades to Advanced LIGO have the potential to significantly increase its scientific output at a cost much lower than already invested in building the detectors. Quantum squeezing and Newtonian noise cancellation are both low-risk and low-cost upgrades which should be performed as soon as is sensible given the priorities set by Advanced LIGO commissioning and observation.

With the addition of squeezing and Newtonian noise cancellation, coating thermal noise will essentially define detector sensitivity, and as such R&D in this area must be prioritized. A robust program to identify new coating materials or techniques which can be scaled to LIGO optics must be undertaken and funded at a level sufficient to achieve success. If coating materials with low thermal noise are identified, such that a significant reduction in coating thermal noise using current coating technology becomes available, new optics with low noise coatings must be considered. Improved coating thermal noise will result in a higher detection rate for compact binary systems.

4 Gravitational Wave Data Analysis

Edited by Laura Cadonati, Alessandra Corsi, Gabriela González, Vicky Kalogera

There are many sources of gravitational waves, and there are many ways to search for them: see Appendix B for details and references on these sources, and expectations for detections. Given this diversity, envisioning "what comes next to LIGO" requires identifying several possible first-detection scenarios, and related follow-up actions that will need to be undertaken to maximize the scientific return.

4.1 If first detections are as anticipated

The most likely first detections are a few binary neutron star (BNS) or neutron star-black hole (NS-BH) systems with neutron stars having mass $> 1.25\mathrm{M}_\odot$ and low spin; or binary black holes (BBH) systems with low mass ($< 15\mathrm{M}_\odot$) and potentially high spin [1]. We expect the signal-to-noise ratios (SNRs) to be moderate (12-15).

In this scenario, the priority should be to increase the number of detections and to extract as much astrophysical information as possible from each detection. To increase the rate of detections, the instrument should be upgraded to improve sensitivity in the low frequency band (below 100 Hz) to increase the reach to low mass binary systems. Improving detectors' high-frequency sensitivity (near and few hundred Hz) could help to constrain the equation of state for neutron stars [14]; and it could allow more sensitive tests of general relativity

(GR) [15], producing more science from each detection. Detectors should also maximize coincident run time: raising the individual duty cycle in two uncorrelated detectors from 70% in early runs to 85% in later ones raises the coincident run time by nearly 50%. A third detector in the network with comparable sensitivity will help with double-coincidence run-time.

We recommend investing effort into comprehensive, maximally automated detector-noise characterization and data-quality vetting, since we expect a non-Gaussian background [1]. Such effort will especially increase the detection rate of higher-mass, shorter-waveform systems and facilitate their prompt electromagnetic follow-up. We also recommend increased emphasis on computationally fast waveforms that account for important spin effects, for the full signal from inspiral to merger and ringdown, valid across the full parameter space; development of quantitative methods for moving from observed sample to underlying source properties (through modeling of pipeline selection biases); combination of results of the stochastic background searches and of the transient searches to develop and constrain the population models of the astrophysical sources; and continued development to achieve comprehensive GR tests (more accurate waveforms, wide parameter priors).

If the first detections are not definitive but are marginal detections, we can learn about the astrophysical source population with statistical arguments, and use that information to guide choices in instrument development. Either way, developing a formal statistical framework so that we can use the detection sample (derived rates and source properties) to constrain astrophysical populations is critical for completing the goals of gravitational-wave astrophysics studies. This is a necessary last step which has not received appropriate attention, but has a long lead time, so it is urgent now, before detections occur.

A possible first detection is a Galactic or near Galactic supernova, with an optical counterpart and possibly in coincidence with low energy neutrinos. Since the emission of GWs is very model-dependent, a detection will focus research on comparison of models to the observed waveform [16].

The envisioned data analysis work in this detection scenario will need an investment of approximately $2M in human resources to accomplish the recommended tasks in 2 years or less.

An important issue that was raised is dealing with the *absence* of electromagnetic counterparts for the first (few) detections: can any conclusions be drawn from this? The answer was not obvious to the attendants, indicating this question needs more attention.

4.2 If first detections are sources we currently consider unlikely

It is possible that the first few detections are from binary sources we consider very unlikely, such as very low mass (less than $1.25\mathrm{M}_\odot$) [17] or high spin (larger than 0.4) or eccentric orbit (>0.1) binary neutron star coalescences, or massive black holes (larger than $15\mathrm{M}_\odot$) [18]. Detection of the stochastic gravitational wave background of cosmological origin (e.g. inflationary, post-inflationary, or cosmic (super)strings) is also plausible while not likely. These breakthrough observations would open many areas of new research in astrophysics.

In this case, there will be an urgent need for a shift in focus, to extend search parameter space in the unlikely region; investigate different parameter priors for parameter estimation studies; and consider the implications for formation of binary systems in general. The extended parameter space for GW searches with “perfect” (or much more accurate) waveforms that include inspiral, merger and ringdown, precessing spins, higher order, and eccentric orbits will allow more clues for finding EM counterparts, but will compete with the speed with which alerts can be made. The community needs to decide on thresholds and tolerance for errors in GW alerts.

If an observed long-duration GW transient is not from a binary system and it has a low-luminosity GRB counterpart, it would be consistent with optimistic models for unstable magnetar signatures generating GWs from secular bar-mode instabilities or magnetic deformation [19]. In this case, several actions are recommended: even a slight increase in detector sensitivity at the appropriate frequencies would increase significantly the event rate (a 30% decrease in instrument noise doubles the event rate); as well as faster and more robust data analysis techniques. More sensitive, model-dependent codes will help understand the astrophysical sources. All-sky searches for these transients may be very computationally expensive, and will require both computing resources and serious code optimization. Increasing the ability to find transients in the EM spectrum coincident with GWs will be vital to confirm or rule out astrophysical models.

In the event of detection of the stochastic gravitational wave background, the efforts should be focused on the parameter estimation and on determination of the origin of the background. To this end, measurement of the background over as large frequency band as possible will be critical, since different models predict different frequency spectra. Hence, detector sensitivity improvements at low frequencies (e.g. Newtonian Noise suppression) and at high frequencies (e.g. squeezing) will be very helpful. Further, measurements of the anisotropy in the stochastic background will also be important, and may help distinguish the (likely isotropic) cosmological contributions from the potentially anisotropic astrophysical foregrounds. Finally, some of the cosmological models involve parity violation resulting in a polarized stochastic background - hence, polarization measurement would also help in determining the energy budget of the stochastic gravitational wave background."

Data from Advanced LIGO and Virgo detectors will lead to improving upper limits on the emission of gravitational waves from known pulsars [20]. Moreover, a detection is possible if the star is closer than observed pulsars, or has a very high ellipticity. In contrast with the situation for transient events, even a single sensitive detector can make precision CW detections If continuous, periodic, gravitational waves are found without a known counterpart and that deviate from the signal model for an isolated star spinning down smoothly, we will need additional computing resources to extend the hierarchical semi-coherent analysis to a fully coherent analysis, in order to fully characterize the source. Such analysis can provide source sky position to arc-second precision with integration times of a year or less. This would allow EM follow-up, which will provide a greater neutron star physics return: observations of pulsations would tell us the rotational frequency and immediately narrow down the emission mechanism. If EM pulses are not observed, EM follow up will require deeper imaging to look for fainter X-ray sources, or SN remnants, or pulsar wind nebulae. Radio, X-ray, and gamma-ray timing of as many pulsars as possible, wide-sky surveys (optical and X-ray) aimed at identifying potential nearby neutron stars, and access to newly-discovered sources, are crucial. We advocate for better modeling of neutron star deformation now - if a detection happens, this will be urgent. Continued theoretical work on mechanisms to produce and sustain ellipticities on neutron stars will provide more realistic metrics for detections and, in case of non-detections, enhance the science case for computationally expensive CW searches. On the detector side, squeezed light can improve the high-frequency sensitivity and offers the same benefits as narrow-banding, but over a broad band.

As in the previous detection scenario, data analysis work will need approximately $2M in human resources, but focused on recommendations in this section. The effort should involve 5-10 postdoctoral scholars, 3-5 graduate students, and several months' time from faculty leaders.

4.3 An event with no EM counterpart or robust inspiral signature

If there is a high significance gravitational wave detection which has neither a robust binary-system signature nor an electromagnetic or low-energy neutrino counterpart (as for a supernova), it will be a puzzle.

Guidance from astrophysics and astronomy experts beyond just follow-up will be critical. We will need to compare the detection with *classes* of gravitational wave signal predictions. We should establish a baseline used to exclude some sources, based on the duration of the signal, frequency content, and frequency evolution with time (if any). The total GW energy would be an incredibly useful quantity to have to rule out models but in the absence of a counterpart this is going to be difficult, because of the lack of estimates for distance. We will need state-of-the-art simulations of possible sources, including the effect of matter.

We estimate that the follow-up work needed in this unexpected situation will require a similar investment of $2M and possibly larger, but it is hard to anticipate given the uncertainties involved.

4.4 Collaboration Modes

Until now, the LIGO Scientific Collaboration (LSC) in the US and other countries has concentrated on developing technology for improving detectors, as well as searching for gravitational waves in the data from the LIGO-Virgo network of detectors. Besides contributing to the LIGO detectors, several groups also made significant contributions to research to the astrophysics of the sources, and forged close collaborations with astronomers and astrophysicists to allow the prompt follow up of gravitational wave observations. There is no requirement to coordinate observations among partners, nor about sharing results. On the other hand, and probably becoming more common after the first few detections, work on gravitational wave astrophysics is more attractive to young scientists than some of the critical but not glamorous or innovative work needed for producing gravitational wave detections, as in performing well tested searches, calibrating the instrument, and improving its sensitivity with data quality and commissioning work. It is not clear this is a sustainable model.

There was some discussion about a collaboration more focused on critical tasks for detection, with "guest investigator programs" with requirements of data sharing, which could ensure new search methods, as well as smaller facilities coordinating observations. There was no consensus on whether this model would indeed be better, or whether other models could achieve the same science. There was consensus though that there should be active exploration of collaboration modes that enable innovative data analysis approaches while also enabling seamless execution of mission critical tasks for first detections.

4.5 Recommendations

If the first few detections are signals from low mass binary systems, as anticipated, the highest priority will be to increase the detectors' duty cycle, low frequency sensitivity and data quality to increase the rate of detections. We recommend building models of astrophysical populations that can be tested with inly a few detections, as well as investing in more automated detector characterization; continued focused development for fast waveforms across parameter space; extending prompt online analysis to use simple precession waveforms; and developing quantitative methods for moving from the observed sample to the underlying source properties (modeling of pipeline selection).

If detections are from binary systems with unlikely parameters, we advocate extending search parameter space in the unlikely region, investigating different parameter priors for parameter estimation studies and exploring implications for formation of binary systems in general.

If a transient is found not to be from a binary system, but has an EM counterpart, we recommend improving detector sensitivity at the appropriate frequencies to increase the event rate; developing faster and more robust data analysis techniques, and increasing the ability to find coincident transients in the EM spectrum.

If a continuous periodic gravitational wave signal is found, we recommend coherent analysis to provide source sky position to arc-second precision to allow the EM follow-up. If pulsations are not observed, EM follow-up will require deeper imaging for fainter counterparts. We advocate better modeling of neutron star deformation now in advance of detections.

If a stochastic background of gravitational waves is detected, we recommend follow-up analyses to measure the energy spectrum of the background over as large frequency range as possible, to measure the potential spatial anisotropy in the background, and to measure the potential polarization of the background.

5 Multi-Messenger Astronomy

Edited by Daniel Holz and Peter Shawhan

While the primary focus of LIGO and other gravitational-wave detectors is on detecting and fully characterizing GW signals, especially in this era anticipating the first detections, there are exciting possibilities for connecting GW data with other astronomical observations. "Multi-messenger" astronomy can include electromagnetic (EM) observations, *i.e.* photons over a wide range of energies, and neutrinos. Gravitational-wave sources with potentially detectable EM and/or neutrino signatures are summarized in Appendix B.

GW and EM/neutrino measurements of the same source are naturally complementary. By combining them, we can extend the range of the GW detectors, improve measurements of the GW parameters, learn about the inner engine of dramatic astrophysical events (such as gamma ray bursts and supernovae), test models for production of the heavy elements found in the universe, and potentially make precision cosmological measurements of the Hubble constant. This, however, requires some degree of coordination and direct or indirect data sharing between different groups of observers. The myriad of opportunities can be grouped into two broad categories as discussed below, and there are recommendations both for the GW science community and for the EM astronomy community.

5.1 Externally triggered GW searches

Because GW data is sampled at kHz frequencies and archived continuously, it is possible to carry out a "deep" search later on for a GW signal guided by a reported astrophysical event. Limiting the search to the known time and sky position of the event (within uncertainties and possible offsets from the source model) reduces the space of potential false alarms, permitting greater sensitivity [21]. The LSC and Virgo have published many such searches, triggered by gamma-ray bursts, magnetars, and pulsar timing glitches, often with EM observers as co-authors. (See, for example, [22, 23, 24, 25].) Joint searches have also been carried out for GW signals coincident with high-energy neutrinos that were potentially from astrophysical sources (for example, [26]). Other targets currently being pursued include supernovae and fast radio bursts.

We recommend that the LSC, Virgo and KAGRA continue to carry out externally triggered GW searches, working with EM and neutrino astronomers as appropriate to optimize the analyses. We also recommend community support for continued wide-area sky coverage by gamma-ray burst survey missions, which currently is provided mainly by Fermi and Swift. SVOM is scheduled for launch in 2021; it may be necessary to supplement that, *e.g.* with a number of small, inexpensive "CubeSats" with hard X-ray detectors.

5.2 Follow-up of GW events

The externally-triggered searches described above are well established as elements of the GW science program. Much of the discussion of the workshop focused on the more technically challenging approach to multi-messenger astronomy, in which candidate GW events are rapidly identified and communicated to EM observers to enable follow-up searches for EM counterparts. LIGO and Virgo carried out a first EM follow-up program during the S6/VSR2+3 science runs [27, 28] and are currently preparing for an expanded program with the advanced detectors [29].

Sky localization is a key challenge for EM follow-up. The localization of the first GW detections will likely be poor ($> 100\,\mathrm{deg}^2$), especially if the GW network consists of only two detectors. Identifying an EM counterpart is therefore not a trivial exercise. Successful identification will generally require a global network of wide field, deep observatories capable of delivering Target of Opportunity (ToO) observations on a short ($\sim$ minutes–hours) timescale. Even with sufficient sky area coverage and observing depth (from aperture and exposure time), there remains the challenge of identifying the EM transient (if any) that is the true counterpart to the GW event, in the ever-present sea of other EM transients. This will require additional observations for classification.

GW candidates can cause telescopes to be re-pointed, or can simply be correlated with ongoing EM transient surveys. The capabilities of currently available instruments and the degree of this identification challenge vary depending on the observing band. Relevant facilities that will be operational in the coming years include optical wide-field surveys (*e.g.* Zwicky Transient Facility [ZTF], Pan-STARRS, Catalina Real-time Transient Survey [CRTS], Dark Energy Survey [DES], Hyper Suprime-Cam [HSC], Visible and Infrared Survey Telescope for Astronomy [VISTA], VLT Survey Telescope [VST], plus LSST farther in the future) and radio surveys with antenna arrays (*e.g.* VLA Sky Survey [VLASS], Long Wavelength Array [LWA], Murchison Widefield Array [MWA], LOFAR, Apertif, ASKAP, Meerkat), to name a few. Larger telescopes with spectrographic capabilities will be critical for the characterizations of the EM candidates that the wide field telescopes will see.

We recommend continued collaboration with observers using these and other facilities around the world, through rapid and numerous ToO observations where feasible.

We recommend the development and usage of new instruments and observing programs that will be especially valuable for finding counterparts. Based on our current understanding of likely EM emissions from binary merger events, top priorities (not provided by the surveys mentioned above) are:

- Wide-field infrared imaging on the ground and/or in orbit to search for the much-anticipated kilonova signature. Current instruments (such as WFCAM on the ground and the Spitzer Space Telescope) have rather limited fields of view that can be improved upon, for instance by the future WFIRST-AFTA mission.
- Wide-field soft X-ray imaging (in orbit) to search for X-ray afterglows in the relatively low-background X-ray sky. Proposed missions using Lobster-eye imaging optics offer good sensitivity with a wide field of view.

We also encourage investment by the GW community in the following activities which would significantly enhance and strengthen the multi-messenger astronomy program:

- Development of optimal follow-up observing strategies (depth, tiling large areas, etc.)
- A validated, high-completeness, all-sky public galaxy catalog out to at least 200 Mpc. This catalog will be a valuable resource for small field-of-view telescopes that can point at a list of candidate host galaxies in the GW error region, but would not be able to tile large regions of sky. It may also be valuable for large-field-of-view telescopes to cull false positives and prioritize follow-up of the most promising counterparts.
- Development of algorithms for identifying counterparts to GW events in follow-up data: image subtraction pipelines, color cuts, etc.
- Characterizing the transient sky, and in particular, understanding the nature of potential false positives.
- A data center for GW alerts and EM follow-up information, with community-friendly public data sets (including low-latency data) and software, and user support. This would expand the scope of the current LIGO Open Science Center, which provides gravitational wave strain data.
- Work toward truly real-time GW data analysis (seconds instead of tens of minutes), opening the possibility to implement a GW-triggered "early warning system" that sends alerts to gamma-ray missions to observe an imminent binary neutron star merger.

- Continued support of numerical modeling: predictions from these simulations will guide follow-up strategies, but they are also important because once counterparts are found the models will allow us to infer the underlying physical processes.

The success of multi-messenger astronomy will depend crucially on the GW event rate. If GW signals are abundant (say, > 5 per year), improving the high-frequency sensitivity can improve localization and the prospects for determining the neutron star equation of state and testing general relativity. On the other hand, if GW signals are infrequent, the top priority should be to obtain more signals; for binary mergers, this calls for maximizing sensitivity at low frequencies. Note also that improving the weakest detector in the network can benefit sky localization, while improving the strongest detector in the network can benefit parameter estimation. We recommend that these options be evaluated more fully in various scenarios to prepare for making such choices when the first GW signals have been detected.

6 International Network

Edited by Fred Raab and David Shoemaker

While a pair of detectors can determine unambiguously that a gravitational wave event has been detected (the number one priority of Advanced LIGO), much of the astrophysical promise of gravitational-wave detection (particularly for transient sources) requires a network of detectors. Three detectors are needed to provide a position on the sky for sources, and can cover a broad cone around the normal to the triangle formed by their locations. However, to obtain all sky coverage and full polarization information, a fourth site is needed. Adding detectors beyond four increases the sensitivity of the network and gives more precise astrophysical information, and improves robustness of the network. Creating an integrated network is a necessary evolution of the field, and one which all parties should embrace through mutual agreements on data sharing, on collaborative work on the instrument science, and (eventually) on designing and building shared next-generation facilities.

The workshop included reports on the state of the different detectors currently under construction or in advanced planning. These are summarized in Appendix A. The Advanced LIGO detectors will begin observations in late 2015, with interleaved observations and detector commissioning to reach design sensitivity in approximately 2018. Advanced Virgo will join LIGO in a joint observation run in 2016 and will have a coordinated program of observing and commissioning. KAGRA reported that it would begin cryogenic operation in 2018, and assuming a similar commissioning schedule to Advanced LIGO and Advanced Virgo, it will reach design sensitivity in 2021. LIGO-India could begin observations in 2021, and might reach design sensitivity in 2022 or 2023.

6.1 Direction for the future development of the Network

The scenarios outlined in Chapter 4 suggest that the first detections will be made with a network consisting of two LIGO detectors and Virgo at somewhat lower sensitivity. Progress in achieving the science goals outlined in Chapters 4 and 5 and in Appendix B will depend critically on enlarging and improving the entire network. A level of international cooperation well beyond what has been achieved to date will be required to realize these goals given limited resources, both in terms of equipment and expertise.

We break the ways in which additional resources could aid in the development of the instruments into two categories which are related to both the epochs and the resources required.

Immediate actions, with relatively modest resources: Estimated funding for these activities is at the level of collaborative multiple investigator funding.

- Exchange programs between projects would allow partners to profit from each other's experience. Substantial visits (multi-month to year durations) by postdocs and graduate students would likely be the most valuable.

- International meetings with a focus on coordinated R&D and detector upgrade planning

- Implementing joint instrument science and engineering working groups, much as has been done for data analysis, to develop technical solutions which are common and share/distribute the effort globally.

- Most importantly (and enabled by the previous points), have a joint working group target one near-term instrument refinement, for example one of

  - the use of squeezed light,
  - control over parametric instabilities,
  - development of practical tiltmeters,
  - development of reduced thermal noise optical coatings, or
  - Newtonian Noise subtraction,

  and start a joint engineering effort. This would require engaging some engineering and project scientist staff and starting to procure key elements.

From these discussions, **we recommend:**

Near-term actions, significant resources: These activities will require significant funding, at the level of mid-scale innovation projects; and a further extension of the international coordination.

- Assemble a joint design effort for improvements to the Virgo and LIGO instruments, and if appropriate, KAGRA as well, which together make a noticeable (e.g., a factor of 2 in reach, and thus a factor of 8 in rate) improvement in the sensitivity of the instruments to one or more classes of events. This would likely be the sum of the innovations mentioned in chapter 3. Addressing this with all partners will allow costs for the development and production to be distributed and will advance the field among all partners in a synchronized manner.

- Once the field is established with a number of gravitational-wave events, planning should begin for a network with significantly enhanced capabilities (e.g., a factor of 10 in reach, or 1000 in rate). These plans must be based on substantial research and development based on developing new ideas, such as cryogenics, alternative laser wavelengths, different test mass substrate materials, new kinds of mirror coatings, and new interferometer topologies. It will also require significant engineering to consider tradeoffs on the length of the instruments, whether they are to be on the earth's surface or underground, and the assurance that the observatories are designed to accommodate a range of possible concepts over the lifetime of the observatories.

6.2 Management and Funding arrangements for the Long-term Global Network

Closer cooperation will become vital as we move forward, because of the scale of future facilities and the fact that the scientific return from any single detector will depend on the existence and capabilities of other instruments. In addition, the facilities needed will exceed the willingness of any one country to carry through, requiring joint funding of individual facilities. Thus, this next phase should be pursued as a global endeavor and will require formation of a strategy that makes the most of the resources that will ultimately be required to build more powerful observatories.

From these discussions, **we recommend:**

- The current collaborations, and other interested national research groups, should establish milestones for planning of the future detectors and manage the activities to achieve them. The goal will be to maximize the scientific return, minimize the possibility of unpleasant surprises, and to give assurance to ourselves and to the various funding agencies that they are getting the best value for their investment.
- It would be very helpful to see closer links among the global funding agencies, to start to coordinate medium- and long-term planning, and to look for synergy between the agency capabilities to most effectively stimulate the field.

7 Gravitational Wave Science in the broader US context

Edited By Beverly Berger

7.1 Motivation for this session

Although, over the years, the US has invested more than a billion dollars in gravitational wave science, this emerging "big science" area is less visible in the context of the National Research Council (NRC) and other federal advisory structures than more established disciplines. In fact, ground-based gravitational wave detection is the only "big science" area in the US without an advisory structure reporting to funding agencies. This section addresses options for creation of such a structure as well as arguments for and against each option.

7.2 Background

In the US, ground-based gravitational wave detection is funded exclusively by the NSF Physics Division while other areas of gravitational wave detection such as pulsar timing, space-based detectors, and CMB polarization have had their primary support in the NSF Astronomy Division, NASA, or both. NSF's Gravitational Physics Program is also unique internationally. Most other countries fund gravitational wave science through particle astrophysics. When such agencies meet, it is important that the US delegation include program staff in gravitational physics.

It should be noted that appropriate advisory structures existed in the early days of LIGO [30] and were essential for starting the project. The Physics Division then had its own advisory committee and could form and charge a subcommittee with LIGO-related issues. The timeframe of the late 1980s also coincided with the 1990 Physics decadal survey where a subpanel for gravitation, cosmology, and cosmic rays could assess the priority for LIGO and "enthusiastically" endorse it [31].

The Physics Division advisory committee no longer exists although the current Mathematical and Physical Sciences Advisory Committee (MPSAC) plays a similar role on the broader scale of the NSF's MPS Directorate. Advisory committees, set up by and reporting to one or more federal agencies, are governed by the Federal Advisory Committee Act (FACA) which makes it very difficult to set up new committees. An existing FACA committee, the High Energy Physics Advisory Panel (HEPAP[1]) reports jointly to DoE and NSF. HEPAP may also generate subcommittees to focus on specific issues. For example, every few years it convenes a Particle Physics Project Prioritization Panel (P5) to set priorities for experiments and then to advocate in Congress and the agencies for funding for the highest priorities. In 2009, HEPAP set up the Particle Astrophysics Scientific Assessment Group (PASAG) to assess discovery opportunities in this subfield.

A different approach to an advisory structure is provided by the Board on Physics and Astronomy (BPA) under the NRC. The BPA can empower standing committees (with funding from the appropriate agencies). One example is the Committee on Atomic, Molecular, and Optical Science (CAMOS).[2] CAMOS organizes the AMO decadal surveys, provides stewardship of the agenda of the most recent survey, engages in dialog with federal agencies on AMO science and related topics, initiates case studies on important and timely topics, and provides a venue for discussion among AMO scientists to unify the field.

Decadal surveys are organized by the NRC with agency funding. In astronomy, they prioritize proposed ground-based and space-based major instrumentation and are taken very seriously by the astronomical community and the funding agencies. All proposed space-based gravitational wave detectors are considered by these surveys. In contrast, decadal surveys in physics tend to prioritize projects only within given subfields and appear to carry less weight with the funding agencies but may, in particular cases, have strategic value as with initial LIGO. The NRC also organizes studies of special topics such as "Connecting Quarks with the Cosmos" [32], released in 2003, that eventually led to increased funding for research in this area.

7.3 Issues

A major question then is whether or not gravitational wave science needs an advisory structure along the lines of those previously mentioned. Strongly in favor of such a structure

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[1] See `http://science.energy.gov/hep/hepap`

[2] See `http://sites.nationalacademies.org/bpa/BPA_048649`.

is the certainty that, after the first discoveries, issues on what to do next may require broadly based, wise input from stakeholders in the field. The emergence of a major new scientific opportunity, gravitational wave astronomy, will require thoughtful deliberation as to how the field should be developed, where in the funding structure it should be in the longer run, and how coordinated planning between physics and astronomy should be developed so that both gravitational wave detection capabilities and appropriate astronomy capabilities are developed to allow the promise of this new field to be realized.

A structured entity could bring thoughtful deliberation to issues like priorities in the next generation of gravitational wave detector research including when and how to upgrade Advanced LIGO and when and how to develop a new facility. On the other hand, an advisory group does not necessarily provide *wise* advice or no one may pay attention even if the advice is wise. An appropriate advisory structure would be necessary if a costly new facility for ground-based gravitational wave science is to be evaluated and given credible support and visibility to funding agencies and to the broader community.

Accepting the need for an advisory structure, should it be standing or *ad hoc*? A standing committee, either FACA or BPA, is always available to address urgent issues. Perhaps more importantly, a well functioning standing advisory structure could develop credibility within the field to encourage community acceptance of difficult decisions. *Ad hoc* studies can respond as needed to urgent, and perhaps unanticipated, issues if it is determined that advice will not be needed on a continual basis. Finally, decadal surveys may come at the wrong time for ground-based gravitational wave science. It should be noted that FACA committees are more dependent on their agencies and tend to focus on tactical issues while BPA committees act more independently and are more concerned with long-term strategy.

Independent of the details of the advising body, who should be on it? From the perspective of this workshop, scientists working on, developing, or using ground-based gravitational wave detectors should play a major role. Should scientists working on space-based or pulsar timing gravitational wave detection also be included? Astronomers and physicists interested in multi-messenger detections should also play a role as well as scientists in technical areas of potential relevance for future upgrades or new facilities. Finally, distinguished scientists from areas outside gravitational wave science could bring an independent perspective to the discussion.

7.4 Discussion summary, and recommendation

There seemed to be a general consensus in favor of some sort of advisory panel with a slight preference for a BPA standing committee to organize an NRC study of the future of gravitational wave science. An alternative would be a subcommittee of an existing FACA committee such as HEPAP or MPSAC.

The upcoming 2020 Astronomy Decadal Survey was also discussed. The as yet unknown timescale for the first LIGO discoveries makes it difficult for the ground-based community to propose next generation projects with mature costing in time. In addition, the level of engagement of the astronomical community may be comparatively low if EM counterparts have not been found. However, agency response to decadal surveys has slipped in recent years from the following decade to the one after that and that ESA's L3 mission is planned for a 2034 launch so ground-based detector development may be relevant for 2020. As a realistic option, the ground-based community could repeat its role in the 2010 survey by submitting a number of white papers describing scientific potential but not requesting any particular future facility.

A related issue is the transition of ground-based gravitational wave detectors from physics experiments to astronomical observatories. The consensus was that such a transition would require frequent detections and stable detector operations, and that LIGO is many years from that state. When the transition does occur, LIGO and its successors would certainly have to participate in the astronomy decadal survey.

Timing is also key for setting up an advisory structure with tension between starting now to foster long lead time R&D and waiting for the window of opportunity after the first discoveries. It is possible that a triggering event such as actual rather than imminent discoveries would be a prerequisite to launching an NRC study. A definite prerequisite is a clear vision for the role of an advisory panel. What are the major questions it will be charged to answer? Finally, it is important to decide the focus of an advisory panel as discussed here—only ground-based gravitational wave science or a much broader purview. In any case, such a panel, whatever its focus, should have a broad membership.

Recommendation

We recommend that NSF (possibly with other agencies), with advice from the gravitational wave community as most broadly interpreted, explore the options for creating an advisory panel to include ground-based gravitational wave science. This advisory panel should, at the appropriate time, consider the opportunities and issues related to both fostering the emerging new field of gravitational wave astronomy, and advance planning for major new facilities to enable ground-based gravitational wave science in the early 2030s if warranted.

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A Appendix: Status of GW detectors

A.1 LIGO

LIGO has recently completed the installation of the Advanced LIGO detectors in the US observatories at Hanford, Washington and Livingston, Louisiana. As of May 2015, the two instruments both had a reach of better than 50 Mpc for 1.4-solar-mass binary neutron star inspiral events, bringing them to a sensitivity sufficient for the first Observing Run planned for the Fall of 2015. At the time of writing this report (June/July 2015), they had both reached ~ 60 Mpc, a factor of 3 improvement over the initial generation of LIGO detectors, after only about one year of commissioning work. Near-term efforts are focused on improving the operational availability of the detectors and of improving the stationarity of the noise background. The data analysis pipelines, detector characterization, and operations activities all appear to be on target for this first run. The design sensitivity of the instruments is approximately 200 Mpc for NS-NS inspirals, and this sensitivity will be reached with several years of commissioning interleaved with observing.

A.2 Virgo

The Virgo collaboration, started by France (CNRS) and Italy (INFN) and now with partners in the Netherlands (NIKHEF), and Poland, and Hungary, is in the process of installing the Advanced Virgo instrument in the EGO Observatory in Cascina, Italy. The 3km-long vacuum infrastructure and the technical buildings are reused from initial Virgo. This design employs the very successful seismic isolation from Initial Virgo with a redesigned optical system. The schedule calls for the instrument to be finished early in 2016, and ready to join ready to participate in a joint LIGO-Virgo Observing run ‘O2’ in Fall 2016. The Advanced Virgo instrument will come on line in a staged fashion: initially, the configuration will be a power-recycled Michelson with the objective to reach an interesting astrophysical sensitivity as quickly as possible, and then later completing its design configuration by incorporating the signal recycling mirror. This will enable the full design sensitivity of 140 Mpc to be achieved.

LIGO and Virgo have a joint Observing plan, laid out in “Prospects for Localization of Gravitational Wave Transients by the Advanced LIGO and Advanced Virgo Observatories” [1]. The European community of the GW groups, which includes all the Virgo and GEO groups, has started already to discuss on the far future of the field beyond the first detection era. The outcome of this effort is synthesized in the Einstein Telescope [3] design study, which was supported by the FP7 program of the European community.

A.3 KAGRA

KAGRA is a Japanese detector project funded by MEXT, with a number of unique features: it is constructed underground, offering a seismically and newtonian-gravity quiet environment, and will use cryogenics to reduce the thermal noise that limit all of the detectors currently envisioned. The major infrastructure is in place. The 3km-long tunnels and experimental halls are complete, and the vacuum system is being installed in Spring

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[3] `https://tds.ego-gw.it/itf/tds/index.php?callContent=2&callCode=8709`

2015. Extensive testing has been performed on first articles of cryocoolers, isolators, and other equipment, and a first phase of interferometry over the 3km baseline is in preparation. KAGRA plans to be ready to join the observing network toward the end of this decade, and the ultimate sensitivity is planned to be similar to that of Advanced LIGO once fully commissioned.

A.4 LIGO India

LIGO India is a joint US-India effort to place one of the NSF-funded Advanced LIGO instruments, originally designated as a second instrument at Hanford, in an infrastructure created by India. For the network it has the significant advantage of being 'out of the plane' of the other detectors discussed above, and provides an extremely valuable node in the network. As of May 2015, it is waiting for in-principle approval of the Project by the Cabinet of the Government of India. The scientific and technical teams are preparing the ground work in the interim. The LIGO designs for vacuum systems have been translated to modern design tools and are nearing readiness for bid, and the search for sites has identified several very promising possibilities. The schedule currently calls for a start on the infrastructure as soon as funds are allocated, and a start of installation late this decade. The goal for this instrument, with sensitivity identical to the US Advanced LIGO sites, is to join in observation in 2022 or 2023.

B Appendix: GW sources and multi-messenger astronomy

B.1 Sources of Gravitational Waves

Astrophysical sources of gravitational radiation are interesting not only for what they tell about the population of stellar remnants, or the dynamics of supernovae explosions, but also for the extreme physical environment they provide. It is inconceivable that any laboratory on Earth will ever reproduce the physics which dominates the dynamics of neutron stars and black holes, making these objects incredible naturally occurring laboratories for physical investigation. The gravitational radiation emitted by these sources will allow us to probe the nuclear physics of ultra-dense matter, and the most startling aspects of general relativity.

We classify sources of gravitational waves using the nature of the waveforms: those generated from binary systems near coalescence (with very good models of their inspiral phase); un-modeled transients generated by cataclysmic astrophysical events like supernova explosions; continuous periodic waves generated by rotating stars; and stochastic backgrounds of cosmological or astrophysical background.

B.1.1 Coalescence of Compact Binary Systems

The most likely source type to be detected first with the Advanced LIGO detectors are coalescences of binary systems of compact stars (neutron stars or black holes). Due to this, often the power spectral density of noise in a detector is translated into a range for binary neutron star (BNS) systems, averaged over sky and orbit orientation. The Initial LIGO detectors achieved a ~ 20 Mpc range in 2010; the Advanced LIGO detectors have already achieved ~ 60 Mpc at the time of writing this article. Estimated progression of LIGO and

Virgo detectors' sensitivity in the next several years is presented in [1], with the LIGO detectors expected to reach $\sim$ 100 Mpc range in the next two years, taking data jointly with the Virgo detector: observations of gravitational waves from binary systems are expected soon, although initial yearly rates are also expected to be low.

Although estimates for the coalescence rate vary largely, it is generally expected that "confident detections will likely require at least 2 detectors operating with BNS sensitive ranges of at least 100 Mpc" [1]. Current estimates of coalescence rates from population synthesis studies predict a rate of 1/yr for 100 Mpc BNS range [33, 34]; estimates assuming sources of short gamma-ray bursts (GRBs) are coalescences of binary systems with opening beam angles $3^o - 8^o$ predict 0.4-4/yr observations of these progenitors with detectors with 100 Mpc BNS range [35]. The rate of detections will increase as LIGO approaches its design sensitivity, with range of about 200 Mpc; note that the detectable volume goes as the cube of the range.

This predicted detection rate is based on the assumption on transients found matching a known template with a false alarm rate of 1/100 yrs or better. If the background of Advanced LIGO and Virgo detectors is about the same as initial LIGO, this false alarm rate is achieved for coalescence of neutron stars or low mass black holes, with algorithms using thousands to tens of thousands of templates. A detector reach for intermediate mass binary black holes is much larger than for binary neutron star systems, although the shorter waveforms in band make the background also higher. Some parameters, for some systems, will be well estimated (like chirp mass), others not so well: even for loud BNS signals with SNR of 20, individual masses will have 10% uncertainties, but distance and inclination cannot be resolved separately.

Advanced LIGO may detect a few coalescences of binary neutron star systems with a large enough signal to noise ratio to place significant constraints on the neutron star equation of state. Increasing the sensitivity at higher frequencies (400 Hz to 4 kHz) will improve our ability to resolve tidal effects in neutron stars, as well as observe the coalescence of two neutron stars into a black hole.

Gravitational waves from binary black hole systems (BBH) provide a unique probe of gravity in the strong field regime. In particular, the final stages of BBH inspiral, merger and ring-down, are expected to involve strongly non-linear parts of General Relativity and can provide great insight into the nature of gravity.

For "stellar mass" black holes in binary systems with a total mass near 20 solar masses, the frequency range of interest is 100 Hz - 1 kHz. Black holes of the necessary mass are known to exist, though binary systems have yet to be observed and the detection rate for Advanced LIGO is not well constrained.

For larger black holes, known as "intermediate mass black holes" or IMBH, in the 100 - 1000 solar mass region, detection frequencies of interest are correspondingly lower (10 - 100 Hz). IMBHs are of great interest in astrophysics because of their potential role in the formation of galaxies and supermassive BHs. They have not, however, been observed, and their existence remains an open question.

B.1.2 Un-modeled transients

The detection of transients from other than binary systems cannot use matched filtering, and are typically identified in detector data as excess power in the time-frequency domain. In the absence of detailed GW waveforms, some astrophysical guidance (including EM follow-up) is valuable (may help confirm astrophysical nature of a detection). Given their potential to impact the field, events like the next galactic supernova (SN), the next 980425-like gamma-ray burst (which was nearby, at a distance of ~ 40 Mpc), or the next soft-gamma-repeater (SGR) giant flare, should remain high priorities even after the first few inspiral detections. LIGO detectors in 2016-17 with ~ 100 Mpc BNS range could detect emission of $10^{-2}\mathrm{M}_\odot$ at 70 Mpc. However, energy emission for basic core-collapse SN is $10^{-7}\mathrm{M}_\odot$, so the range to these sources is likely galactic or near-galactic. Some extreme models of core-collapse SN with rotational instabilities or core/disk fragmentation could reach $10^{-2}\mathrm{M}_\odot$.

With Advanced LIGO having reached its design sensitivity, a few years of data will likely not constrain core-collapse SN models for long GRBs (where extreme models predict a rate of 1/20 yrs or smaller), but will produce interesting upper limits for progenitors of low-luminosity GRBs, expected at a rate of 0.25-0.5/yr at 100 Mpc. If a core-collapse produces an unstable. magnetar instead of a black hole, there could be a low luminosity GRB when the magnetar collapses to for a black hole later, which is a model for the plateau observed in the electromagnetic emission of 60% of long GRBs. The GW emission would be a longer duration transient.

B.1.3 Continuous waves from rotating stars

Rotating neutron stars with a non-axisymmetric deformation (a mass, or mass current, quadrupole) will emit GWs. It is estimated that there are more than 160,000 “normal” and 40,000 millisecond active pulsars in the Galaxy [36]. More than 2,500 pulsars have been observed, with 10% of those possibly emitting gravitational waves in the frequency band of Advanced GW detectors.

The emission strength is often characterized by quadrupole ellipticity ϵ, but this is uncertain within a range from $10^{-12} \leq \epsilon \leq 10^{-5}$, maybe extending higher for hybrid/quark stars.

Theoretical maximum sustainable ellipticities provide upper limits, but whether these are realized in nature is unknown/unlikely. Methods of producing/sustaining ellipticities (mountains locked-in to crust following formation, strong internal magnetic fields) are also highly uncertain.

If a continuous, periodic GW signal is detected, comparing its frequency to the rotational frequency of the neutron star (determined from its EM periodicity) would give us much information on the star: if $f_{GW} = 2f_{rot}$, we know the star is probably a triaxial ellipsoid; if $f_{GW} \sim 2f_{rot}$, it will show us that the components producing EM and GW emission are not completely coupled, and possibly give us information on crust and core coupling of star; if $f_{GW} \sim f_{rot}$, we'll learn that the precession plays an important role in emission; and if $f_{GW} \sim (4/3)f_{rot}$, we'll infer that the emission from r-modes is favored, which will give us also information on the interior fluid motion of the star.

If ellipticities are at the very high range we could potentially narrow down the neutron star equation of state. Multiple sources could yield information on ellipticity distributions to help constrain deformation models. If we have several detections, we can tell whether there are different distributions for “normal” and millisecond pulsars. However, from the amplitude of the gravitational wave signal we actually measure $h \propto I_{zz}\epsilon/d$, where I_{zz} is the moment of inertia and d is the distance, so measuring ϵ requires this additional information.

B.1.4 Stochastic background

There exists a stochastic background of gravitational waves, produced by incoherent superposition of sources, both astrophysical and cosmological. Searching for this background requires more two or more detectors, to measure cross-correlation of their data. The search can assume the background is isotropic, or measure an isotropic background using radiometer techniques and spherical harmonic decomposition.

It is useful to characterize the the spectrum with the energy density at 100 Hz, and assume a power law: $\Omega_{GW}(f) = \Omega_\alpha (f/100\text{Hz})^\alpha$. For $\alpha = 0$, initial LIGO set upper limits of $\Omega_0 \sim 10^{-6}$, Advanced LIGO design sensitivity will likely be sensitive to $\Omega \sim 10^{-9}$.

Detection of a cosmological background from the early Universe is unlikely, given the predicted amplitude of cosmological spectrum from standard inflation. However, if a background is detected, the polarization could be measured too, constraining parity violation in the early Universe. A background of cosmic super-strings will be explored, with Advanced detectors being able to explore a large region of the parameter space.

The astrophysical background of far low mass binary systems could be detected with Advanced LIGO detectors [37]. Searching for a background produced by black-hole ringdowns could probe extremely efficient core-collapse mechanisms ($10^{-4} - 10^{-2}\text{M}_\odot$ energy emission in GWs).

A radiometer search for a point source is complementary to the search for continuous wave sources; a search using spherical harmonic decomposition is more appropriate for extended sources such as the Milky Way.

B.2 Multi-Messenger Astronomy

One of the most exciting aspects of the dawning age of gravitational wave astrophysics is the potential for gravitational wave multi-messenger astronomy: the detection of sources in both the gravitational wave (GW) and the electromagnetic (EM) spectrum [38, 39, 40, 41, 42].

Detecting the non-gravitational wave output of astrophysical events (e.g., electromagnetic radiation or neutrinos) will lead to a more complete understanding of the physical processes involved. The most promising sort of multi-messenger detection involves a targeted electromagnetic search in a variety of bands (e.g., radio, optical, gamma-ray, etc.), many of which are only practical if the source of gravitational wave emission is localized on the sky.

A rule of thumb for the sky localization ability of a network of detectors is that the size of the error box is largely determined by the sensitivity of the 3^{rd} best detector in the network, and that mid to high-frequency sensitivity (100 Hz - 1 kHz) has the greatest impact on sky localization.

We list below some of the possible electromagnetic counterparts of gravitational wave events which are sources for ground based interferometric detectors.

B.2.1 Short gamma-ray bursts

Perhaps the most promising sources for multi-messenger astronomy are short/hard gamma-ray bursts (GRB). There is growing evidence that short GRBs result from coalescence of binary systems of compact stars [43, 44, 45].

The detection of these bursts simultaneously in both GWs and γ-rays will definitively establish compact binary coalescence as the engine driving these events. The GW detection will provide a window into the very inner engine of these dramatic, and highly energetic, electromagnetic events. Measurements of the masses, spins, and inclinations of the progenitors of short GRBs will provide crucial insight.

Multi-messenger studies of these sources may also provide additional insight into short GRB progenitors, for example identifying whether binary neutron stars or neutron stars merging with black holes are responsible (or perhaps even binary black holes with associated accretion disks?!). However, only a small fraction of GW events are expected to have GRB counterparts because the gamma-ray emission is beamed. But once some are found, event rates derived from GW and EM measurements will also constrain the beaming angle of the GRB events, informing us about the total energy involved in the EM bursts.

Detection may also provide information about the nature of host galaxies of these binary systems (and hence inform us about the binary formation mechanisms), provide constraints on the natal kicks associated with neutron star formation, and perhaps even elucidate the nature of common-envelope evolution.

It is also conceivable that by measuring timing differences we will learn about the neutron star equation-of-state.

GW observations of binary coalescences allows for an absolute determination of the luminosity distance to a binary [46, 47]. If the redshift to the source can be independently determined, the source can be used as a "standard siren", the GW analog of a standard candle, to put a point on the luminosity distance-redshift curve. In this way LIGO detections of binary systems may allow precision determination of the Hubble constant. This method relies on independent determinations of the redshifts of the sources.

B.2.2 Kilonovae

Binary mergers of neutron stars, or or neutron stars with black holes, may also produce X-ray, radio, optical, and/or infrared afterglows, in particular kilonovae powered by the radioactive decay of r-process elements ejected during the merger [48, 49, 50, 51]). The radioactive decay of these elements leads to a distinct lightcurve, predicted to peak in the near-infrared over a timescale of about a week. Such kilonovae may play an important role in the production of heavy elements in the Universe. The emission is roughly isotropic, making this a potentially very valuable signature to associate with a GW event candidate.

At the typical distances of detectable binary mergers, kilonovae are very faint [52] and thus difficult to observe on their own, but they may be found as follow-up from GW triggers. (However, it is unlikely that the first few detection will be found with electromagnetic counterparts without great effort, since the localization will be poor; and even for signals with an SNR as high as 20, the median area of a 95% confidence limit region is 10 deg^2.) By observing kilonovae we would constrain the amount of ejecta for each event. Coupling the EM and GW observations, and deriving from those the binary merger rate, we will be able to estimate the role of this process in heavy element production in our Universe.

For high SNR events, it may be possible to shed light on the NS equation-of-state (EOS). For example, tidal deformation of the star perturbs the phase of the GWs. Careful monitoring of the phase may therefore constrain models for the EOS. Other clues may come from the peak frequency of the post-merger remnant and the total amount of mass ejected in kilonovae events (which can be inferred from the kilonova luminosity and timing).

B.2.3 Blue flash

Some models of neutron star binary merger result in the ejection of neutron rich material during the merger [53, 54]. This material subsequently beta decays, leading to a bright blue flash on the hour timescale. Rapid follow-up of GW transients may allow for the identification and characterization of these transients.

C Appendix: Workshop participants who endorse this report

The following workshop participants asked to have their names listed to show their support for the summary and recommendations presented in this document:

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