Science Drivers: We propose a geocentric laser interferometer for the observation of both massive and stellar-mass black-hole binaries. Such an interferometer, which would possess armlengths $\mathcal{O}(10)$–$\mathcal{O}(100)$ shorter than the nominal design for the evolving Laser Interferometer Space Antenna (eLISA) (constrained on the low end by Earth's atmosphere and on the high end by orbital stability and phasemeter requirements), would have a sensitive band that is optimal for observing the most common black-hole mergers in the Universe - between black-hole binaries formed from the mergers of spiral and dwarf galaxies, and between intermediate-mass black-hole binaries formed in globular clusters. In addition, an observatory of this scale would also have an optimal sensitive band for detecting the last few years preceding merger of stellar-mass binaries containing black holes and/or neutron stars (see Figure 1). Observing such systems in advance of their mergers would make it possible to precisely localize the eventual mergers in both space and time, thereby making it possible to observe electromagnetic counterparts to these gravitational-wave signals. Even without observing the mergers, which occur at higher frequencies better explored by ground-based detectors, such counterpart observations would allow us to constrain dark energy by providing several hundred paired redshift-luminosity distance measurements. In addition, it is likely that ground-based observatories would still be in operation during the lifetime of the proposed mission, given the success of Advanced LIGO and the existing plans for third, fourth, and fifth generation detectors over the next two decades. Therefore, by combining the observation of the mergers of compact binaries with the simultaneous observation of electromagnetic emission (which would be made possible by the advanced warning from the space-based detector), we could precisely probe dark energy, measure the neutron star equation of state in fine detail (thereby informing our understanding of particle physics far above nuclear densities), and explore the gaseous and/or electromagnetic environments of merging black holes, to name only a few of the revolutionary possibilities of such a mission.

As was demonstrated during the 2011–2012 Gravitational Wave Mission Concept Study, the most compelling scientific rationale for short geocentric orbits was that it would result in dramatically improved measurement capabilities, despite having a lower signal-to-noise ratio for more massive systems. The typical level of improvement for a realistic astrophysical population would be **2–3 orders of magnitude in every parameter** relative to eLISA, due primarily to the higher frequency of the orbit-induced amplitude modulations for observed signals, which directly improves source localization dramatically, and also allows sidebands to occur at more optimal frequencies for measuring other source parameters. This tremendous improvement in the level of precision measurement would greatly aid in our understanding of astrophysical populations, and would also allow us to localize sources to a **single host galaxy**, rather than an error voxel containing many thousands of galaxies, which would help substantially with identifying electromagnetic counterparts.

Technical Considerations: The proposed interferometer would consist of either three or four satellites in geocentric orbit, depending on the specific design choices adopted, particularly the orbital configuration. The observatory would possess 2 – 4 equal arms measuring 20000 – 100000 km on a side, with the smallest option (hereafter option A) requiring either an increase in the number of arms and satellites or else a decrease in the number of functional arms relative to eLISA due to orbital requirements (essentially, the Earth itself would be in the lasers' path otherwise). The advantage of this design choice would be the availability of sun-synchronous orbits, which would yield greater thermal stability (constant face to the Sun, potential to minimize variations of Earth-shine) and would avoid exposing the telescope to direct sunlight. However, if a solar filter or a retractable shutter for the telescope can be developed and implemented, and if detailed thermal modeling can demonstrate that the performance impact of the resulting thermal variations can be mitigated, then a "traditional" equilateral constellation near or beyond geostationary orbit could be employed (hereafter option B). Communications are simplified in either case, and the possibility of a servicing mission in the event of a single satellite failure is viable. The satellites are identical, and the thrust requirements for a 120 degree phase change for 2 satellites are minimal. The thrusters can supply the necessary thrust, so no propulsion module would be needed for the proposed design.

A perceived disadvantage for option A is the potential for gravity gradient noise to become nonnegligible for the orbits we are considering. However, we note that the relative frequency between the satellite position and the Earth's rotation is well below the sensitive band; since gravity gradient noise falls off rapidly above this frequency, it would only be a concern for lower Earth orbits than we are considering.

A perceived disadvantage for option B is the presumed need for station-keeping, since the Sun and Moon provide torques out of the constellation's orbital plane, which would cause a relative drift among the sciencecrafts. However, this drift is at the level of $\Delta v \sim 45$ m/s each year, and is primarily directed out of the plane of measurement, which implies a Doppler shift well below 45 MHz per year for a micrometer wavelength laser. Conservatively estimating the Doppler shift at 25 MHz, phasemeter sampling at 50 MHz would therefore allow operation for at least two years without station keeping. Such a sampling rate is within the capabilities of available phasemeters planned for the nominal eLISA design. The mission lifetime would therefore be limited by the phasemeter capabilities in that scenario, although it is more likely that cost would determine the mission lifetime, with the lifetime determining the phasemeter requirements.

New Technologies: The technology required for the proposed design will be similar to the developmental requirements for eLISA. The nominal design requirement for the acceleration noise achieved by the Disturbance Reduction System (DRS) will already be demonstrated by LISA Pathfinder. The optical path components will be a larger scale version (larger telescope, higher laser power, etc.) of the technologies that will be demonstrated by the Grace Follow-On mission. One technology with more stringent requirements than eLISA would be the phasemeter, although the nominal phasemeter planned for eLISA could facilitate a mission lifetime of at least 2 years without station keeping. A detailed thermal study would be required

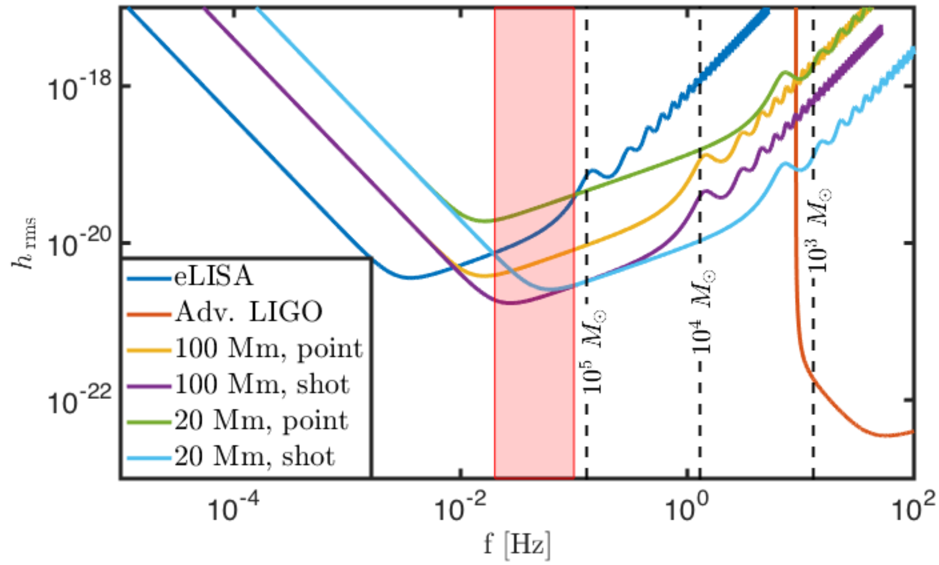

Figure 1: The strain sensitivities of eLISA, Advanced LIGO, and several design options for the proposed mission. The smaller armed design would execute sun-synchronous orbits with greater thermal and orbital stability, but would require either one less arm or an additional satellite relative to eLISA in order to perform its measurement. The longer armed design would require a sun filter or a shutter and would possess poorer thermal stability, but it could operate with the traditional equilateral LISA-like configuration. For each scenario, we show a case where pointing noise is suppressed below shot noise, and another where it can only be suppressed to the pm/$\sqrt{\rm Hz}$ level. We also mark the typical merger frequencies for binaries with total system masses of 10^3, 10^4, and $10^5\,M_\odot$, to show that this loudest, most dynamical, most information-rich part of the signal would be more sensitively measured by the proposed concepts than by eLISA. We emphasize that these three masses represent the range of expected masses for the sources that a space-based detector might actually observe. We also show a shaded region, which indicates the frequency range at which stellar-mass binaries, from 10–100 $M_\odot$, whose mergers could be seen by a ground-based detector, would emit at the start of their final year prior to merger.

to determine whether option B possesses sufficient thermal stability. Option B will likely require a solar shield to mitigate thermal variability, whereas option A *c*ould require some shielding from Earth-shine (although preliminary estimates suggest this might not be necessary). If option B proves thermally viable, then development of an adequate solar filter would be necessary. The necessary filtering capabilities have been demonstrated for the Omega mission concept, but additional development would be required to space qualify an adequate filter.

Probe-Class Justification: While we emphasize that the design options described here are motivated by optimally targeting the described sources, which occur at frequencies significantly above the most sensitive frequencies of eLISA, we nonetheless conclude that every monetarily significant design variation relative to eLISA serves to decrease the overall mission cost. By virtue of its smaller armlength and orbital characteristics, the proposed observatory will have significantly reduced requirements for the telescope mirror size, laser power, and propulsion (no propulsion module would be needed). Due to the substantial reduction in overall mass and orbital Δv, we expect a significant reduction in launch cost; preliminary estimates suggest that the lower cost Falcon 9 (Block 2) launch vehicle would be sufficient, but we note that the geocentric orbits admit the possibility of shared launches and/or having multiple smaller launches to further reduce cost. Though motivated by the science drivers, the design changes nonetheless serve to decrease the overall budget from L-class to probe-class.

Cost: The lighter mass and smaller Δv orbit will be the principle areas of savings relative to the nominal eLISA design. We can follow the costing approach of subtracting cost from the \$1.8B SGO high price point in the 2011–2012 Mission Concept Study (based on LISA with a less expensive launch vehicle). The Falcon 9 (Block 2) from SpaceX would be the most cost effective single launch vehicle capable of supporting the launch mass, for a cost savings of \$300M relative to SGO high, with additional savings of ∼\$100M possible if we can arrange multiple shared launches. A reduced 2 year mission lifetime would provide a further savings of \$200M in personnel cost, although this additional savings directly impacts the achievement of the science goals. The proposed design would not require a propulsion module, since the thrusters planned for SGO or eLISA would be capable of executing the necessary maneuvers; this would provide an additional savings of \$100M. The final cost estimate, performed in this fashion, is therefore \$1.1B – \$1.2B. However, we emphasize that if we instead adopt the Grace Follow-On mission as our baseline, then starting from \$450M, and adding costs from the Mission Concept Study for the DRS (\$50M), increased mass/power and their impact on launch requirements (\$100M), upgraded laser and optics (\$150M), and phasemeter (\$80M), we find a total cost of \$830M. Therefore, it is our expectation that a large portion of the design-option parameter space will fit within the probe-class cost window.